

*What have we learned from a decade of empirical research on growth?*

## Applying Growth Theory across Countries

*Robert M. Solow*

---

I am broadly in sympathy with the spirit of the article by William Brock and Steven Durlauf and that by William Easterly and Ross Levine. They are trying to move the literature in the right direction. I say this even though I have been skeptical from the beginning about the interpretation of cross-country growth regressions.

The potential problem of reverse causality has been obvious to everyone. It has usually been met with the standard econometric dodge: using lagged values of slow-moving variables as instruments. But this cannot be a serious solution to the problem. The causality issue points to a deeper question: Do cross-country regressions define a meaningful surface along which countries can move back *and* forth at will? If this is the idea, what mechanism could underlie such a surface? Brock and Durlauf call such a regression a “model.” I suppose in a statistical sense it is. But an economic model should have some internal structure; its causal arrows should rest on some sort of behavioral mechanism, and that seems to be missing in this literature.

I think I had this prejudice even before cross-country regressions became fashionable. I thought of growth theory as the search for a dynamic model that could explain the evolution of one economy over time. There were no explicit cross-sectional implications. Were there implicit ones? Certainly, and my comments bear on the question of what they might be.

### A JUSTIFICATION FOR MULTIVARIABLE CROSS-COUNTRY GROWTH REGRESSIONS?

In my view growth theory was conceived as a model of the growth of an industrial economy. Its parameters certainly could not be regarded as fixed forever, but maybe they would need to be reconsidered only over intervals of 30–50 years, long enough so that the differences between endpoints could not be dominated by demand-driven business cycles. So far as I can remember, I have never applied such a model to a developing economy, because I thought the underlying machinery would apply mainly to a planned economy or a well-developed market economy. This is not a matter of principle, just wariness.

Robert M. Solow is with the Department of Economics at the Massachusetts Institute of Technology. An earlier version of this article was presented at the World Bank conference “What Have We Learned from a Decade of Empirical Research on Growth?” held on 26 February 2001.

© 2001 The International Bank for Reconstruction and Development / THE WORLD BANK

Now suppose that you want to compare several such economies and make inferences from the comparison. I use these general terms to emphasize that the cross-country regression is not the only way to make comparisons. You could intend just to interpret parallel time series for a small group of countries, with the goal of understanding the source of differences among them. What background assumptions do you have to make if such a comparison is to be sensible?

The economies you are comparing must have something in common, some part of the driving mechanism. If Robert Summers and Alan Heston were suddenly to discover national income and product accounts from an economy that existed on Mars a million years ago, you would not expect that economy to fit neatly into a Barro regression. So the economies must have some things in common, but not everything. One possible specification that early writers tended to make in this spirit, perhaps automatically, and that some perhaps still make, is that the economies in question share common technological knowledge. The normal justification for this assumption was that technological handbooks were easily available everywhere, even before the Internet. So the basic commonality could be the knowledge of a production function  $F(K,L,H;A)$ , not necessarily Cobb-Douglas in form. Of course, countries would have different values of  $K$ ,  $L$ , and  $H$ , but a strict interpretation would give them the same value of  $A(t)$ .

Within the model, which leaves only the saving-investment pattern, the growth rate of employment, and the rate of depreciation—as well as initial conditions, of course—to differ from country to country. Those are the implications to be explored. We are all familiar with exercises like this. Right or wrong, they are coherent. But then what is the role of all the other right-hand-side variables—openness to trade, size of government, black market premium, and degree of inequality, to name just a few that appear frequently? Maybe we should regard them as purely descriptive, a search for empirical correlations with no analytical implications. That would be a respectable occupation. But to stay within the model, we have to think about the role of total factor productivity (TFP) or  $A(t)$ .

When I used to teach growth theory, I would always begin by saying, “Let’s imagine a toy economy that produces only one homogeneous good, using as inputs just (the services of) a stock of the good itself and a flow of labor.” In that context it is natural to think of  $A(t)$  in purely technological terms. That may be how the habit was established of supposing that the shape of the production function *and* the path of  $A(t)$  were common across countries. But soon questions would arise; if they didn’t, I would raise them myself.

For example, someone would be sure to wonder if there really is significant substitutability between labor and capital, at least enough to make the model interesting. The routine reply is, “That is an empirical question, so we have to go outside the narrow model.” Empirically speaking, every industrial economy produces not one but thousands of different goods and services. Even if each of them operates with a Leontief technology, they exhibit a wide variety of capital-labor ratios, from the very labor-intensive, such as personal services, to the very capital-intensive, like electricity generation. The economy as a whole—what the

toy economy symbolizes—can substitute between capital and labor by changing the composition of output in the obvious way. Moreover—and this is very important—there is an elementary market mechanism to make this happen: If capital becomes scarce, its rental price will rise relative to the wage; the price of capital-intensive goods will rise relative to the price of labor-intensive goods; demand will shift to labor-intensive goods; and the aggregate capital-labor ratio will fall. (And that is not the end of the process.)

Once you go down that road, you have to rethink the production function and especially the role of  $A(t)$ . It is certainly unwise to assume that all economies are equally efficient at reallocating inputs across industries. This difference in efficiency would be reflected in  $A(t)$ , and maybe not only there. As soon as that thought enters your mind, it immediately occurs to you that there are many other nontechnological factors that could influence the level and growth of TFP. They would include the intensity of competition—domestic or foreign—because that would influence the amount of waste and slack in various industries, the alacrity with which the national economy adopts new technology, and thus the level and growth of TFP. You can just as easily imagine that the amount and nature of regulation in a country can affect the efficiency of resource allocation, and thus the “effective” level of TFP and quite possibly its rate of growth. Even among Organization for Economic Co-operation and Development (OECD) countries analysts have found that substantial international differences in productivity can persist even within a narrowly defined industry.

It is easiest to think of such institutional differences as if they could be summed up as international differences in TFP, but the situation could be more complicated. We usually model TFP as if it were a Hicks-neutral multiplicative factor. That might be harmless if the production function is Cobb-Douglas. But that assumption is rarely tested. (Michael Boskin and Lawrence Lau [2000] have tested it in a cross-country panel context involving a dozen or so countries and a translog technology, and they reject it strongly.) One obvious generalization is to allow arbitrary factor augmenting technological change, so that the production function would be written as  $F(A(t)K, B(t)L, C(t)H)$ .

Does this line of thought provide a justification for multivariable cross-country regressions? Probably. But it also suggests focusing more directly on TFP or factor augmentation functions as the proper left-hand-side variables in empirical work and thinking more seriously about right-hand-side variables that might legitimately account for differences in TFP or in  $A(t)$ ,  $B(t)$ , and  $C(t)$ . Current practice seems to be much too haphazard.

#### GROWTH THEORY AS THE THEORY OF THE EVOLUTION OF POTENTIAL OUTPUT

One might protest that only pure labor augmentation allows the existence of a steady-state growth path. Maybe that should not be a concern. Easterly and Levine point out that observed growth paths do not look like steady states, ex-

cept perhaps in the United States. This could be related to the nature of factor augmentation. In heavily agricultural economies, for instance, weather and disease could play the role of disturbances to steady-state growth, and their effects would not be represented as Hicks-neutral. But an old Keynesian like me suspects that deviations from steady-state growth are often related to deviations of output from potential output—often demand failures with sticky prices, possibly export failures with sticky prices.

A serious attempt to study the nontechnological components of TFP should also get away from the indefensible presumption that actual output is always close to potential output. In poor countries, especially monocultural primary producers, the choice of off-potential end points, even 20–30 years apart, can materially distort the measured growth of output and maybe even more so the measured growth of TFP. Industrial countries have been less vulnerable to this problem in the postwar period. But Japan has produced nowhere near its potential output for a decade, and I have my doubts about contemporary Europe.

This point may be worth developing. I think of growth theory as precisely the theory of the evolution of potential output. So it is mostly concerned with the supply side of the macroeconomy. Deviations are demand driven. In advanced industrial countries we have ways of estimating the growth of potential output. Some depend on a version of Okun's Law; others work directly with production functions. But all are explicitly trying to measure potential output. That is why I think of real-business-cycle models and the indiscriminate use of the Hodrick-Prescott filter as intellectually backward steps.

For other contexts we need other methods. To take the simplest example, agricultural economies, especially single-crop ones, are subject to large fluctuations in output and output growth stemming from such things as droughts and pests. Easterly and Levine provide examples. How should we deal with such observations? We can define potential output as the output that could be produced with normal weather. An estimate of that quantity (or the TFP derived from it) should be on the left-hand side of a growth regression. Or else we need a weather or disease prevalence variable on the right-hand side. In the first case weather-induced fluctuations are just removed; in the second case they are included in TFP but segregated. Without such makeshifts, we are asking for trouble in cross-country studies that include many countries at different stages of development. If those studies are growth-oriented, they should be aimed at explaining potential output or TFP.

In effect I am agreeing strongly with the advice of Easterly and Levine: Comparative growth studies should focus on understanding and analyzing the various sources of differences in TFP and the policies that might affect them. This goes for both technological and nontechnological factors in TFP. My residual doubt is whether using cross-country regressions involving large numbers of countries with different institutional histories is the best way to go about this task. I would prefer to start with qualitative studies of basically similar countries, extended over time and space.

This could be taken as a step toward endogenous growth theory, and so it is. The good thing about the fox that Paul Romer started chasing more than 15 years ago is that it leads us to focus on the analysis of the economic incentives to create new technology. I have two suggestions for those engaged in this hunt. First, close attention to what goes on in research and development enterprises might pay off more at this stage than simple mechanical modeling that may be off the mark. I once had the opportunity to observe the General Motors research laboratories at close hand; the incentives and responses were anything but simple.

Second, the nontechnological sources of differences in TFP may be more important than the technological ones. Indeed, they may control the technological ones, especially in developing countries. Obvious examples include things like the security of contracts, the intensity of competition, and respect for instrumental rationality as a mode of behavior. There may be less obvious ones.

### CONCLUSION

I conclude by returning to an old hobbyhorse of mine. Our modeling exercises are usually carefully tailored to lead, all too transparently, to a steady-state growth rate. This habit induces analysts to make gratuitous linearity assumptions and to impose other more or less arbitrary restrictions on models. But maybe, as Easterly and Levine argue, steady-state behavior is a rarity outside a few successful advanced industrial countries. Nicholas Kaldor's stylized facts may still have relevance, but not everywhere.

One of the advantages of vast computer power is that theory does not need easy special cases, such as purely labor augmenting technological change or the analogous assumption that allows a steady state to be an attractor even without constant returns to scale. Numerical integration or iteration can answer the kinds of questions we want to ask of a model, even if the model is not tractable with pencil and paper. Correspondingly, it is probably not a good idea to set cross-country regressions the task of explaining the rate of growth or some other stationary characteristic. It is time paths that need to be modeled and studied.

Circumstances have given these remarks a discursive character. So I would like to end by distilling my three main points:

- If they mean anything at all, those many right-hand-side variables in growth regressions are determinants of TFP. But then they should be selected with that function in mind, and TFP (or its growth rate) should be the left-hand-side variable. Moreover, some of those factors could affect other characteristics of the aggregate production function. Allowing for separate factor augmentation functions would be a first step, but there are other possibilities. It may be necessary to think about genuine estimation of the underlying production function.
- The proper measure of output underlying the left-hand-side variable is potential output. In industrial countries there are standard methods of

approximation (not all of which are satisfactory). The deviation of actual from potential output is mainly demand driven. Some of the sharpest deviations of actual from potential output occur in primary producing countries. These deviations may be related to weather fluctuations or disease, but export failures may be a demand-side source. There are various thinkable ways to deal with this issue; they need to be systematized.

- The exponential steady state is a theoretical convenience. But many countries, much of the time, are nowhere near steady-state growth. This suggests that comparative studies should focus less on the growth rate and more on comparing and understanding whole time paths.

#### REFERENCE

- Boskin, Michael, and Lawrence J. Lau. 2000. "Generalized Solow-Neutral Technical Progress and Postwar Economic Growth." Discussion Paper 00-12. Stanford Institute for Economic Policy Research, Stanford University, and Working Paper 8023, National Bureau of Economic Research, Cambridge, Mass.