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

Comment on “It’s Not Factor Accumulation: Stylized Facts and Growth Models,”
by William Easterly and Ross Levine

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When economists in the 1950s and 1960s used growth models to understand the experience of developing countries, they allowed for the possibility of technology differences between developing countries and the United States. But because they did not have a good theory for talking about the forces that determined the level of the technology—in the United States any more than in developing countries—technology factors tended to be pushed into the background in policy discussions.

Modeling the Technology Factor

In the 1980s, several economists began to develop formal models of this technology factor, which has conventionally been designated $A$. We used those models to think about the behavior of $A$ in the United States and in other countries. These “new growth” models had striking implications. Output per capita could diverge. Capital and skilled workers might flow from poor to rich countries. Trade in goods or investment decisions by firms could influence the diffusion of $A$ between developed and developing countries. All this meant that government policies that affect incentives for firms could have big effects on economic outcomes.

The new growth theory of the 1980s generated a counterreaction in the 1990s that Pete Klenow and Andres Rodriguez-Clare have called the “neoclassical revival.” Proponents of the neoclassical view argued that for purposes of explaining cross-country variation in levels of income or rates of growth, economists could return to the framework of the 1960s and append the strong assumption that $A$ is the same in all countries. In this approach, the cross-country variation in the level and rate of economic development could be understood entirely in terms of differences in the level and rate of accumulation of traditional inputs: physical capital, human capital, and unskilled labor.

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225
Attacking Business as Usual in Empirical Growth Theory

The article by William Easterly and Ross Levine is part of the next swing in the scholarly pendulum. It moves away from the critical assumption in the neoclassical revival that the level of technology is the same in all countries. Equally important (if not more so), it makes an implicit case for moving away from a narrow focus on testing models. In its place, the authors challenge economists to understand what happens in the countries that they study.

At a substantive level, they suggest that there is abundant evidence that something such as the level of the technology does vary across countries. That much is clear. But at a methodological level, they are doing more. They attack business as usual in empirical growth theory. To avoid the threat that a wide variety of evidence would pose to the neoclassical revival, economists who supported this program advocated a narrow methodology based on model testing. Pick a few summary statistics generated from a specially selected data source. (Typically they are partial correlations generated by running a regression on a cross-country data set.) Use strong theoretical priors to restrict attention to a very small subset of all possible models. Then 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.”

The obvious problem with this approach is that many models are consistent with the few correlations that emerge from a single data set. Many alternative models are never considered in the standard model testing exercise, so it has no power against these alternatives, no ability to weigh their plausibility relative to that of the “good model.” Suppose an economist runs a regression conditioned on other variables and finds that countries with lower initial income grow more rapidly. Within a neoclassical model with an exogenously determined level of \( A \), this finding can be interpreted as evidence of diminishing returns to physical capital or human capital because of exogenous variation in rates of investment in physical or human capital. But it is also possible that the technology is lower in the country that starts at a lower level of development and grows faster as better technology diffuses there. If the economist looks only at the cross-country regression evidence, there is nothing that would raise questions about the initial assumption of identical technologies in all countries.

But as Easterly and Levine point out, if you bring other evidence, such as the pattern of flows of people between countries, the identical technologies model no longer fits. For someone who wants to maintain an unreasonable prior assumption, the advantage of a narrow focus on one piece of data is that it does not threaten the convenient theoretical framework built on this prior. It is possible to go through the motions of doing science, testing various theories and rejecting some in favor of others. But far from advancing the science, this approach is a dead end. It does not allow for rejecting or modifying prior beliefs that simply turn out to be wrong.
Of course, economists who would like to stay in the business of doing these narrow, model-testing exercises have a response. They say that one model should be used to explain cross-country regressions and another to explain factor flows between countries. If they were defense attorneys in a criminal case, they could provide a different theory to explain every specific piece of evidence at the crime scene. But they would not be able to tell a consistent story about what actually happened.

No amount of methodological obfuscation about “as-if” modeling, “parsimony,” “pushing the limits of benchmark models,” and the like should be allowed to hide the fact that, like judges and jurors, policymakers and economists have to make judgments about what actually happened. Otherwise, they have no basis for making well-informed decisions about what to do next. Easterly and Levine show economists how they can best contribute in this effort—assemble as much evidence as possible and search for a consistent theoretical explanation that fits it all.

At a substantive level, the authors suggest that there is abundant evidence that something like the level of technology does vary between countries. Admitting this possibility could have important implications for policy. If economists take the classical model seriously as a description of the development process—that is, if they accept that an economy is well described as a competitive equilibrium with an exogenously given level of technology—then Harberger’s pioneering analysis of the welfare costs of distortions tells us that bad government policies imply tiny welfare losses. Economists would then be left with a striking conclusion, one that few practitioners would take seriously: Countries are poor not because of bad policies but because of exogenous preference differences that cause them to accumulate less physical and human capital. The risk is not that practitioners will take this conclusion seriously, but that they will dismiss formal economic theory and empirical work as flawed beyond repair.