

**WHY ARE SOME COUNTRIES RICHER THAN OTHERS? A
SKEPTICAL VIEW OF MANKIW–ROMER–WEIL'S TEST OF
THE NEOCLASSICAL GROWTH MODEL**

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Fitted Cobb–Douglas functions are homogeneous, generally of degree close to unity and with a labor exponent of about the right magnitude. These findings, however, cannot be taken as strong evidence for the classical theory, for the identical results can readily be produced by mistakenly fitting a Cobb–Douglas function to data that were in fact generated by a linear accounting identity (value of goods equals labor cost plus capital cost).

(Herbert Simon, 1979, p. 497)

I have always found the high R^2 reassuring when I teach the Solow growth model. Surely, a low R^2 in this regression would have shaken my faith that this model has much to teach us about international differences in income.

(Gregory Mankiw, 1997, p. 104)

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ABSTRACT

This paper provides evidence of a problem with the influential testing and assessment of Solow’s (1956) growth model proposed by Mankiw *et al.* (1992). It is shown that when the assumption of a common rate of technical progress is relaxed in the neoclassical model, the goodness of fit of Mankiw *et al.*’s equation improves dramatically. However, and more importantly, it is shown that this result, as well as the magnitude of estimates obtained, merely reflects a statistical artifact. This has serious implications for the possibility of actually testing Solow’s growth model.

1. INTRODUCTION

In a seminal paper, Mankiw *et al.* (1992) (hereafter MRW) revived the canonical Solow (1956) growth model, which had come under increasing challenge from the development of the new endogenous growth models. This became the first effort in what Klenow and Rodriguez-Clare (1997) have referred to as a ‘neoclassical revival’. In MRW’s words: ‘This paper takes Robert Solow seriously’ (MRW, 1992, p. 407).¹ By this, MRW meant that Solow’s growth model had been misinterpreted in the literature since the 1980s. MRW showed how the model should be specified and its predictions tested, and they emphasized that it predicted conditional, rather than absolute, convergence. Solow’s model continues to be the starting point for almost all analyses of growth (and macroeconomic theories of development), and even models that depart significantly from Solow’s model are often best understood through comparison with this model.

MRW concluded that Solow’s model accounted for more than half of the cross-country variation in income per capita, except in one of the subsamples, namely that of the OECD economies. MRW claimed that ‘saving and population growth affect income in the directions that Solow predicted. Moreover, more than half of the cross-country variation in income per capita can be explained by these two variables alone’ (MRW, 1992, p. 407). They continued: ‘Overall, the findings reported in this paper cast doubt on the recent trend among economists to dismiss the Solow growth model in favor

¹ However, Solow has indicated, in reference to the international cross-section regressions program initiated in the early 1990s, the following: ‘I had better admit that I do not find this a confidence-inspiring project. It seems altogether too vulnerable to bias from omitted variables, to reverse causation, and above all to the recurrent suspicion that the experiences of very different national economies are not to be explained as if they represented different “points” on some well-defined surface. . . . I am thinking especially of Mankiw, Romer and Weil (1992) and Islam (1992)’ Solow (1994, p. 51). Islam (1992) was finally published as Islam (1995). Solow (2001) indicates that he thought of ‘growth theory as the search for a dynamic model that could explain the evolution of one economy over time’ (Solow, 2001, p. 283).

of endogenous-growth models that assume constant or increasing returns to capital' (MRW, 1992, p. 409). Their results showed that each factor receives its social return, and that there are no externalities to the accumulation of physical capital.

In this paper we discuss a problem with the way that MRW, and the subsequent papers evaluating the latter, have tested the predictions of Solow's growth model. This is that there is the income accounting identity that relates output (value added) to the sum of the total wage bill plus total profits, which, as we shall show, can be easily rewritten as a form that closely resembles MRW's specification of Solow's growth model, *even though no well-defined aggregate production function exists*. The problems posed by the accounting identity were noted by, *inter alios*, Herbert Simon (1979) in his Nobel Prize lecture and Shaikh (1980). (Felipe and McCombie (2002) offer a detailed discussion of the various issues involved). We further show that MRW's regression is a particular case of this identity, subject to two empirically implausible assumptions. These are that differences in the level of technology, resource endowments and institutions can be modeled as a constant plus a random error term, and that each country has the same rate of technical progress. The argument in this paper explains why the coefficients in the estimated equation must take a given value and sign, irrespective of whether the neoclassical assumptions hold, and why, if Solow's augmented growth model were specified correctly, it should yield a very high statistical fit, potentially with an R^2 equal to unity.

The rest of the paper is structured as follows. In section 2, MRW's model is discussed. In section 3 we relax MRW's assumption of a constant growth rate of technology across countries by including the level and growth of technology in each country. We estimate the model for the OECD countries and show that the fit improves dramatically. The magnitudes and signs of the parameters are as expected. Section 4 provides an explanation for these results. This argument, however, raises a number of important questions as it demonstrates that the testing of Solow's growth model proposed by MRW may be viewed as essentially a tautology. Section 5 discusses the other important theme in MRW's paper, namely the possibility of conditional convergence. It is shown that, if Solow's model is estimated allowing for differences in technology across countries, it yields the implausible result that the speed of convergence is infinite. We show why this result must occur as a consequence of the underlying identity. Section 6 concludes.

2. SOLOW’S GROWTH MODEL AND THE MANKIW–ROMER–WEIL SPECIFICATION

The elaboration of Solow’s growth model by MRW is well known and so it needs only to be briefly rehearsed here. They started from the standard aggregate Cobb–Douglas production function with constant returns to scale:²

$$Y(t) = [A(t)L(t)]^{1-\alpha} K(t)^\alpha \quad (1)$$

where Y is output, L is the labor input, K is the capital stock and $(1 - \alpha)$ and α are labor’s and capital’s output elasticities ($0 < \alpha < 1$). A is a measure of the level of technology. They assumed constant exponential growth rates for labor and technology, n , i.e. $L(t) = L_0 e^{nt}$ and g , i.e. $A(t) = A_0 e^{gt}$, respectively. Consequently, the number of effective units of labor $A(t)L(t)$ grows at rate $(n + g)$. MRW also assumed, following Solow (1956), that a constant fraction of output, s , is saved over time (although this fraction differs across countries), and depreciation is a constant fraction of the capital stock, namely δK . With these assumptions, it is straightforward to derive the steady-state value of the capital per effective unit of labor ratio (K/AL), which upon substitution into the production function yields the steady-state productivity:

$$\ln y = \ln A_0 + gt + \frac{\alpha}{1-\alpha} \ln s - \frac{\alpha}{1-\alpha} \ln(n + g + \delta) \quad (2)$$

where y denotes labor productivity (Y/L). The model predicts that countries with higher savings/investment rates will tend to be richer (in per capita *levels*). These countries accumulate more capital per worker, and consequently have more output per worker; and countries that have high population growth rates will tend to be poorer. And the model also predicts the magnitudes of the coefficients of these two variables. But savings rates and population growth do not affect the steady-state *growth rates* of per capita output.

At this point, MRW introduced a couple of crucial assumptions. First, they assumed $(g + \delta)$ to be constant across countries (neither variable is country-specific) and set it equal to 0.05. Secondly, they postulated that the term A_0 reflects not just the initial level of technology, but resource

² This is a simplification (justified by the fact that shares are roughly constant), as the neoclassical growth model does not necessarily require an aggregate Cobb–Douglas production function. However, our arguments in this paper apply equally to any putative aggregate production function.

endowments, climate, institutions and so on. On this basis, they argued that it may differ across countries, and assumed that $\ln A_0 = b_0 + \varepsilon$, where b_0 is a constant, and ε is a country-specific shock. Furthermore, they made the identifying assumption that the shock is independent of the saving and population growth rates.

Therefore, the previous equation, using cross-country data, becomes:

$$\ln y = b_0 + \frac{\alpha}{1-\alpha} \ln s - \frac{\alpha}{1-\alpha} \ln(n+0.05) + \varepsilon \quad (3)$$

where b_0 is a constant.

In this context, Islam (1999) commented that 'The problem [. . .] lies in the estimation of A_0 . It is difficult to find any particular variable that can effectively proxy for it. It is for this reason that many researchers wanted to ignore the presence of the A_0 term . . . and relegated it to the disturbance term. This, however, creates an omitted variable bias for the regression results' (Islam, 1999, p. 11). This assumes that the variable being proxied by the constant is correlated with the regressors.

Equation (3) provides the framework for testing Solow's model as a joint hypothesis since it specifies the signs and magnitudes of the coefficients (together with the identifying assumption). Assuming that countries are at their steady-state growth rates, this equation can be used to test how differing saving rates and labor force growth rates can explain the differences in current productivity across countries. This is the essential point of the MRW paper. The argument is that for purposes of explaining cross-country variations in income levels, economists could return to the old framework and the assumption that the term A_0 is the same across countries. This contrasts with other attempts at understanding differences in income per capita, in particular the one advocated by Jorgenson (1995), in whose view the assumption of identical technologies across countries implicit in the neoclassical growth model may not hold. Prescott (1998) has also noted that savings rate differences are not that important; what matters is total factor productivity (TFP) growth, which leads him to conclude that a theory of TFP is needed. Parente and Prescott (1994) argue that the development miracle of South Korea is the result of reductions in technology adoption barriers, while the absence of such a miracle in the Philippines is because there were no such reductions.

Mankiw (1995, p. 281), however, defended the assumption that different countries use roughly the same production function. He argues that the objection that developing and developed countries share a common production function is not as preposterous as some writers have indicated, and is

not a compelling one. In his view this assumption only means that if different countries had the same inputs, they would produce the same output.

On the basis of the identifying assumption, equation (3) was estimated by OLS with data for 1960–85 for three samples, the first one including 98 countries, the second one 75 countries, and the third one containing only the 22 OECD countries. MRW (1992, p. 411) acknowledged that the specification estimated could lead to inconsistent estimates, since s and n are potentially endogenous and influenced by the level of income.

As is well known, the results were mixed. Although the results for the first two samples were quite acceptable, with an \bar{R}^2 of 0.59 and an implied elasticity of capital $\alpha = 0.6$, the results for the OECD countries were rather poor, with the estimate of the coefficient of $\ln(n + 0.05)$ insignificant (although with the correct negative sign) and a very low \bar{R}^2 , namely 0.01 ($\bar{R}^2 = 0.06$ in the regression with the coefficients of $\ln(s)$ and $\ln(n + 0.05)$ restricted to take on the same value).

These results led MRW to propose an augmented Solow model in which they included human capital. The model improved the explanatory power of all three samples, but still the \bar{R}^2 for the OECD countries was a disappointing 0.24 (0.28 in the restricted regression). The authors concluded, under the assumption that technology is the same in all countries, that exogenous differences in saving and education cause the observed differences in levels of income. The production function consistent with their results is $Y = AK^{1/3}H^{1/3}L^{1/3}$, where H denotes human capital. In this formulation the elasticity of physical capital is not different from its share in income. There are also no externalities to the accumulation of physical capital (that are found in the endogenous growth literature).

A number of papers subsequently re-evaluated MRW's work. At the expense of over simplifying, discussions of MRW's original work have split into (i) those that propose further augmentations of the MRW regression, (ii) those that concentrate on the discussion of econometric issues, and (iii) those critical of the literature and who propose important methodological changes. The works of Knowles and Owen (1995), Nonneman and Vanhoudt (1996) fall into the first group, while those of Islam (1995, 1998), Durlauf and Johnson (1995), Temple (1998), Lee *et al.* (1997, 1998) and Maddala and Wu (2000) fall into the second. Durlauf (2000), Easterly and Levine (2001) and Brock and Durlauf (2001) are the third group. They are very critical of the growth literature and propose new research avenues. Quah (1993a, 1993b) also criticizes this literature. Using the concept of Galton's fallacy, he argues that this work does not shed any light on the question of whether poorer countries are catching up with the richer. A very interesting discussion on

Galton's fallacy and economic convergence is Bliss (1999) and the reply by Cannon and Duck (2000).

Knowles and Owen (1995) augmented the original MRW regression with health capital, and Nonneman and Vanhoudt (1996) with technological know-how. Both obtained better results, at least in terms of the fit of the model.

Since the hypothesis that all countries have identical production functions and differ only in the value of the variables of this function, but not in the parameters, appeared to be too restrictive, Islam (1995) relaxed the assumption of strict parametric homogeneity. Through the use of panel data, the aggregate production function was allowed to differ across countries with respect to the technology shift parameter. His panel estimates of the neo-classical model accommodated level effects for individual countries through heterogeneous intercepts in an attempt to indirectly control for variations in A_0 and even to estimate the different A_0 s. However, Islam retained the assumption that the rate of labor-augmenting technical progress plus depreciation of capital is the same across countries (5 percent per year).

Lee *et al.* (1997) extended this work to allow countries to differ in level effects, growth effects and speed of convergence. It was shown that there is indeed a great deal of dispersion in growth rates and speeds of convergence. From an econometric point of view, their concern is with the nature of the biases in the estimated coefficients when the data are pooled and there is heterogeneity in the parameters. They showed that in the pooled regression (as used by Islam, 1995) the estimates of these parameters are biased. Lee *et al.* (1997) derived a stochastic version of the Solow model where the heterogeneous parameters were modeled in terms of a random coefficients model and used exact maximum likelihood estimation.

Durlauf and Johnson (1995) used a classification algorithm known as regression tree in order to allow the data to identify multiple data regimes and divide the countries into groups, each of which obeys a common statistical model. They concluded that the results vary widely. Their results led them to conclude that: 'the explanatory power of the Solow growth model may be enhanced with a theory of aggregate production differences' (Durlauf and Johnson, 1995, p. 365). In the same vein, Temple (1998) used robust estimation methods. He argued that 'If MRW's model is a good one, it should be capable of explaining per capita income when the sample is restricted to developing countries and NICs, or to the OECD' (Temple, 1998, p. 365). However, when Portugal and Turkey were removed from the OECD sample, the fit of his regression fell from 0.35 to 0.02. He concluded: 'It appears that, when one concentrates on the most coherent part of the OECD, the augmented Solow model in this form has almost no explanatory power' (Temple,

1998, p. 366). When he split the sample in quartiles, although the regressions still had acceptable fits (the \bar{R}^2 is between 0.58 and 0.67), there was a lot of variation in the estimated parameters.

Maddala and Wu (2000) used an iterative Bayesian approach (shrinkage estimator) to also address the problem of heterogeneity discussed by Lee *et al.* (1997) in panel data. They claimed that their estimation method is superior to that of Lee *et al.* (1997) because the latter's method is not fully efficient in the presence of lagged dependent variables.

Easterly and Levine (2001) used a procedure similar to that of Islam in order to move away from the assumption that the level of technology is the same in all countries. These authors allowed the term A to differ by introducing regional dummies and refuted MRW's idea that technology levels are the same across countries. The interest of this paper is that the authors used a variety of other evidence (e.g. patterns of flows of people between countries) and went well beyond the regression exercise. They also assessed the relationship between policy and economic growth using a generalized method of moments dynamic panel estimator. They concluded that national policies such as education, openness to trade, inflation and government size, are strongly linked with economic growth.

Durlauf (2000) and Brock and Durlauf (2001) argued that current empirical practice in growth is not policy relevant. The statistical significance or insignificance of a coefficient cannot be taken to establish the importance (or unimportance) of a policy for growth. These authors advocate greater eclecticism in empirical work, including historical analyses and the use of a decision-theoretic formulation in order to compute predictive distributions for the consequences of policy outcomes. These distributions can then be combined with a policymaker's welfare function to assess alternative policy scenarios. To achieve this, they used Bayesian methods.

3. RELAXING THE ASSUMPTION OF A COMMON TECHNOLOGY ACROSS COUNTRIES

In this section a solution is proposed for improving upon the poor results obtained by MRW for the OECD countries. This consists of relaxing the assumption of a common rate of technical progress introduced by MRW. Attention is restricted to the OECD sample, which it will be recalled is the one that yielded the most disappointing results in MRW's paper.

The rate of technical progress may be determined, under the usual neo-classical assumptions, from the dual of the production function, and is likely to differ among countries. Consequently, these are calculated and included

in the regression. Contrary to Islam (1999), quoted above, standard neo-classical production theory suggests that this is a suitable proxy for technical progress. The dual rate of technical progress is given by

$$g_t = \frac{(1-\alpha)\hat{w}_t + \alpha\hat{r}_t}{(1-\alpha)} \quad (4)$$

which implies that $A(t)^{1-\alpha} = B_0 w(t)^{1-\alpha} r(t)^\alpha$, where α is capital's share in output, \hat{w}_t is the growth rate of the wage rate, and \hat{r}_t is the growth rate of the profit rate. This assumes perfect competition and that factors are paid their marginal products, so that $\alpha = a$, where a is capital's share in output (see Jones, 1998, p. 53; Hall and Jones, 1999).

The MRW model (without human capital) becomes:

$$\ln y = c + 1.0 \ln w + \frac{\alpha}{1-\alpha} \ln r + \frac{\alpha}{1-\alpha} \ln s - \frac{\alpha}{1-\alpha} \ln \left[n + 0.02 + \frac{(1-\alpha)\hat{w} + \alpha\hat{r}}{1-\alpha} \right] + \varepsilon \quad (5)$$

where:

- y is real GDP per person of working age in 1985;
- s is the investment–output ratio (average for 1960–85);
- n is the average rate of growth of the working-age population (average 1960–85);
- w is the average of the wage rates in 1963 and 1985;
- r is the average of the profit rates in 1963 and 1985 (total profits divided by the capital stock);
- \hat{w} is the exponential annual growth rate of the wage rate for 1963–85;
- \hat{r} is the exponential annual growth of the profit rate for 1963–85 and;
- δ is the rate of depreciation and equals 0.02.
- In constructing $[(1-\alpha)\hat{w} + \alpha\hat{r}]/(1-\alpha)$ we use the average factor shares for 1963–85 as the weights $(1-\alpha)$ and α .

The estimation results are summarized in tables 1 and 2. The estimates shown in the tables follow the order of the variables in equation (5), i.e., γ_1 for $\ln w$, γ_2 for $\ln r$, γ_3 for $\ln s$, etc. The other coefficients are the restricted coefficients. Table 1 shows the results of MRW's model, namely equation (3) above, which assumes a common rate of technical progress across the sample, and where $(g + \delta) = 0.05$.

These results are consistent with those of MRW and thus will not be discussed further. Table 2 shows the second set of results, namely from the estimation of equation (5).

Table 1. OLS estimates of MRW’s specification of Solow’s model for the OECD countries. Equation (3)

Constant	$(\gamma_3) \ln s$	$(\gamma_4) \ln(n + 0.05)$	\bar{R}^2 ; <i>s.e.r.</i>
8.776 (3.51)	0.586 (1.36)	-0.605 (-0.71)	0.025; 0.375
Implied a from $\hat{\gamma}_3$ is 0.369 (2.16)			
Implied a from $-\hat{\gamma}_4$ is 0.377 (1.15)			
$H_0 : \hat{\gamma}_3 + \hat{\gamma}_4 = 0 : \chi^2_1 = 0.00$			
Restricted regression imposing $H_0 : \gamma_3 + \gamma_4 = 0$			
Constant	$(\gamma_5) \ln s - \ln(n + 0.05)$		\bar{R}^2 ; <i>s.e.r.</i>
8.82 (16.71)	0.591 (1.63)		0.073; 0.364
Implied a from $\hat{\gamma}_5$ is 0.371 (2.59)			

t-statistics in parentheses; *s.e.r.* is the standard error of the regression; α is capital’s output elasticity, taken to be the capital share (a).
 The implied capital share a is obtained as $\alpha = |\hat{\gamma}_i / (1 + \hat{\gamma}_i)|$, where $\hat{\gamma}_i$ is the estimated coefficient.
 Critical value of χ^2_1 (0.05) = 3.84.

The results in table 2 show a substantial improvement in the goodness of fit. Solow’s growth model *does* seem to work for the OECD countries, contrary to MRW’s findings. It is notable that the estimate of $\ln w$ is statistically not different from unity ($\chi^2_1 = 0.01$; the critical value for a significance level of 0.05 is 3.84), and that we can also recover the capital share from the estimate of $\ln r$ using the same transformation a from $\ln s$. Denoting this as γ_2 , $\alpha = \hat{\gamma}_2 / (1 - \hat{\gamma}_2)$. This implies a capital share of 0.454 (with a t-statistic of 5.13). We may similarly obtain estimates of a from the coefficient of $\ln s$ and the negative of the coefficient of $\ln(n + 0.02 + g)$. In fact, the null hypothesis that all three coefficients of $\ln r$, $\ln s$ and $\ln(n + 0.02 + g)$ are equal (the last one with the opposite sign) cannot be rejected ($\chi^2_2 = 0.28$; critical value for a significance level of 0.05 is 5.99).

The second regression imposes the restriction that the coefficients of $\ln s$ and $\ln(n + 0.02 + g)$ are the same. And the last regression imposes on the previous regression the restriction that the parameters of $\ln r$, $\ln s$ and $\ln(n + 0.02 + g)$ are the same. In all three cases results are very similar and confirm that the model is satisfactory in terms of accounting for the differences in per capita income across the OECD countries. The fit is over 80 percent.

At first sight it might seem that Solow’s growth model in its steady-state form is the most satisfactory explanation of ‘why some countries are richer

Table 2. OLS estimates of Solow's model augmented with differences in technology for the OECD countries. Equation (5)

$(\gamma_1) \ln w$	$(\gamma_2) \ln r$	$(\gamma_3) \ln s$	(γ_6) $\ln(n + 0.02 + g)$	$\bar{R}^2; s.e.r.$
1.001 (12.52)	0.833 (2.80)	0.794 (3.02)	-0.673 (-4.78)	0.832; 0.155
Implied a from $\hat{\gamma}_2$ is 0.454 (5.13)				
Implied a from $\hat{\gamma}_3$ is 0.422 (5.42)				
Implied a from $-\hat{\gamma}_6$ is 0.402 (7.99)				
$H_0 : \hat{\gamma}_3 + \hat{\gamma}_6 = 0 : \chi^2_1 = 0.26$				
Restricted regression imposing $H_0 : \gamma_3 + \gamma_6 = 0$				
$(\gamma_1) \ln w$	$(\gamma_2) \ln r$	(γ_7) $\ln s - \ln(n + 0.02 + g)$		$\bar{R}^2; s.e.r.$
0.971 (26.98)	0.719 (3.77)	0.681 (4.95)		0.838; 0.152
Implied a from $\hat{\gamma}_2$ is 0.418 (6.47)				
Implied a from $\hat{\gamma}_7$ is 0.405 (8.32)				
$H_0 : \hat{\gamma}_2 - \hat{\gamma}_7 = 0 : \chi^2_1 = 0.03$				
Restricted regression imposing $H_0 : \gamma_2 - \gamma_7 = 0$				
$(\gamma_1) \ln w$		(γ_8) $\ln r + \ln s - \ln(n + 0.02 + g)$		$\bar{R}^2; s.e.r.$
0.965 (206.71)		0.693 (6.10)		0.846; 0.148
Implied a from $\hat{\gamma}_8$ is 0.409 (10.33)				

See table 1 notes.

than others’. It could be further argued that these results strongly justify MRW’s faith in Solow’s model. Countries are rich (poor) because they have high (low) investment rates, low (high) population growth rates, and high (low) levels of technology. See Jones (1998, p. 53) for a similar view.

Paradoxically, these results are rather suspicious. This is because they are too good to be true given all the theoretical problems associated with the concept of aggregate production function (Harcourt, 1972; Fisher, 1993). Furthermore, it is surprising that only three variables (technology, employment and capital), notwithstanding their likely serious measurement problems, so comprehensively explain the variation in per capita income. And Srinivasan (1994, 1995) has argued the data in the Summers and Heston database, the one used by most authors (including MRW), are of very poor quality since most of the data for the developing countries are constructed by extrapolation and interpolation.

In the next section it is shown why the data must, indeed, always give a near perfect fit to the ‘model’. This raises serious problems for the previous interpretations of Solow’s model. In this sense, we believe our arguments go beyond those of Brock and Durlauf (2001) in their criticisms of the empirical growth literature. They confined their criticisms to the fact that it is difficult to know what variables to include in the analysis; the problem that the failure to refute a theory does not imply the falsity of another one; the unrealistic assumption of parameter homogeneity across countries; and the lack of attention to endogeneity problems.

4. TOO GOOD TO BE TRUE? THE TYRANNY OF THE ACCOUNTING IDENTITY

In this section it is shown that the results in the last section can be regarded as merely a statistical artifact. This is because the above results are totally determined by the national income accounting identity that relates value added to the sum of the wage bill plus total profits. The identity is given by:

$$Y_t \equiv W_t + \Pi_t \equiv w_t L_t + r_t K_t \quad (6)$$

where the symbol \equiv denotes that the expression is an identity, not a behavioral model, Y_t is real (constant price) value added, W_t is the total wage bill, Π_t denotes total profits (operating surplus in the National Accounts terminology), w_t is the real average wage rate and r_t is the *ex post* real average profit rate. This identity simply shows how total output is divided between wages and total profits (where the latter is the normal return to capital plus

economic profits). Therefore, equation (6) does not follow from Euler's theorem. The wage and profit rates need not be determined by the (aggregate) marginal products which, in the light of the aggregation literature, most likely do not even exist (Fisher, 1971a, 1971b). It is common, however, to argue that if capital and labor are paid their marginal products, constant returns to scale implies $Y = wL + rK = F(AL, K)$, where r is defined as $\partial F(AL, K)/\partial K$ and w as $\partial F(AL, K)/\partial L$ (Romer, 1996, p. 35).

This is misleading. In the words of Fisher: 'If aggregate capital does not exist, then of course one cannot believe in the marginal productivity of *aggregate capital*' (Fisher, 1971b, p. 405; italics in the original). The conditions to generate an index of aggregate labor are also extremely restrictive, so the same comment applies to the (aggregate) marginal product of labor. The marginal productivity conditions follow from Euler's theorem which, while correct as a mathematical proposition, conflicts with the aggregation problem in economics. If the aggregates K and L cannot be constructed because of the aggregation problems, then the aggregate production function $F(AL, K)$ does not exist, and it follows that $wL + rK = F(AL, K)$ has no meaning. Therefore, the notion of estimates of returns to scale at the aggregate level becomes problematic, to say the least. The identity (6) will nevertheless always hold.

Consequently, the aggregate production function itself is unlikely to be well defined or even to exist (Fisher, 1969, 1993). In this sense we strongly disagree with Romer (1996, p. 8) who claims that the critical assumption of the aggregate production function in Solow's model is that it has constant returns in capital and labor. The crucial assumption in the authors' opinion is that the aggregate production function exists. Felipe and Fisher (2003) summarize the most important results of the aggregation literature and discuss why economists continue using a tool without a sound theoretical underpinning.

The first assumption we make is the stylized fact that factor shares are roughly constant (i.e. $a_t = a$; $(1 - a_t) = (1 - a)$). The accounting identity in growth rates is given by:

$$\begin{aligned} \hat{Y}_t &\equiv (1-a)\hat{w}_t + a\hat{r}_t + (1-a)\hat{L}_t + a\hat{K}_t \\ &\equiv \phi_t + (1-a)\hat{L}_t + a\hat{K}_t \end{aligned} \quad (7)$$

where $\phi_t = (1-a)\hat{w}_t + a\hat{r}_t$, $(1-a) = (w_t L_t)/Y_t$ is labor's share, and $a = (r_t K_t)/Y_t$ is capital's share in value added. It will be noticed that the expression for ϕ_t coincides with what we called above the dual measurement of productivity growth, i.e. $g_t = \phi_t/(1-\alpha)$. However, if the aggregate production and cost functions do not exist (as opposed to the microeconomic relationships), the interpretation of ϕ_t as a measurement of technical progress becomes problematical.

The concept of total factor productivity (in both its primal and dual forms) at the aggregate level is linked to the notions of aggregate production and cost functions (Nadiri, 1970). Without the latter there is no reason why the so-called residual ϕ_t in the income accounting identity must be a measure of productivity growth. Notice, for example, that the weights (the factor shares) appear in this derivation without invoking the first-order conditions. The neoclassical interpretation, however, follows from the supposed link between the identity, the aggregate production function and Euler’s theorem.

It is sufficient to note that equation (7) follows directly as a transformation of the accounting identity, equation (6), without any behavioral assumptions (such as competitive markets). The only assumption is the constancy of the shares, which can occur for a number of reasons totally unrelated to the existence of an aggregate production function, such as firms pursuing a constant mark-up pricing policy.

Integrating equation (7) and taking antilogarithms gives:

$$Y_t \equiv B_0 w_t^{1-a} r_t^a L_t^{1-a} K_t^a \equiv B(t) L_t^{1-a} K_t^a \quad (8)$$

where

$$B(t) = B_0 w_t^{1-a} r_t^a \quad (9)$$

Equation (9) is referred to in the neoclassical literature as the *dual* measure of productivity. It can be called anything one wishes, but certainly the procedure adopted here (namely rewriting an identity) is very different from the standard derivation of the dual measure in neoclassical economics. The procedure followed here is correct, but tautological.

We argue that equation (8) is not a production function. It is simply the income identity, equation (6), rewritten under the assumption that factor shares are constant. Also note that the factor shares appear without invoking the marginal productivity conditions.

This implies that if the assumption of constant shares is correct in the data set in question, the regression $Y_t = B(t) L_t^{\eta_1} K_t^{\eta_2}$ must yield $\eta_1 = (1 - \alpha) = (1 - a)$ and $\eta_2 = \alpha = a$ and a perfect fit (cf. equation (8)). This assumption is, in practice, correct for most data sets.³ Therefore, why do researchers using time

³ There is not a great deal of variability in the size of the shares across either the group of advanced countries considered here or for these countries over the time period considered (this is also the neoclassical justification for the use of the Cobb–Douglas production function). However, the standard calculation of labor’s share as a ratio of employee compensation to GDP from the NIPA shows very low values for a number of developing countries. Gollin (2002) has

series data sometimes obtain ‘increasing returns to scale’? The answer is that $B(t)$ is incorrectly proxied, often through a linear time trend, i.e. $B(t) = B_0 \exp(\theta t)$. If this approximation is incorrect (as it most often is), it will induce a bias in the estimates of η_1 and η_2 (including even negative values: see Lucas, 1970; Tatom, 1980). But this does not undermine our argument. The correct proxy for $B(t)$ will yield the best possible regression and hence, will take us back to the identity.

The growth of the capital stock is defined as:

$$\frac{\Delta K_t}{K_t} \equiv \hat{K}_t \equiv \frac{I_t}{K_t} - \delta \equiv \frac{sY_t}{K_t} - \delta \quad (10)$$

where I is gross investment, δ is the constant rate of depreciation and s is the investment–output ratio.

It is assumed that the capital–output ratio does not change over time, so that $\hat{Y}_t = \hat{K}_t$. While this is a condition for steady-state growth in the neoclassical schema, it is also one of Kaldor’s (1961) stylized facts, unrelated to neoclassical theory. However, while this is the case for the data set used here, over a longer period of time there is evidence of a secular increase in the capital–output ratio (Maddison, 1995). This, together with constant factor shares, implies a fall in the rate of profit.

Using only (i) the accounting identity, (ii) the definition of the growth of the capital stock, (iii) the assumption that factor shares are constant, and (iv) the assumption that there is no growth in the capital–output ratio, the expression for labor productivity may be straightforwardly derived as:

$$\ln y = c + 1.0 \ln w + \frac{a}{1-a} \ln r + \frac{a}{1-a} \ln s - \frac{a}{1-a} \ln \left[n + \delta + \frac{(1-a)\hat{w} + a\hat{r}}{1-a} \right] \quad (11)$$

It should be noted that these assumptions also imply $\hat{r} = 0$.

The question that arises at this point is ‘how is equation (11) to be interpreted?’ This is the same question that we posed at the end of last section. It is obvious that equation (11) resembles equation (3) above, and that it is identical to equation (5). Equation (5) was derived from the Cobb–Douglas production function and could be considered to be a generalization of the MRW model, as it allowed for technical progress and technology to vary between countries.

shown that once the labor share is adjusted to reflect correctly the labor income of the self-employed sector, which appears in most National Accounts registered as profits, it increases to values that are consistent with those observed in the developed countries.

But, and here is the important point, equation (11) which is identical to equation (5) was derived without any recourse to neoclassical production theory. All that has been done is to transform the income accounting identity, equation (6), into another identity, under two assumptions, namely, constant factor shares and a constant growth of the capital–output ratio.

Recall our arguments above about equations (6) and (8): they are both identities. *What is important to note is that equation (6) and the two assumptions made are equally compatible with the absence of a well-behaved aggregate production function. There is no requirement that factors be paid their marginal products, and no assumptions need be made about the state of competition, or that growth is steady-state.*

Indeed, if the assumptions are roughly correct, econometric estimation of equation (11) must yield a perfect fit, and simply because of the underlying identity (and not for any other reason), we should expect the estimates of the profit rate, saving rate and that of the sum of the growth rate of the labor force plus depreciation plus ‘technical progress’ to give a ballpark figure for $a/(1-a)$ of 2/3 and for a of 0.4. The estimate of the coefficient of the logarithm of the wage rate should equal unity.

In fact, it turns out that matters are a little more complicated than this. The following terms of the right side of equation (11) may be expressed as:

$$c = -\frac{a}{1-a} \ln a - \ln(1-a) \quad (12a)$$

$$\frac{a}{1-a} \ln r = \frac{a}{1-a} \ln \left(a \frac{Y}{K} \right) \quad (12b)$$

$$\frac{a}{1-a} \ln s = \frac{a}{1-a} \ln \left(\hat{K} + \delta \right) \frac{K}{Y} \quad (12c)$$

$$\frac{a}{1-a} \ln \left(n + \delta + \frac{(1-a)\hat{w} + a\hat{r}}{1-a} \right) = \frac{a}{1-a} \ln \left(\hat{K} + \delta \right) \quad (12d)$$

Substituting these equations into equation (11) gives:

$$\ln y = -\ln(1-a) + 1.0 \ln w \quad (13)$$

This is analogously true for equation (5).

Equation (13) has been derived on the assumption that factor shares are constant and there is no growth in the capital–output ratio. The fact that equation (11) gives a good fit to our data is due to the fact that, ironically, there is enough variation in the factor shares and the growth of the

capital–output ratio to prevent perfect multicollinearity and to give reasonably precise estimates of the coefficients of all the terms.⁴ We return to this point below, but, for expositional purposes, generally confine our attention to equation (11) because of its close correspondence with MRW's estimating equation.

But can all this be interpreted to be a test, in the sense of providing verification (strictly speaking, non-refutation) of Solow's model? The answer is clearly 'no' because, as we have noted, the estimates are compatible with the assumption of no well-defined aggregate production function. Moreover, an R^2 of unity is suspicious. The argument implies that if factor shares are roughly constant and the capital–output ratio does not grow, equation (11) will always yield a high fit (with data for any sample of countries) and with the corresponding parameters. Moreover, equation (13) must also hold, by definition, solely if factor shares are constant as $wL/Y \equiv (1 - a)$. Thus, although we have used the assumption that there is no growth in the capital–output ratio to derive equation (11), equation (13) does not require this assumption.

Furthermore, if $g_t = \hat{w}_t + \frac{a\hat{r}_t}{(1-a)}$ and $A(t)^{1-a} = B_0 w(t)^{1-a} r(t)^a$ are constant across countries, then equation (11) becomes MRW's equation (3), and it will similarly give highly significant and plausible estimates.

These two assumptions used are quite general. The hypothesis of a constant capital–output ratio is one of Kaldor's (1961) stylized facts. It is a very general proposition. In fact, Kaldor would not have been pleased to discover that this stylized fact is interpreted in terms of an aggregate production function, a notion that for many years he heavily criticized. Suppose, for example, that oligopolistic firms adopt a constant mark-up pricing policy and set prices to achieve a certain target rate of return, which may vary between firms. If the average rate of return does not greatly vary over the period being considered, then the growth of the capital–output ratio will be roughly constant. This does not depend upon the economy being in steady-state in the neoclassical sense of the term.

⁴ Suppose we have an *almost exact* relationship between four undefined variables y , u , v and x , (i.e. there is a small error term) given by $y_i = b_1u_i + b_2v_i + b_3x_i$ and the following also holds: $b_2v_i + b_3x_i = c$. If we were to estimate the regression given by the first equation, there would be severe multicollinearity and imprecise estimates of the regression coefficients would result, which would have large standard errors. However, a precise estimate of b_1 would be obtained by regressing $y_i = c + b_1u_i$. On the other hand, as the goodness of fit deteriorates, it progressively becomes more likely that we can estimate the multivariate regression and obtain reasonably precise individual estimates of the three coefficients. This is the case that occurs here; it is a statistical problem and does not affect the theoretical argument.

Regarding the assumption of constant shares, another of Kaldor’s (1961) stylized facts, it could be asked whether it implies a Cobb–Douglas production function. It is standard to argue that the reason why factor shares appear to be more or less constant is that the underlying technology of the economy is Cobb–Douglas (Mankiw, 1995, p. 288). The answer, however, is that this is not necessarily the case.

In his seminal simulation work, Fisher (1971a) simulated a series of micro-economies with Cobb–Douglas production functions. He aggregated them deliberately violating the conditions for successful aggregation. He found, to his surprise, that when factor shares were constant the aggregate Cobb–Douglas worked very well. This led him to conclude that the (standard) view that constancy of the labor share is due to the presence of an aggregate Cobb–Douglas production function is erroneous. In fact, he concluded, the argument runs the other way around, that is, the aggregate Cobb–Douglas works well *because* labor’s share is roughly constant. Thus, what the argument says is that the Cobb–Douglas will work as long as factor shares are constant, even though the true underlying technology might be fixed coefficients. Note that in the neoclassical model, factor shares are constant in steady-state growth for any production function. Mankiw (1995, p. 288) indicates that factor shares may be roughly constant in the USA data merely because the US economy has not recently been far from its steady state.

Factor shares will be constant, for example, if firms follow a constant mark-up pricing policy with any underlying technology at the plant level. (See Lee, 1998 for a discussion of mark-up pricing). See also Nelson and Winter (1982), who create a non-neoclassical economy that leads to constant factor shares and where a Cobb–Douglas yields good results.

The fact that when the necessary assumptions are exactly fulfilled, equation (11) reduces to equation (13), even more graphically illustrates the argument. As we have noted, the fact that shares are constant over time and across countries does not, *per se*, imply that there is an underlying aggregate production function or that it is a Cobb–Douglas. Thus, while the neoclassical model under the assumption of constant factor shares (together with differences in the rate of technical progress and in the wage rate and possibly the profit rate) give rise to equations (11) and (13), the finding that the statistical estimates are close to their expected values cannot be taken as a test of the Solovian hypothesis.

The conclusion is that if the two assumptions used above are empirically correct, the national income accounts imply that an equation like (11) exists, and we will *always* find that there is a positive relationship between the savings rate and income per capita, and a negative relationship between

population growth and income per capita. Moreover, as we have noted, if shares are exactly constant, equation (13) will give a good statistical fit, even though the stylized fact of a constant growth in the capital–output ratio is not met.

One may also be tempted to argue that the problem is similar to that of observational equivalence, in this case between equations (13), (11) and (5) (or equation (3) if technology levels and growth rates are constant across countries). However, for this argument to be correct, one would have to deal with two *models* that have the same implications about observable phenomena under all circumstances. Here, however, we do not have two alternative theories that generate the same distribution of observations. There is Solow's theory, but the other explanation is just an identity. Therefore, this is not an identification problem in the strict sense. Placing *a priori* restrictions on Solow's model will never identify an identity. On the observational equivalence problem in macroeconomics, see Backhouse and Salanti (2000).

But the important question is whether this approach can in any way be interpreted as a test of Solow's model. The answer is, again, no. If the estimated coefficients are identical to those predicted by equation (11), it could be because the model satisfies all the Solovian assumptions, but the estimated coefficients are equally compatible with none of Solow's assumptions being valid. The data cannot discriminate between the two explanations and all one can say is that the assumptions of constant shares and a constant capital–output ratio have not been refuted.

The case which is perhaps more difficult to gauge is the one when there is not a perfect fit to the data, like in MRW (and virtually all applications). In fact, with data taken from the national accounts we will never obtain a perfect fit. The reason is simply that neither factor shares nor the capital–output ratio are exactly constant. Does this then imply a rejection of Solow's model? We suggest that it does not. All this means is that either factor shares or the capital–output ratio are not constant. The first can be taken under a neoclassical interpretation as a rejection that the underlying production function is a Cobb–Douglas one. However, we can always find a better approximation to the identity (and one which will resemble another production function) that allows factor shares to vary, and this could be (erroneously) interpreted as a production function, e.g. a translog 'production function' or a Box–Cox transformation. The second does refute the proposition that growth is in steady-state; but the results convey no more information than if a direct test of whether the capital–output ratio is constant were undertaken.

Moreover, given our arguments, the statistical estimation of equation (11) is not needed. One simply has to check whether or not the two assumptions

above are empirically correct. For most countries, the assumption that factor shares are constant is correct (see footnote 3). Factor shares vary very slowly and within a narrow range. This is true of our data set. Factor shares increased slightly in the 25-year period considered but display very little variation across countries in both initial and terminal years. So, it all comes down to confirming whether or not the capital–output ratio is constant. Here again we observe a similar pattern: capital–output ratios increased over time in all countries but the standard deviations in both initial and terminal years were small and identical in both periods. We conclude that, overall, equation (11) has to work well in terms of the goodness of fit and must yield estimates close to the hypothesized results. As we noted above, the variation in factor shares is not small enough for equation (13) to be preferred to equation (11).

A related important issue is that estimation of equation (11) does not require instrumental variable methods, as MRW (1992, p. 411) suggest, because the equation is fundamentally an identity. The error term here, if any, derives from an incorrect approximation to the income accounting identity. There is no endogeneity problem in the standard sense of the term. Certainly, the wage rate, the profit rate, employment and capital are endogenous variables, but nobody would argue that estimation of equation (6), an identity, requires instrumental variables, since there is no error term. If equation (11) is a perfect approximation to equation (6), the argument remains the same. It is true, however, that if equation (11) is not a perfect approximation to equation (6), the estimation method will matter. It may be possible that instrumental variable estimation, for example, could yield, under these circumstances, estimates closer to the theoretical values. But this is a minor issue once the whole argument is appreciated.

The implications of this argument are far reaching: it is not possible to test the predictions of Solow's growth model, as it is known *a priori* what the estimates will be. Equations (11) and (13) are little more than a tautology.

What is the result of further augmenting Solow's model in the sense of including additional variables, such as human capital? If the variables used in these regressions are statistically significant, it must be because they serve as a proxy for the weighted average of the wage and profit rates. Consequently, they reduce, to some extent, the degree of omitted variable bias. As noted above, Knowles and Owen (1995) and Nonneman and Vanhoudt (1996) extended the model by introducing health capital and the average annual ratio of gross domestic expenditure on research and development to nominal GDP, respectively. The correlations between the logarithm of this variable and the logarithms of wages and profit rates are 0.811 and -0.768 , respectively. It is not surprising that the addition of this variable to the MRW

specification improved the fit of the model as they found a 'good' proxy for $B(t)$ (see equation (9)), although the savings rate, the proxy for human capital and the growth rate of employment plus technology and depreciation, were statistically insignificant. This is because Nonneman and Vanhoudt (1996) used $\ln(n + 0.05)$, and thus ϕ_t was poorly approximated (this is also true of the modification of Knowles and Owen, 1995).

Islam (1995), on the other hand, used panel estimation and heterogeneous intercepts. The use of individual country dummies also helps to approximate better the identity. And finally, Temple (1998) correctly pointed out that the MRW specification lacks robustness. The problem, however, is not that the model is flawed because its goodness of fit varies substantially with the sample of countries. Even the specification given by equation (11), derived directly from the identity, may conceivably not give a close fit. It all depends on whether or not the assumptions used (*viz.* constant factor shares and a constant capital–output ratio), are approximately correct. It would be possible to find a sample of countries where these do not hold and thus there would be a poor fit to the identity. This would not, however, affect the theoretical argument concerning the problems posed by the underlying identity for the interpretation of the parameters of the model.

We close this section by quoting Solow (1994) in reference to this research program (see also footnote 1):

The temptation of wishful thinking hovers over the interpretation of these cross-section studies. It should be countered by cheerful skepticism. The introduction of a wide range of explanatory variables has the advantage of offering partial shelter from the bias due to omitted variables. But this protection is paid for. As the range of explanation broadens, it becomes harder and harder to believe in an underlying structural, reversible relation that amounts to more than a sly way of saying that Japan grew rapidly and the United Kingdom slowly during this period. (Solow, 1994, p. 51)

In a similar vein, Paul Romer (2001) also has strong reservations about this research program from a methodological point of view. In essence, Romer argues that what this program has done is to advocate a narrow methodology based on model testing and on using strong theoretical priors with a view to restricting attention to a very small subset of all possible models: '... show that one of the models from within this narrow set fits the data and, if possible, show that there are other models that do not. Having tested and rejected some models so that the exercise looks like it has some statistical power, accept the model that fits the data as a "good model"'

(Romer, 2001, p. 226). Romer is correct in his assessment that MRW never considered alternative models. For example, the finding of a negative coefficient for the initial income variable is interpreted, in the context of the neo-classical model, as evidence of diminishing returns to capital. But, as Romer argues, this finding could also be interpreted as implying that the technology of the country that starts at a lower level of development is lower and it grows faster as better technology diffuses there. Romer claims that MRW’s approach does not advance science and refers to it as a dead end.

5. THE CONVERGENCE REGRESSION AND THE SPEED OF CONVERGENCE

As indicated in section 2, the steady-state growth rates of per capita output in the standard Solow growth model are independent of the savings ratio and population growth rates. Therefore, the model does not provide explanations of the differences in long-run per capita growth. The model, however, has some important implications about transitional dynamics. This transition shows how an economy’s per capita income converges towards its own steady-state value, and in this way it provides an explanation for the observed differences in growth rates across countries. In simple terms, this explanation is that poor countries tend to grow faster than rich countries. The neoclassical growth model predicts that an economy that begins with a stock of capital per worker below its steady-state value will experience faster growth in per capita output along the transition path than a country that has already reached its steady-state per capita output.

It is necessary to consider the implications of the arguments in section 4 for the estimates of the speed of convergence given by the MRW specification. One of the main points MRW stressed in their paper was that Solow’s growth model predicts conditional, not absolute, convergence. The speed of convergence, denoted by λ , measures how quickly a deviation from the steady-state growth rate is corrected over time. In other words, it indicates the percentage of the deviation from steady state that is eliminated each year. When MRW tested for conditional convergence they found that indeed it occurs, but the rate implied by Solow’s model is much faster than the rate that the convergence regressions indicate. A number of studies, including MRW’s, have found evidence of conditional convergence at a rate of about 2 percent per year. That is, each country moves 2 percent closer to its own steady state each year (Mankiw, 1995, p. 285). This implies that the economy moves halfway to steady state in about 35 years. On the other hand, it can

be shown that the speed of convergence according to Solow's model equals $\lambda = (n + \delta + g)(1 - \alpha)$ (Barro and Sala-i-Martin, 1995, pp. 36–38; Mankiw, 1995, p. 285). Using the averages in our data set (we assume $\delta = 0.02$), λ equals $(0.01 + 0.02 + 0.021) \cdot 0.768$, or 3.91 percent per year, almost twice the rate that most studies estimate.⁵

The convergence regression is derived by taking an approximation around the steady state (Mankiw, 1995). Empirically, λ is estimated through a regression of the difference in income per capita between the final and initial periods on the same regressors as previously used (i.e. savings rate and the sum of the growth rate of employment, depreciation rate and technology), plus the level of income per capita in the initial period. The coefficient of the initial income variable (τ) is a function of the speed of convergence, namely, $\tau = -(1 - e^{-\lambda})$ (MRW, 1992, p. 423). In the neoclassical model, this equation is:

$$\begin{aligned} (\ln y_t - \ln y_0) = & gt + (1 - e^{-\lambda}) \ln A_0 + (1 - e^{-\lambda}) \frac{\alpha}{1 - \alpha} \ln s \\ & - (1 - e^{-\lambda}) \frac{\alpha}{1 - \alpha} \ln(n + 0.05) + \tau \ln y_0 + \varepsilon \end{aligned} \quad (14)$$

where y_t and y_0 are the levels of income per worker in 1985 and 1960, respectively, and the expression $gt + (1 - e^{-\lambda}) \ln A_0$ is assumed to be constant across countries. Here, t is the length of the period.

Estimation results of equation (14) are displayed in the upper part of table 3 (the first two regressions, where the coefficients are estimated unrestricted and restricted, respectively). The results are close to those of MRW (1992, table IV), with a very similar speed of convergence, slightly below 2 percent a year. The speed of convergence is derived from the last coefficient, that is, $\tau = -(1 - e^{-\lambda})$. Once λ is determined, the implied capital share is obtained from the other coefficients. Note that the traditional MRW is mis-specified to the extent that λ is a function of n , population growth (MRW, 1992, p. 422) which varies between countries. Hence λ also varies. In our reformulation λ also varies to the extent that g now varies. However, we merely follow the traditional approach here.

What do the arguments in Section 4 imply for the convergence regression and the speed of convergence? In terms of equation (11) above, this specification can be derived by subtracting the logarithm of income per capita in the initial period from both sides of the equation. This yields:

⁵ See also Jones (1997).

Table 3. Testing for conditional convergence

Convergence regression equation (14)					
<i>Constant</i>	$(\gamma_3) \ln s$	(γ_4) $\ln(n + 0.05)$	$(\gamma_5) \ln y_0$	$\bar{R}^2; s.e.r.$	
2.646 (2.40)	0.447 (2.75)	-0.649 (-2.04)	-0.352 (-5.86)	0.666; 0.141	
Implied a from $\hat{\gamma}_3$ is 0.559 (5.83)					
Implied a from $-\hat{\gamma}_4$ is 0.648 (5.46)					
$H_0 : \hat{\gamma}_2 + \hat{\gamma}_4 = 0 : \chi^2_1 = 0.30$					
Implied λ (from $\hat{\gamma}_9$) is 0.017 (4.67)					
Convergence regression equation (14) imposing $H_0 : \gamma_2 + \gamma_4 = 0$					
<i>Constant</i>	(γ_5) $\ln s - \ln(n + 0.05)$	$(\gamma_6) \ln y_0$		$\bar{R}^2; s.e.r.$	
3.164 (5.70)	0.493 (3.58)	-0.354 (-6.00)		0.678; 0.138	
Implied a from $\hat{\gamma}_5$ is 0.582 (7.58)					
Implied λ (from $\hat{\gamma}_9$) is 0.017 (4.78)					
Convergence regression equation (15)					
$(\gamma_1) \ln w$	$(\gamma_2) \ln r$	$(\gamma_3) \ln s$	(γ_4) $\ln(n + 0.02 + g)$	(γ_5) $\ln y_0$	$\bar{R}^2; s.e.r.$
1.121 (5.58)	0.814 (2.67)	0.828 (3.03)	-0.799 (-3.21)	-1.154 (-4.62)	0.580; 0.158
Implied a from $\hat{\gamma}_2$ is 0.449 (4.85)					
Implied a from $\hat{\gamma}_3$ is 0.453 (5.54)					
Implied a from $\hat{\gamma}_4$ is 0.444 (5.78)					
$H_0 : \tau = -1 : \chi^2_1 = 0.38; H_0 : \gamma_3 + \gamma_4 = 0 : \chi^2_1 = 0.01$					
Implied λ (from $\hat{\gamma}_9$) is ∞					

Table 3. (Continued)

Convergence regression equation (15) imposing $H_0 : \gamma_3 + \gamma_4 = 0$					
$(\gamma_1) \ln w$	$(\gamma_2) \ln r$	(γ_6) $\ln s - \ln(n + 0.02 + g)$	(γ_9) $\ln y_0$	$\bar{R}^2; s.e.r.$	
1.125 (5.85)	0.793 (3.72)	0.811 (3.82)	-1.167 (-5.65)	0.603; 0.153	
Implied a from $\hat{\gamma}_2$ is 0.442 (6.67)					
Implied a from $\hat{\gamma}_6$ is 0.448 (6.91)					
$H_0 : \tau = -1 : \chi^2_1 = 0.65; H_0 : \hat{\gamma}_2 - \hat{\gamma}_6 = 0 : \chi^2_1 = 0.01$					
Implied λ (from $\hat{\gamma}_9$) is ∞					
Restricted regression equation (15) imposing $H_0 : \gamma_2 - \gamma_6 = 0$					
$(\gamma_1) \ln w$	(γ_8) $\ln r + \ln s - \ln(n + 0.02 + g)$	$(\gamma_9) \ln y_0$	$\bar{R}^2; s.e.r.$		
1.123 (6.03)	0.802 (4.66)	-1.163 (-6.05)	0.624; 0.149		
Implied a from $\hat{\gamma}_8$ is 0.455 (8.40)					
$H_0 : \tau = -1 : \chi^2_1 = 0.72$					
Implied λ (from $\hat{\gamma}_9$) is ∞					
Convergence regression equation (15) imposing $(\delta + g) = 0.05$					
$(\gamma_1) \ln w$	$(\gamma_2) \ln r$	$(\gamma_3) \ln s$	(γ_4) $\ln(n + 0.05)$	(γ_9) $\ln y_0$	$\bar{R}^2; s.e.r.$
0.373 (2.63)	0.525 (1.99)	0.551 (2.67)	-0.881 (-4.78)	-0.492 (-4.12)	0.712; 0.131
Implied λ (from $\hat{\gamma}_9$) is 0.027 (2.88)					

See table 1 notes.

Initial year (y_0) is 1960.

$$\begin{aligned}
 (\ln y_t - \ln y_0) = & c + 1.0 \ln w + \frac{a}{1-a} \ln r + \frac{a}{1-a} \ln s \\
 & - \frac{a}{1-a} \ln \left[n + \delta + \frac{(1-a)\hat{w} + a\hat{r}}{1-a} \right] + \tau \ln y_0
 \end{aligned} \tag{15}$$

Equation (15) indicates that the parameter of $\ln y_0$ has to be $\tau = -1$ (i.e. the estimate obtained is minus unity). Our argument indicates that since equation (11) is essentially an identity with the assumptions of a constant

growth rate of the capital–output ratio, subtraction of $\ln y_0$ on both sides implies that the estimate of $\ln y_0$ will be minus one. The third, fourth and fifth regressions in table 3 report the OLS estimates of equation (15).

Equation (15) is also estimated with the restricted constant.

These results provide a very different picture of the convergence discussion. The findings for τ are as predicted, and the rest of the parameters continue being very well estimated in terms of size and sign (and the restrictions on the parameters are not rejected). Notice that the positive and negative coefficients of $\ln s$ and $\ln(n + 0.05)$, respectively are multiplied by $-\tau$ in equation (14) and they are not in equation (15). Because the estimate of τ must be minus one it does not affect the result.

If this equation were to be interpreted as being the neoclassical growth model, the results imply $\tau = -(1 - e^{-\lambda}) = -1$, or $\lambda = \infty$ (under the null hypothesis that $\tau = -1$). Equation (14) is based on the assumption of constant growth of the capital–output ratio. However, two points should be noted here. First, empirically, the growth of the capital–output ratio is not exactly constant in the data set—the statistical fit is not perfect. Second, under the neoclassical assumption, theoretically the estimate of λ should be a constant and equal to $\lambda = (n + \delta + g)(1 - \alpha)$ regardless of how near the economies are to their steady-state growth rate. If all the economies are growing at their steady-state growth rate, then the speed of convergence is not infinite but undefined as:

$$\hat{y}_t = g + (1 - \alpha)(\delta + n + g)(\ln y_t^* - \ln y_t) \quad (16)$$

where the superscript * denotes the steady-level of per capita income and in the steady-state $\ln y_t = \ln y_t^*$. But, as we have seen, with differences in ‘technical progress’ allowed for and a roughly constant growth in the capital–output ratio, the identity will always give this result. The only reason why the conventional estimates are greater than minus unity is the assumption imposed on the model of a rate of technical change and level of technology that do not vary between countries. It should be emphasized that if there is no well-behaved aggregate production function and all we are estimating is an identity, then there is no reason why τ should be a measure of the speed of convergence.

Islam (1995, equation (11)) argues that a better way to estimate the rate of convergence is through an equation that incorporates transitional behavior. He derives an equation with $\ln y_t$ on the left-hand side (as opposed to the difference between last and initial periods) and with $\ln y_0$ on the right-hand side (and the same for the other regressors, i.e. $\ln s$ and $\ln(n + 0.05)$). He acknowledges (Islam, 1995, p. 11) that his regression has the same omitted-variable bias problem as MRW’s equation, due to an improper specification

of A_0 . In this case, and from the point of view of the accounting identity, the estimate of the coefficient $\ln y_0$ has to be zero, leading to the same conclusions about the speed of convergence as with the MRW regression. As Islam's approximation to the identity is substantially worse than that provided by equation (15), it might erroneously appear that he is estimating a true behavioral equation. Lee *et al.* (1998, p. 321) indicate that the estimate of the coefficient of $\ln y_0$ tends to minus unity in the probability limit. Quah (1996) shows that the two percent convergence rate observed is a statistical artifact, the product of 'unit root econometrics in disguise'.

As one better approximates the identity by including other variables in the regression (compare tables IV and V in MRW, or the augmentations by Nonneman and Vandhoudt, 1996) or by including heterogeneous intercepts (Islam, 1995) and allowing the growth rates of technology to differ (Lee *et al.*, 1997), the speed of convergence increases because variations in $B(t)$ and ϕ_t are better captured. Durlauf and Johnson (1995, tables II and V, pp. 370, 375) found higher rates of convergence in the regressions for each subsample than in the single regime, but rejected the hypothesis of convergence among the high-output economies. On the other hand, Temple (1998, table 3, p. 369) did not find much higher rates of convergence, except in the lowest quartile (9.2 percent a year). The exchange between Lee *et al.* (1998) and Islam (1998) concerned differences in the size of λ as a consequence of the different estimation methods and assumptions about what is allowed to vary. Lee *et al.* (1998) report regressions where the mean speed convergence increases to 0.23 (when the restriction that g is the same across countries is relaxed) and to 0.29 (with heterogeneity in λ and in g). Islam's intuition in his exchange with Lee *et al.* (1998) was correct: 'Clearly, a different estimation method is not the main reason for this substantial increase' Islam (1998, p. 325). Maddala and Wu's (2000) estimation procedure allowed them to calculate individual convergence rates for the OECD countries. Their estimates range from 1.27 percent per year for Switzerland to 10.32 percent per year for West Germany, with an average for the 17 OECD European countries of 4.68 percent per year. And when they separate the sample into different periods, the average convergence rate increases to 19.7 percent per year for 1950–60.

As has been shown, as the restrictions on $B(t)$ and ϕ_t are relaxed (i.e. that they are the same across countries), the convergence regression estimated approximates equation (14) better; τ tends to -1 and λ increases. But this must be true irrespective of the sample size, the number of countries (in the context of panel estimation) and the estimator used. Although the exchange between Lee *et al.* (1998) and Islam (1998) about the meaning of convergence when one permits heterogeneity in growth rates provides some useful insights (most notably that the very concept of convergence becomes problematical),

it is not appreciated that the underlying problem is more fundamental, namely, that no matter what method is used to estimate this regression, the results will be conditioned by the presence of the underlying accounting identity. Technical econometric fixes do not solve the problem. The last regression in table 3 shows equation (15) estimated with a common ϕ (g in MRW). The results are very similar to those of Islam (1995, table I, p. 1141). The biases and other econometric issues discussed by Islam (1995, 1998), Lee *et al.* (1998) and Maddala and Wu (2000) are not, fundamentally, econometric problems. The whole argument rests on how close the regression used approximates the income accounting identity.

6. CONCLUSIONS: WHAT REMAINS OF SOLOW'S GROWTH MODEL?

Why are some countries richer than others? Is the neoclassical growth model, based on an aggregate production function, a useful theory of economic growth? This paper has evaluated whether the predictions of Solow's growth model, namely, that the higher the rate of saving, the richer the country; and the higher the rate of population growth, the poorer the country, can be tested and potentially refuted.

We have used MRW's specification of Solow's model and shown that a form identical to that used by MRW can be derived by simply transforming the income accounting identity that relates output to the sum of the total wage bill plus total profits. To do this only requires the assumptions that factor shares and the capital–output ratio are constant. This has allowed us to question whether Solow's growth model can be tested in the sense of allowing its refutation.

It has been argued that the key to understanding the results discussed in the literature lies in the assumption of a common level of technology and rate of technical progress across countries. Although this assumption has been discussed in the literature, the important point has been overlooked that all that is being estimated is an approximation to an accounting identity. From this point of view, the assumption of a common rate of technological progress amounts to treating the weighted average of the logarithm of the wage and profit rates that appears in the accounting identity as a constant across countries. The form derived from the accounting identity explicitly incorporating differences in growth of the weighted average of the wage and profit rates and using only two assumptions (constant shares and a constant capital–output ratio) is so close to the identity itself that it explains most of the variation in income per capita in the OECD countries. Moreover, if shares are sufficiently constant, this is sufficient to give a relationship that will explain the variation across countries in the level of productivity.

MRW's original regression, on the other hand, explained only one percent. It has been argued that MRW's equation imposes on the identity the empirically incorrect assumptions that the weighted average of the wage and profit rates and the weighted average of the growth rates of the wage and profit rates are constant across countries. The fact that this gives a less-than-perfect statistical fit may give the impression that a behavioral regression, rather than an identity, is actually being estimated. Once these two assumptions are relaxed the identity, or a good approximation to it, guarantees a good statistical fit, where the implicit values of the output elasticities are very close to the respective factor shares. The estimate of the coefficient of the savings rate must be positive and that of the sum of employment and technology growth rates must be negative. All this is solely the result of the accounting identity.

The conditional convergence equation discussed in the literature is also affected by our arguments. It has been shown that once the weighted average of the wage and profit rates is properly introduced, the 'identity' predicts that the speed of convergence, under neoclassical assumptions, must be infinite or alternatively interpreted as undefined.

The conclusion that has to be drawn is that the predictions of Solow's growth model cannot be tested econometrically because they cannot be refuted. In view of the above findings, it is difficult to end on an optimistic note. This neoclassical framework does not, in our opinion, help answer the central question of why some countries are richer than others. The implications of the paper, therefore, go far beyond a mere critique or a proposal for improvement in the estimation and testing of the neoclassical growth model. The problem discussed is far more fundamental than that of the necessity for a further augmentation of Solow's model, or the use of more appropriate econometric techniques.

From the policy perspective (Kenny and Williams, 2000; Rashid, 2000), the argument implies that we cannot measure the impact of standard growth policies, e.g. the effect of an increase in the savings rate on income per capita. However, these arguments should not be taken as implying that a country's income level is not, in some sense, related to savings and investment, population growth and technology, any more than that the production of an individual commodity is not related to the volume of inputs used, just because an aggregate production function cannot theoretically exist.

The arguments in the paper should not be misconstrued either as a claim that any regression explaining income per capita is futile because, one way or another, the right-hand side variables (e.g. countries' latitude) are proxying the right-hand side variables of the income accounting identity. The same applies to the convergence literature, that is, studying whether historically

countries have tended to converge is an important issue (the notion of sigma-convergence is not affected by our arguments). And a regression of growth rates on initial income (and perhaps other variables) certainly says something. But care is needed in the interpretation of the coefficients. The technology gap approach, for example, posits that the rate of economic growth of a country is inversely related to the technological level of the country. Important factors in this paradigm are the catch-up process and the country’s ability to mobilize resources for transforming social, institutional and economic structures (see Fagerberg, 1987).

What has to be inferred is that the neoclassical growth model, as formulated in MRW’s specification and derived from an aggregate production function, is not the appropriate place to start any discussion about growth, development and convergence. And the argument casts doubt on whether the growth rates of the labor and capital input, each weighted by its factor share, can be regarded as the ‘contribution’ of the factor inputs to the growth of output in a causal sense.

In the authors’ opinion, the above calls for a serious reconsideration of the neoclassical growth model and its explanatory power ‘of why growth rates differ’. If we were to continue to use this framework in order to think about questions of growth, we should need a different procedure and methodology to test its predictions. Given that the whole framework depends on the existence of the aggregate production function, the feasibility of this option seems problematical.

Felipe and Fisher (2003) conclude their survey on the aggregation problems by arguing that ‘macroeconomists should pause before continuing to do applied work with no sound foundation and dedicate some time to studying other approaches to value, distribution, employment, growth, technical progress, etc., in order to understand which questions can legitimately be posed to the empirical aggregate data’ (Felipe and Fisher, 2003, p. 257). It is not possible to rely on the instrumentalist argument that as all models necessarily involve abstraction because they are merely fables or stories, the fact that the neoclassical growth model can give good statistical fits means that aggregation problems or the Cambridge Capital Theory Controversies can be ignored as empirically unimportant. It has been shown precisely why these models, if correctly specified, must always give near perfect statistical fits. A model that cannot be potentially refuted empirically is not a productive metaphor.

We see two options open. First, the discussion of economic growth should be formulated in terms other than the neoclassical production function, perhaps along the lines of evolutionary growth models (Nelson and Winter, 1982). Second, there has to be a move away from the use of highly aggrega-

tive data into accounting for productivity differences at the microeconomic level. Lewis (2004) has argued in such terms and offers empirical evidence of the insights that firm-level analysis and case studies can provide for aggregate growth. Another possible way is through 'matched samples' studies of firms. See, for example, the instructive studies by Daly *et al.* (1985) and Mason *et al.* (1996), which provide some useful insights into why levels of productivity differ greatly between manufacturing firms making the same product. The concept of the aggregate production function is noticeably absent from their analysis.

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