Journal of Economic Literature
Vol. XL (September 2002) pp. 907–918

Review of Easterly's *The Elusive Quest for Growth*[1]

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1. *The Failure of Development Panaceas*

All-encompassing hypotheses concerning the sources of economic growth periodically surface, and with the support of adequately chosen cross-country correlations, enjoy their fifteen minutes of fame. Over the last few decades, the list of proposed panaceas for growth in per-capita income has included high rates of physical-capital investment, rapid human-capital accumulation, low income inequality, low fertility, being located far from the equator, a low incidence of tropical diseases, access to the sea, favorable weather patterns, hands-off governments, trade-policy openness, capital-markets development, political freedom, economic freedom, ethnic homogeneity, British colonial origins, a common-law legal system, the protection of property rights and the rule of law, good governance, political stability, infrastructure, market-determined prices (including exchange rates), foreign direct investment, and suitably conditioned foreign aid. This is a growing and non-exhaustive list.

These attempts to elucidate the sources of growth, and therefore to discover the miracle policies that could solve the problem of underdevelopment, are often viewed with skepticism by those in our profession who justly favor empirical work closely connected to models of optimizing behavior, clearly identifiable causal relationships, and large samples of data. Few comparative studies of growth have these characteristics, and as a result simply pronouncing the words "cross-country growth regression" in an academic seminar increasingly attracts scorn or disgust. On the other hand, the questions posed by this literature are among the most fundamental in economics. As Robert E. Lucas once put it, "once one starts to think about them, it is hard to think about anything else."[3] There are no good alternatives to comparative growth studies to explain differences in the wealth of nations.

William Easterly provides us with a vivid reminder of this point. His superb book draws on what we have learned from almost two decades of cross-country growth comparisons. He adopts a modest tone, recognizing that on numerous occasions in the past, supposedly miracle growth policies have proven disastrous or ineffective. Wisely, Easterly avoids proposing a new panacea of his own. The book breaks with a now well-established tradition of throwing every variable under the sun into the kitchen sink of growth regressions, emphasizing instead simple correlations chosen from the "greatest

[1] *The Elusive Quest for Growth: Economists' Adventures and Misadventures in the Tropics*. By William Easterly. 2001. Cambridge and London: MIT Press. Pp. xiii, 342. $29.95. ISBN 0–262–05065–X.

[2] Stanford University and NBER.

[3] Lucas (1988), p. 5.

hits" of the cross-country growth literature.[4] Easterly, however, does not break with the best tradition of World Bank humanism: by concluding each chapter with the narrative of the lives of real people in the developing world, he reminds us of the importance of the ultimate goal of growth research—raising living standards.

The aspect of the book that has attracted the most attention is its criticism of current aid policies.[5] Easterly describes convincingly the vicious cycle of adjustment loans directed by the World Bank and IMF at countries that squander these resources in current consumption rather than investment, leading to stagnant growth, debt crises, debt relief, and further adjustment loans. While there is evidence that aid directed at countries with effective policies can foster growth, there is no evidence that aid, as a matter of principle, is allocated to these countries.[6] Rather, despite the stated "conditionality" of IMF and World Bank structural adjustment loans, international politics almost always determines aid allocation, and when it does not the recipient's domestic politics make sure that the conditions are promptly thrown by the wayside. The observations that aid is insufficiently conditional, and that the weak conditions imposed are not enforceable, are not new. It is almost certain that, when the dust from the recent debates on aid effectiveness settles, politicians in donor and recipient countries will carry on with business as usual, and aid will continue to be disbursed on the basis of political expediency rather than sound economics.[7] What is distinctive about the book's criticisms of aid policies is that they were formulated by a World Bank official, but these criticisms are not where the main contributions of Easterly's book lie. For this reason, and because his criticisms of multilateral aid policy have been reviewed elsewhere, I will not dwell on them any longer.[8]

What *The Elusive Quest for Growth* will be remembered for most is its largely successful attempt to provide a critical synthesis of the current state of empirical knowledge on growth. Indeed, it provides an excellent opportunity to discuss what we know, and what we do not know, about the determinants of cross-country growth performance. Easterly carefully reviews recent empirical studies on this topic, many of which were authored or coauthored by him. Even a skeptic of cross-country comparisons of economic growth would be struck by how much we have actually learned from these studies. We have learned enough to decisively reject misguided policies that have, at some point or another in the post-Second World War history of development thinking, been viewed as ideal solutions to the problem of development: state planning; price controls; import substitution; aid directed at filling the so-called "financing gap" between domestic savings and "required" investment; debt forgiveness; policies that assumed benevolence on the part of developing countries' policymakers, ignoring the economic and political incentives to which they are subject (yes, even they). Again, the list could go on, but the point is that simple conditional correlations from cross-country

[4] Much of this work originated at the research department of the World Bank, of which Easterly was an active part.

[5] The prime consequence of this criticism was to force a career change on the book's author, but we will not dwell on that topic.

[6] For a recent nuanced defense of multilateral development assistance, see The World Bank, "The Role and Effectiveness of Development Assistance: Lessons from World Bank Experience," March 2002. This paper is widely viewed as a response to Easterly's criticisms. A notable commonality between this recent paper and Easterly's book is that they draw largely on the same basic research in cross-country growth. The inferences about what this research entails for the conduct of aid policy seems to be the main distinguishing factor—Easterly stressing past failures and the World Bank manifesto stressing the successes.

[7] Notwithstanding recent pronouncements of the Bush administration to the contrary—see for example "World Leaders to Discuss Strategy for Aid to the Poor," by Joseph Kahn and Tim Weiner, *New York Times*, March 18, 2002.

[8] See "Helping Hands: How Foreign Aid Could Benefit Everybody," by John Cassidy, *The New Yorker*, March 18, 2002, pp. 60–66.

datasets can lead to a decisive updating of priors on the range of possible growth-enhancing policies. This is a huge achievement for a literature so often derided as lacking in its identification of causal links and in the robustness of its results. The skeptics make some good points, however, and their scorn should steer us toward better research.

In what follows I will use Easterly's book as a starting point to assess what we have learned from two decades of empirical growth studies (section 2). I will then step away from the book to address two salient methodological issues facing this literature (section 3), concluding with a summary of directions for future research (section 4).[9]

2. *Correlates of Economic Growth*

In his (largely successful) attempt to appeal to a wide audience, and to avoid being caught up in the technical debates of the literature on the determinants of economic growth, Easterly focuses on correlations between economic performance and its hypothesized determinants, based on the most-often cited papers in this literature. I would argue that this is in fact the best use that can be made of these cross-country studies of growth. Even simple or partial correlations can restrict the range of possible causal statements that can be made, and nowhere is this more the case than in the comparative growth literature, where causality is especially difficult to establish. For example, the partial correlation between indicators of corruption and growth is negative, in a wide variety of specifications. This, clearly, does not *imply* that corruption negatively *impacts* growth. But the sign of the partial correlation makes it more difficult (although not impossible) to argue that corruption is in fact good for growth, or, equivalently, that an increase in corruption would *lead to* an increase in growth.[10] The correlation, in other words, restricts the range of the possible. In contrast, sophisticated attempts to establish the direction of causality of a growth relationship, either through instrumentation or the use of time lags, are rarely based on structured models of the process being estimated (rather, they are based on finding "clever" instruments). As a result, the corresponding estimates are hard to interpret, and these attempts are generally characterized by heroic claims of causality—this is a topic to which we shall return below.

So what are the main correlates of economic growth? Research has focused on several broad categories of growth determinants, and rather than exhaustively reviewing these numerous determinants, we will focus on the categories.

2.1 *Determinants of Growth in the Augmented Solow Model*

The Solow model, in its augmented version allowing for both human and physical capital, is the theoretical foundation of modern growth regressions.[11] The very specification that growth empiricists invariably start from, involving growth in per-capita income on the left-hand side, and initial income, the rates of physical-capital investment, of human-capital accumulation and of labor-force growth on the right-hand side, can be formally derived from a log-linearization of the Solow model around its steady-state.[12] It is well known that the aforementioned variables, in this model, are determinants of the steady-state *level*

[9] See also Jonathan Temple's survey of the evidence on new growth theory in the JEL (1999).

[10] See Paolo Mauro (1995) for the first cross-country empirical study on the relationship between corruption and growth. In the 1960s and 1970s, some observers suggested that corruption could enhance economic efficiency by "greasing the wheels" of burdensome bureaucracies. See for instance Nathaniel Leff (1964) and Samuel Huntington (1979). A sufficiently robust partial correlation is adequate to do away with this view with a high level of confidence.

[11] See N. Gregory Mankiw, David Romer, and Philippe Weil (1992).

[12] Strictly speaking, this list of determinants should also include the rate of depreciation of the capital stock. This term is generally omitted under the assumption that depreciation rates do not vary across countries or through time, and is therefore reflected in the constant term. The consequences of relaxing this assumption, to my knowledge, have not been addressed empirically.

of income, and therefore affect its *transitional* growth rate only. As Easterly reminds us, the only determinant of *long-term* per-capita income growth in the Solow model is labor-augmenting technological progress, which is taken as exogenous. The current level of technology is also a determinant of steady-state income levels, and the realization of this fact, coupled with the small share of the variance of growth explained by the Solow regressors, has opened up a Pandora's box of kitchen-sink growth regressions, aimed at explaining the rest of the variance.

The fact that the investment and population growth variables in cross-sectional growth regressions are the determinants of the level of steady-state income is too often forgotten, and sometimes leads to the erroneous conclusion that we should not expect these variables to appear with coefficients significantly different from zero on the right-hand side of growth regressions, on the grounds that they do not affect long-term growth. Such a view underlies Easterly's critique of physical-capital investment, human capital, and population growth as determinants of per-capita income growth.[13] Higher rates of investment in physical and human capital, and lower population growth still have a great role to play in development, since they relate to the ultimate level of income. He is right to point out that they are no panacea, but this is because they account for only a small fraction of this level, not because they are not correlated with growth. With thirty or so years of data, it is simply impossible to distinguish steady-state growth from transitional growth, since common cross-sectional estimates of the half-life of a transition to the steady-state of the Solow model suggest periods in the range of 32 years.[14] Moreover, steady-state determinants themselves change through time, so that we are shooting a moving target. Distinguishing empirically between steady-state growth and transitional growth is one of the holy grails of growth empirics, but is a task that has not been tackled convincingly, and it is not clear that the available cross-country data will allow a satisfactory treatment of the subject.

In cross-sectional empirical studies, the Solow model, especially in its augmented form, performs quite well. Variables enter with the expected signs and the parameter restrictions implied by the Solow model tend to hold up.[15] Conditional on the determinants of the steady-state, convergence is observed. In this narrow sense, there is much evidence that the neoclassical determinants of "growth" (i.e., steady-state income levels) enter significantly. Easterly's critique of physical-capital investment, human-capital accumulation, and population control policies as panaceas for growth may in this respect seem at odds with some of the available evidence.[16] Ross Levine and David Renelt (1992) in fact showed that the physical-capital investment rate was one of the only robust regressors in growth regressions, a fact that seems to hold up quite generally both in within-country and between-country studies.[17] There is an

[13] This is done in chapters 2–5 of his book. He does recognize and discuss the importance of these variables for transitional dynamics, for example on page 54.

[14] See for instance Robert Barro and Xavier Sala-i-Martin (1995), chapter 10. Fixed-effects estimates suggest much shorter periods, but as we will argue these estimates are not without problems of their own.

[15] See Mankiw, Romer, and Weil (1992) and Nazrul Islam (1995).

[16] To be fair, his critique is nuanced and subtle. For instance, he recognizes that there is a long-run correlation between investment rates and growth, and simply suggests this relationship does not hold in the short and medium run (p. 40). He also suggests that a third factor (technology) explains the observed long-run relationship between investment and growth. On human capital, he also provides a balanced overview of the debate (ch. 4), stressing that a consensus does not exist, and reviewing the Pritchett-Mankiw debate.

[17] The robustness of the investment rate in growth regressions has recently been confirmed by Sala-i-Martin (1997), using a methodology of robustness analysis that seeks to uncover the entire distribution of estimates on each growth determinant, rather than extreme bounds alone. Extreme bounds were devoid of statistical meaning, so Sala-i-Martin's technology is arguably superior. It also assigns robustness to many more variables than in Levine and Renelt's extreme bounds analysis. Some have also questioned the direction of causality between investment and growth, but we are here concerned with mere partial correlations.

ongoing debate on the importance of human-capital accumulation for growth, which started with Lant Pritchett's (2001) observation that increases in schooling did not seem to translate into increases in growth, but some early studies found strong support for the hypothesis that at least some measures of human-capital accumulation have statistically significant predictive power.[18] Finally, Easterly's statement that "the general wisdom among economists . . . is that there is no evidence one way or another that population growth affects per capita growth" (p. 91) may seem at odds with several studies, such as Barro (1991) and Barro and Xavier Sala-i-Martin (1995), where fertility rates consistently enter with a statistically significant negative coefficient. There is perhaps less consensus on this point than the author may imply.

Easterly's real critique of these variables as growth determinants is that they explain too little of the variation in growth to constitute panaceas for development policy, and this is a very good point. But whether they enter significantly is subject to more debates than the author concedes. Consensus is not common in this literature, but, if anything, the preponderance of the evidence is that the neoclassical determinants of growth do enter significantly in growth regressions. Explaining the rest of the cross-country variance in growth, however, is where Easterly's book really shines.

2.2 *New Growth, Luck, and the Residual*

If none of the classical growth panaceas deliver the goods, what explains persistent or growing differences in per-capita income? In other words, how can we explain the residual? Easterly suggests that the observed unconditional divergence in per-capita incomes is a sign that endogenous growth based on increasing returns is at play.[19] Unfortunately, there is little systematic empirical evidence to support or to contradict this statement. Arguably, while endogenous growth has proven to be one of the most successful new ideas in economics in the last two decades, generating an enormous theoretical literature since Paul Romer started it all in 1986, it has not nearly attracted the interest it deserves from empiricists. This is in part due to the multiplicity of models of endogenous growth, each with their specific emphasis (external or internal increasing returns, knowledge spillovers, endogenous R&D, etc.) and their specific predictions (existence of a steady state or not, multiple equilibria, scale effects, etc.). No single tractable workhorse model of endogenous growth, that could mirror what the Solow model did for "old growth," has appeared.[20] "Tests" of endogenous growth theory, as a result, are all over the place. In chapter 8, Easterly describes interesting examples of traps, o-rings, knowledge leaks, and spillovers, but these are little more than anecdotes. He cites no systematic cross-country empirical results about which of these "flavors" (one or more) best accounts for residual growth, in particular for developing countries. He can hardly be faulted for it: these results are hard to come by.

Explaining the residual has constituted another holy grail of empirical growth research since Edward Denison (1962)

[18] The studies include Barro (1991) and Mankiw, Romer, and Weil (1992), among numerous others. The debate stems largely from the appropriate measure of human capital. The augmented Solow model suggests that flow measures such as enrollment rates are appropriate, since they proxy for the rate of accumulation of human capital. Pritchett (2001) has attacked these measures as poor proxies, and uses instead a measure of the growth in human capital constructed from stock series, finding coefficients close to zero.

[19] As suggested above, an alternative hypothesis would be that the determinants of steady-state income levels, in some augmented Solow model, themselves diverge for some reason. This, it seems, would be testable using cross-country data on these determinants and their estimated impact on growth.

[20] The closest we came to a "workhorse" model is the AK model, judging by its widespread use in applied work; but it is hardly a "model" at all, since long-run growth is posited more than explained. See Barro and Sala-i-Martin (1995).

showed that factor accumulation accounted for only a small portion of U.S. growth, and since Dale Jorgenson and Zvi Griliches (1967) showed that accounting for improvements in the quality of factors did not eliminate Denison's large residual. As discussed above, cross-country growth regressions deliver a similar message, using a methodology that differs radically from growth accounting. Here, we are unable to explain much of the variance in cross-country growth using the Solow regressors alone (or the augmented Solow regressors), and we lack systematic macro evidence on the various flavors of endogenous growth that could help account for the "technological progress" portion of growth. It could therefore appear that we have made little progress on the quest for growth. One hypothesis, which Easterly reviews thoroughly, is that growth is simply random (ch. 10). So, by page 214 of his 290-page book, he has shown that the old growth panaceas can at best only explain a small portion of cross-country growth differences, speculated that endogenous-growth forces (technological progress) might account for growth, and ultimately acknowledged that the unexplained variance of growth could also be due to randomness. Are we then to believe that the quest for growth is bound to failure? The answer to this question is unambiguously negative: there is an indirect way to explain how technological progress and factor accumulation explain growth, and that is by looking at the features of economies that facilitate them: structure and policies.

2.3 *Structure and Policies*

If the structural features of economies or government policies can to some extent explain the cross-sectional variation in per-capita income growth in reduced-form growth regressions, support can be claimed for theories that stress incentives to innovate or accumulate capital (in its various forms) as necessary conditions for innovation and accumulation to occur.[21] Fortunately for growth empirics, there is a large and growing body of evidence supporting this view.

The structural features of an economy include characteristics that are, at least to a large extent, beyond the direct control of policy makers, and plausibly exogenous over the horizon of growth studies (typically around thirty years). These include climate, geographic features such as country size and location, and the ethnic, religious, and linguistic composition of a country. Structure also includes characteristics of social organization, such as political institutions, political instability, and the extent of strife or conflict that are endogenous over long horizons. Finally, it includes structural features such as sectoral composition and diversification, infrastructure, income inequality, and market structure that are endogenous at shorter horizons. Rather than survey an exhaustive list of these potential determinants of TFP growth and factor accumulation, it is sufficient to stress here that considerable evidence has been found that a large number of these variables, in some form or another, are correlated with economic growth, and therefore qualify as candidates for proximate causes of growth.[22] Among the salient research projects in this recent tradition, we can cite John Luke Gallup, Jeffrey Sachs, and Andrew Mellinger (1999) and Sachs (2001) on geography and growth, Easterly and Levine (1997) on ethnic fractionalization, Alberto Ades and Edward Glaeser (1999), and Alberto Alesina, Enrico

[21] This point is made nicely by Hall and Jones (1999), who stress that what they term "social infrastructure" (a concept that is meant to capture institutional features of societies, as well as governmental policies) will determine the returns to accumulation and technological progress, themselves the proximate causes of growth or income levels. They focus on explaining income levels rather than growth rates, but the same point can apply to growth regressions.

[22] Again, causality is rarely established convincingly, so we are better off referring to the possibility of causality, allowed for by the sign and statistical significance of partial correlations.

Spolaore, and Romain Wacziarg (2000) on country size, Alesina et al. (1996) on political instability, Daron Acemoglu, Simon Johnson, and James Robinson (2001) on institutions, to mention but a few. Easterly's book summarizes the findings of some of these studies and many more.

Correlates of growth related to policy or governance have also received considerable attention. This recent literature paints a more optimistic picture of the potential for raising growth in developing countries, because unlike luck and to a large extent economic structure, policies are under the immediate control of policymakers. For this reason, the number of policies that have been entered on the right-hand side of growth regressions exceeds even the number of structural features that have been tested for. Easterly (and more generally the World Bank's growth project) are behind much of this research on policy and growth, and his chapters 11 and 12 survey the findings of the literature on, respectively, policy and governance as they relate to economic growth.[23] The usual findings are that bad policies correlated negatively with growth—high black-market premiums, a large share of government spending in GDP, fiscal deficits and public debt, very high inflation, and protectionist trade policies, to name a few. Bad policies are usually highly correlated with each other, too, and that makes it hard to pinpoint exactly which policies are worse than others. But, as with structural factors, there is now little debate on the role of policy quality overall for growth.

There are some debates on the impact of specific policies. For example, a recent debate concerns the impact of trade policy openness on growth, a important topic on which Easterly unfortunately spends too little time (ch. 11, p. 229). There is considerable evidence that various measures of openness, whether policy measures or outcome measures, are positively associated with growth even after controlling for a variety of other factors.[24] A recent paper by Francisco Rodríguez and Dani Rodrik (2000) is now often cited as casting some doubt on this finding. They review several major contributions to the empirical literature on trade and growth, and conclude that these results are not robust to changes in measurement concepts or specifications. Another possible interpretation of their findings, however, is more optimistic for openness. They basically make two statements:

1) The first is that protectionism is highly collinear with other indicators of "bad" policies, such as a high black-market premium and poor macroeconomic management, and that these have adverse effects on growth. Since all these policy variables are measured with error, it is hard to disentangle exactly which is in fact relevant. The high collinearity between openness and other policy variables suggests exploring a causal interpretation: that more-open countries adopt better policies, and that this indirect channel may explain part of the effect of openness on growth. The effects of openness may extend to governance and institutions, and this is a fruitful area for future research.[25] This point is also a call for a more structural approach to growth empirics, where causal links are better specified and estimated, rather than blindly running reduced-form kitchen-sink regressions (more on this below).

2) The second statement, in their words, is "we do not want to leave the reader with the impression that we think trade protection is good for economic growth. We know of no credible evidence—at least for the

[23] See also http://www.worldbank.org/research/growth.

[24] Another recent literature has argued that the extent of the market, of which openness is only one aspect, is what really matters. See Ades and Glaeser (1999), Alesina, Spolaore, and Wacziarg (2000) and Spolaore and Wacziarg (2002).

[25] Ades and Di Tella (1999), for instance, show that a higher rate of imports to GDP is associated with lower indicators of corruption across countries. Wacziarg (2001) shows that more-open countries have better macroeconomic management.

post-1945 period—that suggests that trade restrictions are systematically associated with higher growth rates." When it comes to cross-sectional growth regressions, it is almost always possible to find specifications that will knock out or even reverse the sign of a variable of interest, so a finding that there is no credible evidence of a negative relationship between openness and growth could be deemed a great achievement by the standards of this literature.

Whatever the debates on specific variables, the quest for growth in the academic world has fruitfully explored geography, institutions, and policies, and has uncovered in the process a wealth of useful relationships.

3. *Methodological Issues in Growth Empirics*

Easterly's book and the empirical literature on growth leave us with a "nexus of correlations," i.e., a broad set of correlates of growth over which some amount of consensus has emerged. Accumulation variables and population growth play some part in accounting for the cross-country variation in growth, but a small part. Institutions, geography, economic and political structures, policies and governance relate to the incentives to innovate and accumulate, and go some way toward explaining the rest of the variation. Randomness is responsible for the residual.

This pattern of partial correlations provides some indication of the sources of growth, but their interpretation as causal effects is generally problematic.[26] In addition, with the tendency of the literature to move to kitchen-sink regressions, parameter estimates that previously had clearly defined theoretical meaning in the context of the Solow model have completely lost their neat mapping into behavioral parameters.[27] The problem of reverse causation is compounded by the possibility of an omitted-variables bias—there is no guarantee that exogenous shocks, as opposed to some omitted factor(s), are truly responsible for the unexplained variance of growth in any of the studies surveyed above. In this section we will examine the methodological issues associated with omitted variables and reverse causation.

3.1 *Fixed Effects: The Great Regression*?

In the second half of the 1990's, the recognition that cross-country variations in the level of unobserved technology (the A parameter of the Solow model) could be correlated with the traditional Solow regressors, and therefore lead to omitted variables bias, led to two strands of literature: the first was to incorporate proxies for technology levels, or facilitators of technology adoption, directly into growth regressions, in an attempt to account for a greater portion of the cross-country variation in growth. This has led to the literature on structures and policies surveyed above. Another idea consisted of examining within-country variation in the data, under the assumption that technology levels vary mostly across countries, rather than through time. By incorporating fixed-effects in growth regressions, it was thought, time invariant cross-sectional differences in technology levels could be controlled for

[26] At the same time, the comment that "this regressor is endogenous" is both the easiest to make and the most common comment at academic seminars. I have heard pretty convoluted and unlikely stories for why causality could be reversed in specific cases. While establishing the direction of causality is a noble goal, concerns about reverse causality are sometimes taken too far. As mentioned earlier, simple correlations can also go a long way towards constraining our priors on the world.

[27] For example, as mentioned above, there is a tendency to view structures and policies as facilitators of innovation and accumulation, by providing suitable incentives and a suitable environment for these activities. But the way in which structures or policies operate is never specified—a simple reduced-form growth regression in no way informs us on the channels whereby the multiplicity of regressors that are entered affect economic growth.

without specifying what the components of this technology term were. A series of papers appeared isolating the within-country variation in the data.[28]

The main findings of this literature were twofold: firstly, the estimated speed of convergence to the steady-state was much higher. The half-life of the transition under cross-sectional estimates was around thirty years, and it was reduced to five–ten years under fixed effects. In other words, the absolute value of the estimated coefficient on lagged income was now increased. Secondly, many of the variables typically found significant in cross-sectional regressions were now insignificant, for example human-capital indicators.

The tendency to employ within-country estimators in the empirical growth literature has perhaps not been subjected to sufficient criticism, and has been too quickly embraced as a solution to the problem of cross-country technological heterogeneity. The first unfortunate consequence of this tendency was to force analysts to construct panels from their data, thus reducing the focus from the long-run to higher frequency data. The effects were relatively limited in practice because, with estimators that employ between-country variation, estimated growth relationships are quite stable with respect to the chosen frequency of the data (five-year, ten-year, or thirty-year averages). Random effects estimates based on five-year data are remarkably similar to OLS estimates on thirty-year averages, suggesting that the between-country variation in the data is the main source of variation.

The second unfortunate effect was a conceptual impoverishment: by relying on fixed effects to account for technological differences, the burden of specifying the unexplained portion of growth was somewhat lifted. On both a policy and a conceptual basis it is more appealing to examine the partial correlation of meaningful variables and growth, even in *ad hoc* specifications, rather than to rely on country dummies to get rid of unwanted cross-sectional variation, and call that "technology."

One could retort that fixed effects do not preclude controlling for such variables, but unfortunately the third undesirable side effect of isolating the within-country variation prevents this from being an effective response: fixed effects exacerbate measurement error. This is true for example when the right-hand side variables are persistent through time, and measurement error is white noise. Most growth regressors are highly persistent through time (hence the preponderance of the cross-sectional variation in random effects estimates), and since fixed effects amounts to taking differences from means, what is left is mostly noise.[29] This is particularly true for variables such as human capital or institutional variables that do not change much through time (most measures of human capital simply trend upwards almost everywhere). Unfortunately, nobody has paid much attention to this problem, and no serious attempts have been made to quantify its impact. A simple Monte Carlo exercise would suffice, under alternative assumptions on the incidence of measurement error, to determine whether the gains in terms of correcting the omitted variables bias resulting from the exclusion of the technology parameter exceed the costs of exacerbated measurement error.

To conclude on this point, the use of fixed effects is neither conceptually nor econometrically an appealing way to address the

[28] See Malcolm Knight, Norman Loayza, and Delano Villanueva (1993), Islam (1995), and Francesco Caselli, Gerardo Esquivel, and Fernando Lefort (1996), among many others.

[29] More formally, it can be shown in a univariate regression setup that whenever the autocorrelation coefficient in the measurement error is smaller than the autocorrelation coefficient in the "true" variable, the error-to-truth ratio will be larger for the differenced variable than for the level variable, and the bias will be larger using the differenced data.

issue of technological heterogeneity. The quest for appropriate proxies to this missing term should go on.

3.2 *Toward Structured Growth Empirics*

As suggested in section 2, the existing cross-country empirical literature on economic growth has provided us with a vast set of partial and simple correlations. What remains to be done is to provide a systematic interpretation of these correlations in light of existing models of growth. Unless they can be interpreted in light of some model, there is no way to provide theoretical meaning to these correlations. Attempts to instrument for the variables of interest using plausibly exogenous variables have taken the literature in this direction by resolving some causality issues, but this trend could yet be taken further.[30] Simply using an instrumental variables estimator does not confer to the estimates any structural meaning, unless an underlying model specifies what is being estimated. Growth empirics is an area of inquiry where empirical work is held to one of the lowest standards in economics when it comes to its theoretical foundations.

To address this shortcoming, a fruitful direction for future research would consist of specifying models with proximate and fundamental causes. The proximate causes of growth are accumulation (of various forms of capital) and technological change (measured for instance by total factor productivity growth). The fundamental causes are structures (institutions, geography, demography, etc.) and policies that facilitate or hinder accumulation and technological change. Instead of recognizing the benefits of separating these layered causes of growth, the typical empirical paper simply estimates a reduced form in which all these variables appear simultaneously on the right-hand side.[31] The causal links between them are lost and no conceptual meaning can be given to the estimates. Too much emphasis is placed on statistical robustness, and insufficient emphasis is placed on the mechanisms that link the variables under study. The next great leap forward in the empirical study of growth will occur when growth empiricists gather the wealth of relationships and correlations that their imposing literature has accumulated, and make theoretical sense of them.

4. *A Research Agenda for Growth Empirics*

William Easterly has given observers and policy makers in industrial and emerging countries a striking description of the failures of past and present development policies. Most of his arguments turn on its head the critique commonly heard from the Seattle Man and other anti-globalization protesters: development policies have failed because aid was insufficiently conditional and monitored, not excessively so. Incomes have stagnated or diverged in part because policymakers have pursued policies of import substitution and market closing, not because of globalization. Domestic policies and politics, not multinationals and capitalist imperialists, are largely to blame for unproductive rent-seeking and plunder. For this reason, the book has already had and will continue to have a profound impact in the current debates on aid effectiveness and aid allocation. At the same time, Easterly remains humble on the prescriptive side, recognizing that there is no miracle cure, and

[30] Famous recent attempts have included gravity variables such as bilateral distance, country size, and geography to estimate the effects of trade volumes on growth (as in Jeffrey Frankel and David Romer 1999) and the mortality rate of European colonialists as an instrument for institutions (Acemoglu, Johnson, and Robinson 2001).

[31] A recent exception is Hall and Jones (1999), who recognize and apply this point in the context of explaining level differences in per-capita income. José Tavares and Wacziarg (2001) estimate a full structured model of growth and its determinants in an attempt to estimate causal channels between political institutions and growth. Wacziarg (2001) does the same for trade policy openness.

that achieving high rates of growth requires the confluence of many specific policies and hard-to-satisfy conditions.

For academics, the book's main lessons perhaps lie elsewhere: Easterly has provided an excellent opportunity to assess what we have learned from two decades of growth regressions and to define broad areas for future emphasis. We have actually learned quite a lot from running cross-country growth regressions, and this may come as a surprise to those who too easily dismiss these studies as data-mining and prior-driven research. Yet we have only scratched the surface. We should continue to pursue the holy grail of accounting for unexplained growth. We should refocus on functional form and structured specifications to make sense of the available "nexus of correlations." In particular, we should clarify and estimate the theoretical relationships that link structures and policies to accumulation and innovation.

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