

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

Comment on “Growth Empirics and Reality,”  
by William A. Brock and Steven N. Durlauf

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World Bank economists are mostly practical people—people who try to answer the question, “What exactly should this particular country do right now?” But if they had hoped that the growth regression lessons summarized in William Brock and Steven Durlauf’s article would enhance their practical advice giving, they might feel some dissatisfaction. How would they change their advice to, say, Brazil? But that is why this article is important conceptually. It goes to the heart of the matter by proposing a change in the empirical growth literature’s fundamental methodology—from model testing to decision theoretic.

The article’s valiant but flawed attempt reveals the difficulties in making this shift, however. I’d like to make three points: There is a tension between the interests of academics and practitioners in growth regressions. Output response heterogeneity is a huge practical problem. And policy decisions can be guided only in broad outlines by growth regressions.

THE TENSIONS BETWEEN ACADEMIC AND PRACTICAL INTERESTS

What Paul Romer said about the intellectual history is on point: First there was this received model, the Solow model, and then along came others—but nearly all were essentially models of Organisation of Economic Co-operation and Development (OECD) countries. The interest in empirically testing these competing models of the evolution of GDP in technologically advanced countries led in two directions, reflecting two sources of pressure.

The first source of pressure is that even though a model may be about Germany or the United States (technological leaders), data from Guyana and Papua New Guinea and Senegal get recruited into increasing the degrees of freedom for the statistical tests among models. But that should not lead anyone to believe that the model is about Guyana.

The second is that the model testing perspective focuses attention on hypotheses that present a clean separation between alternative models—but not necessarily on what is empirically or practically most important. This may account for the seemingly casual approach to the specification of “policy.” Robert Hall,

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in a nice paper in honor of Professor Solow's birthday, (Hall 1990) basically points out that if we take Solow's "toy" model literally, everything should be orthogonal to total factor productivity (TFP). The reason is that given the model's assumptions, there cannot be incentives to augment TFP because factor payments to labor and capital exhaust product. From the narrow perspective of model testing, proving that *any* policy affected steady-state or TFP growth by altering incentives provides a clean separation of the models. Empirical work that couldn't possibly contribute to the practical advice but provided a clean separation between competing models became academically important.

#### THE PROBLEM OF OUTPUT RESPONSE HETEROGENEITY

Assume that we are considering a permanent shift in the value of policy  $P$  from level  $P$  to  $P'$  at time  $t$ . A key question is, "What is the difference between properly measured GDP with policy  $P'$  and that with policy  $P$  at time  $t$ ,  $t + 1$ ,  $t + 2$ , out to  $t + N$ ?" A huge problem with empirical estimation of an output response function, however, is that there is no reason to believe that it looks the same—across countries or over time.

Brock and Durlauf's article does a good job of emphasizing this problem by focusing on parameter heterogeneity. But even characterizing the problem as parameter heterogeneity—and, especially, limiting heterogeneity to a small number of linear interactions—artificially narrows the problem, as output response function could vary in many ways for many reasons. It could depend on structural differences across economies, on economic structure, on institutional differences that would mediate the policy change, or on complementary policies that could affect the output response of a given policy.

This proliferation of parameters for characterizing output response makes the empirical situation seem bad—but it is much worse than that. Even for countries observationally equivalent in terms of structure, output response dynamics could depend on timing, expectations, and history. So a policy that may be perfect for bringing a country out of a recession might be neutral, or even counterproductive, at the peak of the cycle. Expectations play a huge role in output response—observationally equivalent policy changes can potentially have enormously different impacts depending on whether all people believe that the policy change will persist or all believe that it will not persist. Finally, there may be output response function hysteresis, in which the output response function depends not just on conditions today but on an economy's entire history of policy changes and their impacts.

#### THE LIMITS OF GROWTH REGRESSIONS AS A SOURCE OF POLICY ADVICE

The policy variables that go into growth regressions are at a level of abstraction far greater than that at which policy recommendations and decisions are made

and implemented in the real world. So although it is possible to come up with a growth regression that says that, on average, countries that are more open tend to grow faster, that leaves a million questions about trade policy reform unanswered: Should the tariff be lowered on this good or that set of goods? Should tariff reductions be *concertina* or from the top down? Should changes be carried out in one stroke or phased in?

Suppose we take the decision-theoretic approach to empirics seriously. The inevitable problem is that the level of specificity at which most growth economists need to work is far greater than can ever be adequately informed by growth regressions. Some people act disappointed that we haven't learned more from growth regressions—but we have to live with the fact that growth regressions are not going to tell us what the tariff on capital goods in Brazil should be in 2001.

Growth regressions are incredibly useful in providing a general empirical background of stylized facts about the world. The partial associations of policy variables with growth provide a grounding in reality from which policy discussions can build. But none of us is in any danger in the near term of being replaced by an automaton based on growth regressions that takes in country conditions, searches the data, and then spits out policy solutions. Policy decisions draw on a variety of information and remain the domain of that most elusive of qualities: good human judgment.

#### REFERENCE

- Hall, Robert. 1990. "Invariance Properties of Solow's Productivity Residual." In Peter Diamond, ed., *Growth/Productivity/Unemployment: Essays to Celebrate Bob Solow's Birthday*. Cambridge, Mass: MIT Press, pp. 71–112.