The Rise and Fall of Cross-Country Growth Regressions

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In this article I describe the evolution of the use of cross-country growth regressions in economics over the last two decades. The rise of cross-country growth regressions was an important component of the sea change in economic research associated with the new growth economics. By their fall, I do not mean to suggest that such regressions are no longer used; the opposite is very much the case. Rather, the word fall concerns how these regressions have been interpreted in the context of growth theories. Certain forms of these regressions enjoyed a period in which they were taken as the statistical analogs of the law of motion implied by the neoclassical growth model in general and the Solow growth model in particular. Since Robert Solow’s original model is most commonly used to justify both the linear structure of cross-country regressions and the forms in which certain core variables appear, it will be my primary focus; Mankiw, Romer, and Weil 1992 continues to be the standard derivation of cross-country growth regressions.

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1. References to the Solow growth model are not meant to slight Trevor Swan’s (1956) contribution but to reflect the terminology conventionally used in the empirical growth literature.

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growth regressions from Solow dynamics. Remarkably, this specification was commonly used even when the neoclassical model was augmented with growth determinants that Solow’s original growth model took as either exogenous (e.g., technical change) or as fixed background variables (e.g., legal institutions). New growth theories have to a large extent been evaluated by adding empirical proxies to a linear regression justified by the dynamics of the Solow model. While the practice of comparing candidate growth determinants via their statistical significance in growth regressions continues, the stronger claims as to the interpretation of these exercises have greatly diminished. I argue that growth regressions are now used to generate a modern form of stylized facts, which provides a parallel to earlier ways in which growth theory and empirics were linked. However, I also argue that the particular stylized facts that have been developed are not ideal in terms of creating a synergy between theory and empirics.

The evolution of the interpretation of cross-country growth regressions is a good example of how theory, econometrics, and empirical practice evolve together. New growth theories stimulated the use of cross-country growth regressions to identify their empirical salience. Growth econometrics evolved as a way to bring theory and empirics closer together, but had the effect of uncovering limits to the theory-empirics relationship; in this sense the ubiquitous identification problem emerged in a growth context. Empirical practice has partially adjusted to the econometric criticisms, but primarily by relaxing the strong claims that were made earlier in the development of growth empirics. My view is that it would be very difficult to argue that this coevolution has been nearly as successful as parallel developments in the study of business cycles, let alone when compared with advances in microeconomics. After discussing the case of growth regressions, I draw some comparisons with business cycle theory and empirics.

Rise

The relationship between the Solow growth model and cross-country regressions was initially quite casual. In a generally neglected paper, Roger Kormendi and Philip Meguire (1985) conduct cross-country regression exercises that are conceptually identical to those that are now conventional; it is difficult to identify any methodological difference between their
work and that subsequently found in Barro 1991 as discussed below.\(^2\) (Barro’s subsequent writings, e.g., Barro and Sala-i-Martin 1992, are more explicit in linking neoclassical dynamics to a linear regression.) Kormendi and Meguire motivate the inclusion of particular variables in their regression, specifically initial income and population growth, by appealing to “standard neoclassical growth theory” (143) and evidently regard this as sufficiently well known that they do not cite either Solow, Trevor Swan, or optimal growth variations because of David Cass or Tjalling Koopmans. One likely reason why their paper has received so little attention is that its focus is on the growth effects of macroeconomic variables such as the inflation rate. While variables of the type they studied have reemerged in the empirical growth literature (see Durlauf, Johnson, and Temple 2005 for a survey), Kormendi and Meguire were not interested in the neoclassical model per se. For them, the model suggests variables whose omission could have resulted in spurious correlations with respect to those growth determinants in which they were interested.

The seriousness with which the neoclassical growth model was taken as the basis for econometric work profoundly changed with the emergence of endogenous growth theory. Within the new growth economics, the neoclassical model naturally represented a comparison point against which to evaluate theories such as Romer 1986 and Lucas 1988, in which increasing returns to scale imply the possibility of perpetual growth without exogenous technical change. Barro 1991, which arguably launched the industry of identifying variables that explain cross-country growth differences, gave explicit motivation of Barro’s exercise to evaluating the convergence property of Solow (and other) growth models. His analysis opens with “in neoclassical growth models such as Solow (1956) . . . a country’s per capita income growth rate tends to be inversely related to its starting level of income per person” (Barro 1991, 407). So, while Barro 1991 is no more formal than Kormendi and Meguire 1985 in Barro’s derivation of a statistical model corresponding to the neoclassical theory, his analysis

\(^2\) Kormendi and Meguire is not cited in either Barro 1991 or Mankiw, Romer, and Weil 1992. My own large-scale surveys on empirical growth research (Durlauf and Quah 1999 and Durlauf, Johnson, and Temple 2005) do cite the paper, but otherwise pay very little attention to it. My conjecture for this neglect is that Kormendi and Meguire’s focus was on the role of macroeconomic policies on growth, policies that are generally associated with the business cycle, as opposed to lower-frequency dynamics. Policy variables of this type did subsequently emerge as candidates for growth explanation.
gives primary focus to the empirical implications of the neoclassical model. Barro and subsequent authors have interpreted the finding that the regression coefficient on initial income in these regressions is typically negative as evidence against the Romer and Lucas view. If, ceteris paribus, lower initial income is associated with higher subsequent growth, this relationship implies that contemporaneous income differences are narrowing, a property that is in turn predicted by an aggregate production function in which the marginal product of capital is decreasing. The idea that the marginal product of capital (human or physical) was increasing in its level represents the key conceptual difference between the neoclassical and first-generation endogenous growth theories, as an increasing marginal product can produce growth in initial income differences and thereby capture the increasing international inequality observed since World War II.

The empirical relevance of the Solow model, and the interpretation of cross-country growth regressions as a complete econometric version of the growth dynamics implied by the model, is given its fullest development by N. Gregory Mankiw, David Romer, and David Weil (1992), who open their paper by describing their analysis as the direct legatee of Solow: “This paper takes Robert Solow seriously. . . . This paper argues that the predictions of the Solow model are, to a first approximation, consistent with the evidence” (407). “Our results indicate that the Solow model is consistent with the international evidence if one acknowledges the importance of human as well as physical capital” (423). Their analysis concludes that the Solow model can explain approximately 70 percent of cross-country growth variability from 1950 to 1985 for a large cross-section of countries.

The research program initiated by Barro and Mankiw, Romer, and Weil continues to deeply influence empirical growth research. Steven Durlauf, Paul Johnson, and Jonathan Temple (2005) find that growth regressions have been used to assess over forty distinct growth theories, with over 140 different variable choices to explore these theories. Further, the theoretical implications of the Solow growth model condition how these regressions are constructed. To the extent that an analyst wishes to move beyond the determinants of the Solow model, additional explanatory

3. In the growth literature, this negative coefficient property is known as β-convergence. See Durlauf, Johnson, and Temple 2005, 2008 for an overview from statistical and economic perspectives.
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4. The Solow model implies that cross-country growth differences may be explained via initial income, the population growth rate, the human capital savings rate, and the physical capital savings rate. These are sometimes referred to as the Solow variables. Mankiw, Romer, and Weil (1992) provide specific forms of the variables, when the aggregate production function is Cobb-Douglas. These particular forms have not always been employed in subsequent work.

regressors are added to those suggested by Solow; the statistical significance of these additional variables is interpreted as affecting the level of technology as occurs in Solow’s original framework. Growth regressions of course do not exhaust modern growth empirics; while growth accounting exercises (themselves the product of a fundamental contribution, this time Solow 1957) play an important empirical role in current growth debates, cross-country regression analysis still maintains pride of place in empirical work.

A good example of the extent to which cross-country growth regressions, at their heyday, have been used to make substantive economic claims is the relationship between democracy and growth. Based on the statistical significance (or lack thereof) in a series of cross-country regressions, Barro (1996, 24) has made such general claims as the following:

The analysis has implications for the desirability of exporting democratic institutions from the advanced developing economies to developing nations. The first lesson is that more democracy is not the key to more economic growth, although it may have a weak positive effect for countries that start with few political rights. The second message is that political freedoms tend to erode over time if they get out of line with a country’s standard of living. . . . The more general conclusion is that the advanced western economies would contribute more to the welfare of poor nations by exporting their economic systems, notably property rights and free markets, rather than their political systems, which typically developed after reasonable standards of living had been attained. . . . in the long run, the propagation of Western-style economic systems would also be the effective way to expand democracy in the world.

Barro has made such country-specific claims as the following:

Thus growth would likely be reduced by further democratization beyond the levels attained in 1994 in countries such as Malaysia and Mexico. Moreover, political liberalization has probably gone beyond the point
of growth maximization in places such as Chile, South Korea, and Taiwan. (Barro 1997, 59)

Controversy

In discussing the emergence of criticisms of cross-country growth regressions, I use Solow’s view of his model as an organizing framework. Solow ranks among the visible critics of how cross-country growth regressions have been used to make broad substantive claims. As early as 1994, only three years after the publication of the Barro paper and two years after Mankiw, Romer, and Weil, he wrote that

a particular style of empirical work seems to have sprung from the conjunction of growth theory with the immensely valuable body of comparative national-accounts data compiled by Summers and Heston (1991). It rests on international cross-section regressions with the average growth rates of different countries as the dependent variable and various politico-economic factors on the right-hand side that might easily affect the growth rate if the growth rate were easily affected. 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 represent different “points” on some well-defined surface. . . . The temptation of wishful thinking hovers over the interpretation of these cross-section studies. It should be countered by cheerful skepticism. The 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 grew more slowly over this or that period. (Solow 1994, 51)

He later remarked,

I have been skeptical from the beginning about the interpretation of 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. . . .
So far as I 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. (Solow 2001, 283)

Solow’s criticisms correspond to three components of the criticisms of cross-country growth regressions:

1. **endogeneity**: the variables in both the neoclassical model and new growth alternatives are themselves endogenous, so that causal claims about determinants are invalid.

2. **model uncertainty**: there is no *a priori* basis for identifying the appropriate set of variables that represent explanations outside of those suggested by the neoclassical model, hence claims about any particular set of growth determinants are based on an ad hoc choice of model.

3. **exchangeability**: different countries do not represent draws from a common growth model, in particular a linear one; heterogeneity in the objects of analysis cannot be reduced to differences in the values of control variables and differences in the realizations of shocks to the growth process.

Each of these criticisms has been developed in detail as the empirical growth literature evolved and has in turn produced a range of responses. Endogeneity has, from the perspective of the literature, received the most attention. Early work, for example, Barro and Lee 1994, treated endogeneity from the perspective of classical simultaneous equations analysis and used lagged dependent variables in panel analogs to the original cross-section data sets; the justification for these types of instruments follows from assumptions about the lag structure of the growth process, which do not possess any economic basis; as such they were naturally susceptible to Christopher Sims’s (1980) critique of the arbitrary use of exclusion restrictions to identify business cycle models. This style of analysis was, unsurprisingly in light of the history of simultaneous equations modeling in business cycle macroeconomics, supplanted by efforts to find instrumental variables whose legitimacy may be argued on substantive grounds. This search parallels the popularity of identification of natural experiments in microeconomics, which as shown by James Heckman (1996) is a form of instrumental variables estimation. One famous example is Daron Acemoglu, Simon Johnson, and James Robinson’s (2001) use of settler mortality rates as an instrument for the effects of colonial institutions on
growth; the idea underlying their empirical analysis is that higher mortality stemmed from exogenous geographic factors, leading to a lower migration from the colonial power to the colony, which in turn reduced the extent to which the colonizers’ institutions were transplanted. Another example, from Frankel and Romer 1999, is the use of geographic variables to proxy for economic openness. Here the idea is that differences in geography have exogenously induced variation in the ability of countries to integrate themselves with the rest of the world. Jeffrey Frankel and David Romer claim that these instruments may be used to construct consistent parameter estimates that capture the causal effect of trade on growth.

The issue of model uncertainty, unlike endogeneity, generally has been addressed using relatively powerful econometric methods. Uncertainty about the appropriate set of growth determinants initially led to efforts to identify “robust” growth determinants. An early example comes from Ross Levine and David Renelt (1992), who applied extreme bounds analysis to a wide set of growth variables and concluded that the only robust growth variables were initial income and the share of investment in GDP, with coefficient signs as predicted by the neoclassical model. Modifications of extreme bounds analysis have also appeared, for example, in Sala-i-Martin 1997, which asks whether, across 95 percent of model specifications, a coefficient has the same sign. Neither extreme bounds analysis nor modified approaches have proved that influential in the growth literature. One reason is that the interpretation of the robustness criterion is problematic; the extreme bounds approach corresponds to a minimax decision problem, which is difficult to reconcile with the objectives of a growth exercise; alternatives such as Sala-i-Martin’s do not have any decision-theoretic interpretation (Brock and Durlauf 2001). A more popular approach has involved the use of model averaging methods (Fernandez, Ley, and Steel 2001; Sala-i-Martin, Doppelhofer, and Miller 2004; Durlauf, Kourtellos, and Tan 2008). In this approach, the primary alternative involves model selection algorithms (Hoover and Perez 2004; Hendry and Krolzig 2004). Heterogeneity in cross-country experiences has been modeled via nonlinearities (Durlauf and Johnson 1995; Bloom, Canning, and Sevilla 2003) and random coefficients interpreted as realizations from a hyperdistribution (Canova 2004).

Exchangeability has been addressed in a host of ways. The most popular approach has been to allow for fixed effects in a panel growth regression; different countries are allowed to have idiosyncratic growth components, but these are assumed to be invariant across time. An early
influential example is Islam 1995. This approach is not particularly in the spirit of Solow's argument, as it essentially assumed that, modulo a constant of proportionality, each country is described by the same aggregate production function. Another approach in ensuring exchangeability involves an explicit allowance for country-specific coefficients. Kevin Lee, M. Hashem Pesaran, and Ron Smith (1997, 1998) do this in the context of a panel analysis that uses one-year time increments (very short by growth analysis standards); Fabio Canova (2004) employs Bayesian methods in a cross section. While these approaches are closer to the spirit of Solow, they raise a conceptual issue that is delineated in an exchange between Lee, Pesaran, and Smith (1998) and Nazrul Islam (1998): at what point does the parameter heterogeneity become the object of interest, rather than the model, per se?

Other work in growth has attempted to treat exchangeability violations through the introduction of nonlinearity into growth regressions. An early example is Durlauf and Johnson (1995), who argued that different regimes exist for the aggregate production function. These regimes are associated with differences in initial income and literacy. This approach is related to work by David Bloom, David Canning, and Jaypee Sevilla (2003), which attempts to uncover the coexistence of multiple growth regimes, one corresponding to a poverty trap, the other to sustained growth. Other authors have focused on linking parameter heterogeneity to nonlinearity by modeling parameters as functions rather than constants. Thanasis Stengos's work is the key example of this strategy and predates subsequent authors who have used similar methods; see Liu and Stengos 1999 and Mamuneas, Savvides, and Stengos 2004. Durlauf, Andros Kourtellos, and Artur Min-kin (2001) extend Stengos's thinking to perhaps its logical limit and produce a "local" Solow growth model in which each level of initial income and initial literacy is associated with a distinct aggregate production; to be precise, each of these initial conditions produces a distinct set of Cobb-Douglas production function parameters. Cross-country differences in production functions are implicitly measured by differences in initial conditions.

How can one summarize the state of practice for cross-country growth regressions? With respect to endogeneity, the instrumental variables proposals that have been made are widely used. This is so even though the validity of the instruments suffers from a basic conceptual problem (Brock and Durlauf 2001), namely, there are no good reasons why the instruments are orthogonal to the model errors. This parallels the dispute in the
microeconometrics literature about natural experiments; see the debate between Heckman (1997, 1999) and Joshua Angrist and Guido Imbens (1999). For model uncertainty, model averaging techniques have become increasingly popular. As for exchangeability, fixed-effects corrections are ubiquitous for panel data analyses. On the other hand, nonlinear methods have not been widely adopted by empirical growth researchers.

Fall

The various critiques of cross-country growth regressions have perhaps had an influence in that these regressions are in general no longer interpreted in the way done by Mankiw, Romer, and Weil, that is, as a structural econometric analog of the Solow growth model.5 One sees relatively little contemporary research on β-convergence, especially as a source for resolving the relative merits of the neoclassical and endogenous growth approaches. In contrast, one sees relatively simple growth regressions employed to buttress various substantive economic claims.

A good example of this evolution is the debate between the relative role of institutions (Acemoglu, Johnson, and Robinson 2001; Rodrik, Subramanian, and Trebbi 2004) versus geography (McArthur and Sachs 2001; Sachs 2003) in explaining long-term differences. To focus on one very prominent example, Dani Rodrik, Arvind Subramanian, and Francesco Trebbi (2004) argue in favor of a primary role of institutions as opposed to other fundamental growth factors based on a three-variable regression in which an empirical proxy for institutions “competes” against an empirical proxy for geography and an empirical proxy for the degree of integration with the rest of the world. The Solow variables are in essence ignored, on the basis that they are proximate rather than ultimate causes of growth. While neither side in the debate has explicitly denied that there may be separate roles for geography, integration, and institutions, there is considerable attention given to monocausal explanations. Relative to the work of Barro and Mankiw, Romer, and Weil, the empirical study of “fundamen-

5. My belief that the econometric criticisms of growth regressions have helped deflate the stronger claims on their interpretability is to some extent a conjecture, as it represents my impressions of the growth literature and not a detailed examination of how these criticisms diffused in the literature and across researchers. Another factor in their reduced significance is the rise of microeconomic data sets for developing economies, which has allowed for different ways to evaluate growth factors such as randomized experiments. See Banerjee 2008 for a spirited argument that these types of data analyses trump growth regressions as a methodology.
tal” determinants of growth is a step backward both in terms of the diversity of growth determinants that are considered and in terms of explicit links between a statistical framework and any underlying theoretical model. And it also seems to be a step backward in terms of substantive thinking. More econometrically sophisticated studies such as Bleaney and Nishiyama 2002 find that larger sets of growth determinants outperform smaller subsets of the type used to advocate particular theories; model averaging efforts have not buttressed claims about the importance of the particular theories on which various horse races are based. This is not surprising, since there is no a priori reason to expect cross-country growth behavior differences to reduce to a single major determinant or a very small set of determinants, which is exactly the upshot of advances that have been made in the modern theory.

**Back to the Future**

Nevertheless, in my judgment, it would be a mistake to dismiss this new, limited use of growth regressions. Cross-country regressions have begun to evolve into a tool for pattern recognition and construction of stylized facts. In considering the debate on the respective importance of institutions and geography, one can conclude that, modulo measures of malaria, it is difficult to identify a marginal value for geographic variables in explaining cross-country growth heterogeneity, whereas one can do so for institutional measures. For the specific context of Acemoglu, Johnson, and Robinson’s work, the finding that mortality rates are correlated with growth differences is itself a data pattern of interest and one that theory ought to address.6 This is a very different approach to empirical work from one in which patterns are used to “prove” that institutions matter.

By moving away from strong claims and focusing on the identification of sturdy empirical growth regularities, cross-country regressions can eventually play the same constructive role that Kaldor's stylized facts did in the neoclassical literature. Solow’s original analysis was motivated by the desire to understand major features of long-run growth as experienced by advanced industrial economies, an experience in which growth did not

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6. Albouy (2008) has questioned the construction of the settler mortality variable and argued that correct construction fails to replicate the initial claims made about its predictive power. This dispute is far from resolved. The dispute illustrates the measurement problem I have emphasized as a limit to empirical progress in growth economics.
produce either permanent mass unemployment or permanent mass labor shortages, as occurred outside knife-edge cases in the Harrod-Domar model. Further, the linkage of the neoclassical model to Kaldor’s stylized facts, as done in Solow’s 1970 book, provided a range of dimensions along which the model could reasonably be concluded to provide empirical insights.

But unlike Kaldor’s stylized facts, current analyses involve questions of statistical significance of regression coefficients as opposed to the identification of interesting empirical regularities. (And here I refer to regularities beyond the brute fact that there is much heterogeneity in international experiences.) While these types of exercises address the fact of heterogeneity in cross-country growth experiences, they do not organize their analyses in ways to understand salient facts such as the discrepancy between sub-Saharan Africa and the rest of the world. Hence, while cross-country growth regressions have moved away from the structural claims of Mankiw, Romer, and Weil, they have yet, I believe, proceeded to explicitly identify regularities of the type that were so important in the development of the neoclassical theory. We simply do not have the equivalent of the Kaldor stylized facts to evaluate alternative growth theories.

This need for stylized facts also applies to policy evaluation; cross-country growth regressions are still used to make policy recommendations. From the perspective of policy-relevant regression analysis, the main need is for stylized facts that link policy heterogeneity and growth heterogeneity, facts that are not contingent on strong prior beliefs about the “true” growth model. This modest view of how empirical research should inform growth policy is consistent with Solow’s view on the contribution that theory can make. Solow’s views of the policy implications of his model are very modest; his classic 1970 book states,

Any theory that says something about the real world is likely to have implications for policy. But it is only good sense to realize that an abstract

7. Even here, Solow is remarkably modest; his 1956 paper does not emphasize the empirical salience of his analysis relative to the Harrod-Domar approach.

8. While interesting has a subjective component, it is not simply a matter of taste, as arguments can be made based on the current body of economic knowledge as to whether a given fact is or is not interesting; a similar point applies in arguing that one ethical claim is correct versus another (Nagel 1997).

9. Brock, Durlauf, and West (2003) argue that contemporary empirical practice in growth economics, when applied to growth policy, is flawed because it does not respect the decision-theoretic nature of the exercise; some ways to constructively proceed are made.
theory, like the one I have been developing, can only say abstract things about economic policy. At the very beginning, I described the aggregative theory of growth as a parable. You expect a parable to have a moral, but hardly to contain concrete instructions for the conduct of life. So here, when I talk of policy implications, I have to stay roughly at the same level of abstraction as the theory on which they are based. (71)

My conjecture is that stylized facts about the portfolios of policies that are highly correlated with successful growth experiences will prove to be policy relevant. More specific policy conclusions require detailed analysis of the individual country in question.

Conclusions: Parallels with the Evolution of Business Cycle Research

The evolution of empirical growth research, as exemplified in cross-country growth regressions, has interesting parallels with the evolution of business cycle analysis from the 1960s to the rational expectations revolution to contemporary perspectives. One of Solow’s (1994, 49) criticisms of modern growth economics is that it takes too extreme a view of theory, particularly when the growth process is interpreted through a representative agent paradigm:

I cannot say the same about the use made of the intertemporally optimizing representative agent. Maybe I reveal myself as old-fashioned, but I see no redeeming value in using this construction, which Ramsey intended as a representation of the decision-making of an idealized policymaker, as if it were a descriptive model of an industrial capitalist economy. It adds little or nothing to the story anyway, while encumbering it with unnecessary implausibilities and complexities.

This criticism by Solow has, to a large extent, been accepted on the empirical side of growth economics, as evidenced by the removal of the neoclassical model from the forefront of cross-country growth analyses. While I believe that there needs to be more work in developing growth regressions as a source of stylized facts and as a source of policy-relevant evidence, it is certainly the case that their links to theory have been attenuated.

Solow has made similar criticisms of the developments in business cycle macroeconomics, as the leading model paradigm of dynamic stochastic general equilibrium models (DSGE) emerged; see Solow 2002
for some of his general views and Hahn and Solow (1995) for a fully delineated alternative to some of what passed at the time for microfoundations. Aspects of this type of criticism have guided developments in macroeconomics over the last decade. At one level, fully rigorous modeling has proved consistent with the incorporation of many of the substantive objections that were raised to the early incarnations of the DSGE. The robustness research program initiated by Lars Hansen and Thomas Sargent (see their 2007 book for a brilliant synthesis) has challenged the rational expectations assumption; the now-standard new Keynesian model includes a range of market frictions that were unheard-of in the early rational expectations literature. Further, macroeconomics has found a useful role for models based on relatively “weak” microfoundations. Much of the modern work on monetary policy evaluation is based on models in which expectations and lagged terms appear, but which are not explicitly generated by individual decision problems. While it is certainly the case that there exist variants of these models with fully delineated microfoundations (exemplified in Woodford’s [2003] magisterial development of the new Keynesian model), it is the case that models with weak microfoundations have a current respectability. Yet these efforts to enrich business cycle theory have been consistent with the preservation of much closer links between the theory and empirics than now occurs in growth economics.10

Why are there differences between the theory-empirics nexus for business cycle and growth research? Here, I see two distinct reasons. A first reason is theoretical and concerns the open-endedness of growth models that, following Brock and Durlauf 2001, means the mutual compatibility of different growth theories. The research program developed by Lars Hansen, Finn Kydland, Robert Lucas, Edward Prescott, and Sargent on the microfoundations of macroeconomics focused on precisely articulated economic environments. The limitations of these assumptions became the source of the recent developments to which I have referred. It is no exaggeration to say that macroeconomics has progressed in a dialectic fashion; the logic of intertemporal decision making is fully instantiated in the current generation of recent macroeconomic models, as the post-1970 approach insisted on, but the assumptions of complete markets, instanta-

10. Here I am excluding the calibration of growth models from what is meant by empirical work. In my view, calibration is better thought of as a quantitative form of theory. While I believe most calibration advocates would be comfortable with the latter description, many would dispute my claim that calibration analysis is not a form of empirical analysis.
neous price adjustment, and the degree of information available to individual agents as to the correct structure of the economy have all been challenged, as has the ability of a representative agent model to fully capture the sources of aggregate fluctuations. While there is no consensus in macroeconomics with respect to these substantive assumptions, it seems clear that there is less disagreement than there was, say, twenty-five years ago. And I believe that this represents very clear progress in macroeconomic understanding.

The new growth theories, with the important exception of the question of constant versus increasing returns, are based on ideas that are essentially compatible with the substantive economic assumptions of the neoclassical model and with each other. As a result, one does not have the same dialectic process as occurred in the business cycle context. The profusion of new growth theories has in turn produced neither the successful replacement of past theories nor a synthesis across theories. My belief is that the business cycle literature had the critical advantage that the basic issues of contention between schools of thought, be they Keynesian, new Keynesian, new classical, or real business cycle theory, have long been well delineated. The universe of implied assumptions is a relatively well-defined one. As a result, different sets of assumptions are amenable to relatively straightforward empirical assessment. Further, different assumptions are often capable of either synthesis or relaxation. Open-endedness, in contrast, means that growth economics suffers from an abundance of theoretical riches and a shortage of clash between the theories. In light of the desire of the new growth theory to integrate the most and least advanced economies under a common framework, it is unsurprising that so many different theoretical explanations have emerged. In particular, the effort to integrate such disparate societies naturally led to the emergence of growth theories that work on very different time scales; the distinction between fundamental and proximate explanations is perhaps a misnomer, as what distinguishes institutions and savings rates is not that one deserves priority over another but that they move at different frequencies. This expansion of the timescale of admissible explanations itself is a source of open-endedness.

The second reason is empirical: growth theories are not subject to the same degree of critical evaluation as business cycle theories. In the case of business cycle theories, there is general agreement on the objects to be measured as well as (frequently) starkly different implications of theories for data, a classic example of which is the stability of the Phillips curve. In
contrast, growth economics faces first-order measurement problems with respect to the candidate theories that are studied. It is one thing to argue that corruption affects growth; it is quite another to identify an appropriate measure of corruption. While the growth literature is replete with clever measures of the broad theories under debate, as well as clever suggestions for instrumental variables to allow for identification of causal effects, one has no consensus on the appropriate objects of study. Further, in the presence of mutually compatible theories as implied by open-endedness, the empirical distinctions between theories are blurred by the multicollinearity of their empirical analogues and the similarity of their empirical implications.

Open-endedness and measurement limitations do not necessarily condemn growth economics to a lack of progress. This is where growth regressions can play a role. By identifying appropriate stylized facts with respect to the heterogeneity of growth experiences, they can provide a basis for theory comparison. The problem with current growth regressions approaches is that while they have appropriately moved away from strong structural claims, they continue to focus on efforts to run horse races between theories, rather than the use of regressions to identify salient and economically significant data patterns. In this instantiation, they have lost what would seem to be a more fundamental objective of empirical work: construction of a baseline set of empirical facts for which growth theories are supposed to provide understanding. This interplay occurred over the course of the development of the neoclassical literature. When Solow suggests that the embedding of a growth model in an intertemporally optimizing framework has little value, I believe this is best understood as meaning that there is little gained in terms of understanding cross-country growth differences; the role of increasing returns in generating increasing inequality is qualitatively similar when appended to Solow’s model or embedded in a fully developed general equilibrium framework. But this is a statement about the stylized facts that are focused on; a richer set of stylized facts may lead to different conclusions.

Of course, to declare the need for the new growth economics to produce a new set of stylized facts is trivial compared with producing them. Some work, such as Danny Quah’s (1996) development of the “twin peaks” description of the cross-country income distribution, has successfully engaged in this task. My hope is that the present article communicates its importance and indicates how cross-country regressions can play a constructive role in growth analysis.
References


