

INTRODUCTION: SEARCHING FOR THE SECRETS OF GROWTH

NOTA DE LOS EDITORES

Revista de Análisis Económico agradece la valiosa contribución del Editor Invitado, Sr. William Easterly, en la materialización de este número especial.

El contenido del presente número de la *Revista de Análisis Económico* será presentado en el Seminario Internacional "Políticas Nacionales y Crecimiento Económico", que se llevará a cabo en Buenos Aires, Argentina, los días 12 y 13 de agosto de 1993, organizado por el Centro de Estudios Macroeconómicos de Argentina (CEMA), con la colaboración del Banco Central de Argentina.

NOTE OF THE EDITORS

Revista de Análisis Económico appreciates the valuable contribution of the Invited Editor, Dr. William Easterly, to this special issue.

The contents of this current number of *Revista de Análisis Económico* will be presented at the International Seminar "National Policies and Economic Growth", which will take place in Buenos Aires, Argentina, on August 12-13, 1993. The Seminar will be hosted by Centro de Estudios Macroeconómicos de Argentina (CEMA) and sponsored by the Central Bank of Argentina.

WILLIAM EASTERLY
World Bank

In 1870, Argentina had twice the per capita income of Japan. Argentina was considered a developed country and Japan an underdeveloped one. As recently as 1960, the incomes of the two were the same. Today, Japan has three times the income of Argentina (Figure 1). Japan is now considered a developed nation. Argentina is called "developing", although even that may be optimistic as it has had zero per capita growth for the past two decades.

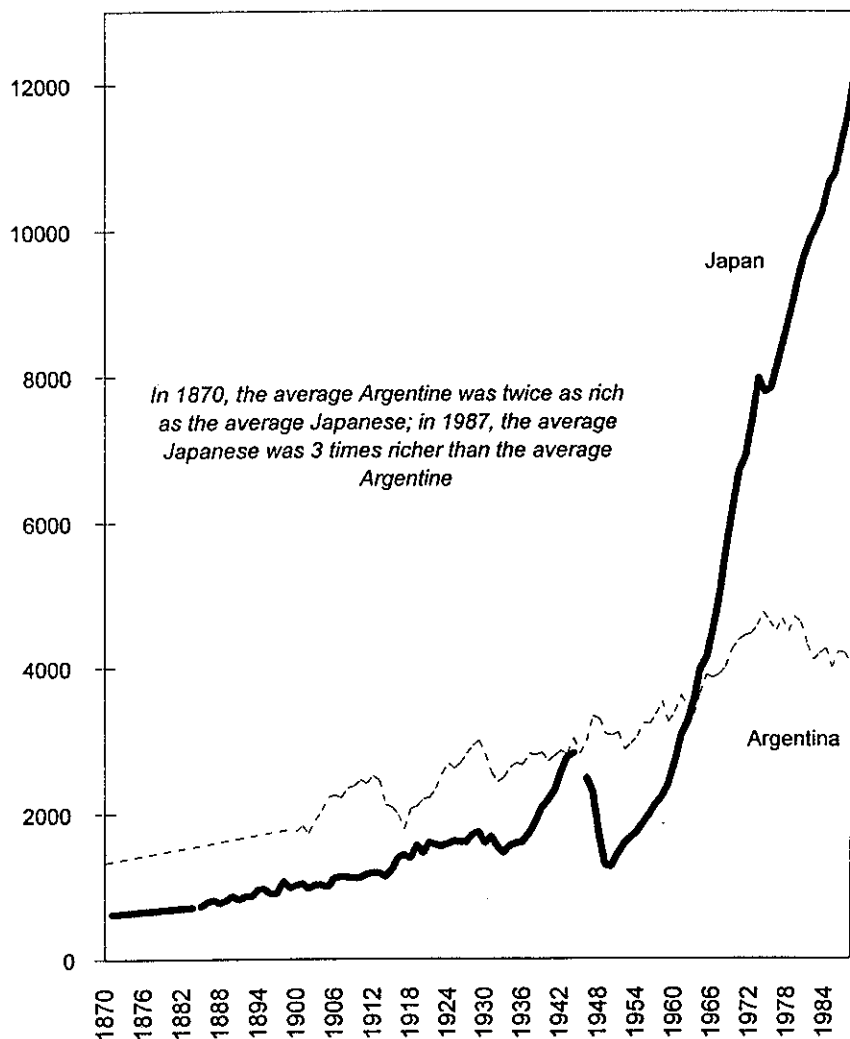
Are the reasons obvious for Argentina's failure relative to Japan's success? Here is one suggested explanation by an observer before 1870: "wealthy we do not think it will ever become: the advantages conferred by nature... and the love of indolence and pleasure of the people themselves forbid it"¹. Unfortunately, this prediction was made not about Argentina but about Japan.

What explains such surprising outcomes? The search for the secrets to national prosperity has tantalized economists at least since the publication of the *Wealth of Nations*. In recent years, the path-breaking work of Romer (1986) has set in motion another wave of intense interest in economic growth and its determinants, following upon the earlier waves of Solow (1957), Harrod and Domar, John Stuart Mill, Adam Smith, etc. When one sees the wonder of Japan and the frustration of Argentina, one can sympathize with the comment of Lucas (1988) that "once one starts to think about [these issues], it is hard to think about anything else".

Since the Japan Miracle, further miracles have been sighted in Hong Kong, Korea, Singapore, and Taiwan. Miracles in Malaysia, Thailand, China, and Indonesia are suspected but not yet completely confirmed. The development faithful have flocked to the shrine of East Asia to discern the secrets behind these visitations, and to pray that they be repeated elsewhere. The pilgrims' prayers have not been answered: much of Latin America and Africa has been close to zero per capita growth over the past decade or two.

Are the East Asian successes truly replicable elsewhere? The miracle metaphor is inherently pessimistic: we have only heard of water being turned into wine *once*. Are

FIGURE 1

PER CAPITA INCOME IN ARGENTINA AND JAPAN,
1870-1988

Sources: Summers and Heston (1991), Maddison (1989) and others (see description in Easterly and Rebelo (1993)).

the East Asian countries successful because of well-known fundamentals that other countries can copy? Or are they successful because of their own unique characteristics? Is it as Lucas (1993) quipped, that "simply advising a society to 'follow the Korea model' is a little like advising an aspiring basketball player to 'follow the Michael Jordan model'?"

The new growth theory has not answered these questions but it has enriched the set of possible answers. We can classify the answers into two broad categories:

(1) "Luck": Some theories in the new growth literature predict multiple equilibria and sensitivity to initial conditions. In such models, accidents of history and other idiosyncracies have a big effect on a country's future. This is the old idea of virtuous and vicious circles, which played a big role in the early development literature. A favorable shock could start a country off on a self-perpetuating cycle of success breeding success.

(2) "Fundamentals": Other new growth models predict that fundamentals like undistorted prices, low taxes, and nonrepressed financial markets (which in the old neoclassical growth model would have had effects on the *level* of income) will have strong effects on the *growth* of income, and thus largely determine a country's future.

Let us look first at the multiple equilibria models. For example, Krugman's (1991) geography models predict that direct foreign investment will flow to where capital already exists, because the new capital can share the services of fixed-cost services like infrastructure and specialized consultants, not to mention benefit from the size of the market created by investments already made. Murphy, Shleifer, and Vishny (1989) similarly theorize that firms will only adopt high-productivity technologies (which also involve large fixed costs) if other firms do so, because only then will the market be large enough to justify the investment in advanced technology. As they note, this is the famous "Big Push" idea of Rosenstein-Rodan (1943) –successful development requires a coordinated advance on all fronts.

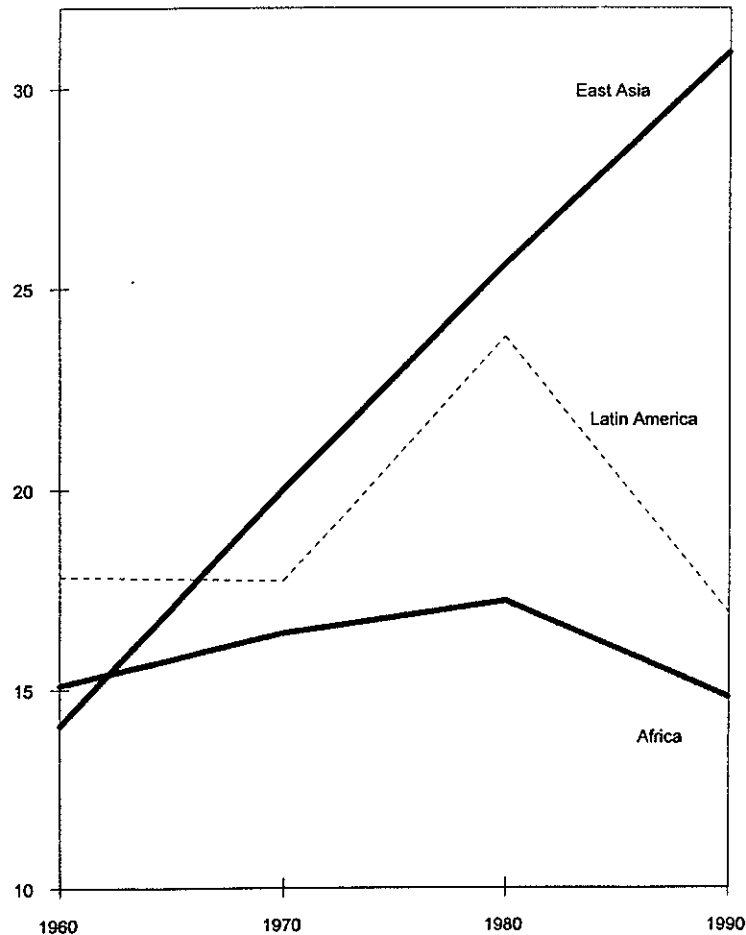
It was exactly such ideas that inspired early mistaken industrialization attempts –most notably the import-substituting industrialization of Latin America in the 50s and 60s. An attempt to create virtuous circles resulted instead in vicious ones. Why did this happen? Early development theory underestimated the difficulty of starting the virtuous circle. The required offensive is bigger than one thinks, and, according to the theory, failure to advance on a single front could still lead to defeat.

Many early development attempts have a sort of farcical quality, like the attempt of Flint Michigan to offset the decline of the auto industry in the 1980s by building an extremely unsuccessful cold-weather amusement park called "AutoWorld"². The model of Kremer (1993) suggests that the development chain will snap if any of the links are weak. Success is so rare, according to this view, because *any* mistake will sabotage the whole development effort. You have to have good initial conditions, and good human capital, and favorable market opportunities, and political stability, and not tax too heavily, and not mess up prices too badly, and have some good luck.

The virtuous circle idea also points out that good country characteristics for growth themselves may be partly endogenous. The most obvious is investment/saving where casual observation and econometric evidence suggests that high growth causes high investment at least as much as high investment causes high growth. Figure 2 shows data from Nehru and Dhareshwar (this issue) on the rise in investment in East Asia compared to Latin America's stagnant and erratic investment and Africa's falling capital accumulation. East Asia had the lowest investment of the three prior to its spectacular takeoff in the 60s. Casual inspection certainly suggests that investment

FIGURE 2

INVESTMENT TO GDP RATIOS, REGIONAL MEDIANS



Source: Nehru and Dhareshwar (this issue).

followed, rather than triggered, growth. Carroll, Weil and Summers (1993) provide formal evidence that growth precedes saving rather than the other way around. Even policies may have large feedbacks from growth rates: successful growth may make it easier to start and continue opening of the economy to foreign competition (see Krueger (1992) for related ideas).

While theoretically exciting, the virtuous circles models make it extremely difficult to do empirical research on growth. Many authors have criticized the empirical growth literature for the endogeneity of the right-hand side variables (see *e.g.*, Srinivasan (1993))³.

The models also would imply that growth will have a large unpredictable element because of its sensitivity to initial conditions. While the unpredictability of growth has been abundantly confirmed⁴, ignorance is not a very convincing proof of a theory. Another field where theory predicts multiple equilibria and sensitivity to initial conditions is meteorology. Empirical practitioners in that field –weathermen– are justly ridiculed for the inaccuracy of their forecasts. Economists trying to explain growth would feel right at home in the company of weathermen.

In other models in the new growth literature, “fundamentals” do indeed play a potent role –indeed a much more important role than in old growth theories. The second set of theories would allow countries to follow proven recipes for success. Worries about endogeneity aside, a huge amount of cross-section and case study work presents suggestive correlations between high growth and such fundamentals as high education, financial depth, macro stability, infrastructure, and outward orientation.

Despite intensive empirical investigation, we still have little information on which view of the world is correct. Did Japan succeed because it followed the Three S’s: Save your income, School your kids, and Send your goods abroad? Or was it lucky because it got a big boost from the Korean War at a critical moment, because it fortuitously chose exports during one of the great trade booms of world history (while its potential competitors foolishly chose import substitution –by the time they discovered outward orientation, the happy days of booming trade were over).

A simple graphic representation of the two views is shown in figure 3. The “luck” view is shown in figure 3a. The marginal product of “capital” (including human capital) is rising because the return to new capital is higher the more economy-wide capital already exists, as in models like Krugman (1991), Kremer (1993), Murphy, Shleifer, and Vishny (1989), and Becker, Murphy, and Tamura (1991). Starting from any point to the left of the intersection of the marginal product of capital (MPK) curve with the discount rate (shown as point A in the graph), each household will have an incentive to decumulate capital, because the discount rate is higher than the return to capital. Decreasing capital will make its return even lower than the discount rate and cause it to decline even faster. It’s clear we are in one of the dreaded vicious circles here. The downward spiral will not stop until households reach zero capital (where presumably some natural endowment of labor-skills will allow the population to avoid starvation). Individual households cannot make the jump from the declining to the growing region because the rising marginal product depends on an economy-wide externality to the *average* level of the capital, and each household is too small to move the average in isolation.

On the other hand, starting to the right of A, households will want to accumulate capital because the return is higher than the discount rate. As capital rises, its return rises and one will want to accumulate even faster –we are in a “take-off” here. Moreover, once the plane takes off it will never land –as long as the return to capital stays high the incentive to accumulate capital will continue forever. A country’s “luck” in starting off on one side of A or the other will determine its future. Moreover, random shocks could move a country from one side to the other, converting success to failure and failure to success.

The contrary view is shown in figure 3b. Here the returns to capital are constant, which makes endogenous growth feasible (Rebelo (1991)). If a policy like an income tax lowers the return to capital to the household, then the growth rate will be permanently lower; growth could even become negative if the curve is shifted below the discount rate line. The starting place –“luck”– has nothing to do with the outcome. If we assume for convenience that the intertemporal elasticity of substitution for consumption is equal to one, then the distance between the marginal product of capital line and the discount rate line gives the growth rate of the economy.

The two views –luck versus fundamentals– are not absolute contradictions. Even in the increasing returns world in figure 3a, the fundamentals can influence the outcome. A tax rate cut (or even a subsidy to capital accumulation) would shift the critical point between decline and growth to the left (from A to B) –making it easier to be on the “right” side of this critical point⁵. In fact, there will always be a large enough policy improvement to shift the critical point to the left of wherever one starts. Presumably such rewarding policy improvements are not initiated because of lack of information, lack of sufficient resources (if take-off requires subsidies), credit constraints, and political economy constraints.

The same analysis would also imply that analyzing a country’s policies will have *some* predictive power even if there are multiple equilibria. A country with more favorable policies, like a lower tax rate, would be more likely to have the critical point farther to the left, and so we would predict that such a country is more *likely* to take off. Even if we ridicule weathermen because they are frequently wrong, it is still useful for them to tell us that there is a 70 percent chance of rain. Multiple equilibrium models suggest that economists will frequently be wrong, but they do not rule out some useful amount of predictive ability.

In this special issue of *Revista de Análisis Económico*, we have seven provocative and representative contributions from this new literature on economic growth. In the next section, I will summarize the articles and relate them to the broader literature. I will look first at the two empirical papers in this issue, and then summarize the five theoretical papers. I will close with some thoughts about possible new directions for research on growth.

Empirical papers

Corbo and Rojas (this issue) analyze the determinants of growth in Latin America in a pooled sample of successive 5-year periods from 1960 to 1988. Along with the usual determinants (investment, primary school enrollment, government expenditure, the black market premium), they highlight two other types of factors that are plausibly critical in Latin America: macroeconomic stability and terms of trade changes. Macroeconomic stability is represented by the inflation rate and the trade deficit, both of which significantly affect growth. The growth rate in the terms of trade also has a strong effect on growth. Both of these factors could help explain the disasters of the 80s in Latin America.

Corbo and Rojas also explore how country characteristics affect investment. They find high and significant coefficients on similar types of variables as in the growth regression. One of their important conclusions is that growth itself strongly affects investment. As Corbo and Rojas imply, it seems likely that high investment is more like one of the facts to be explained rather than an explanatory variable (Lucas (1993)).

FIGURE 3A

GROWTH DEPENDENT ON INITIAL CONDITIONS WITH INCREASING RETURNS TO CAPITAL

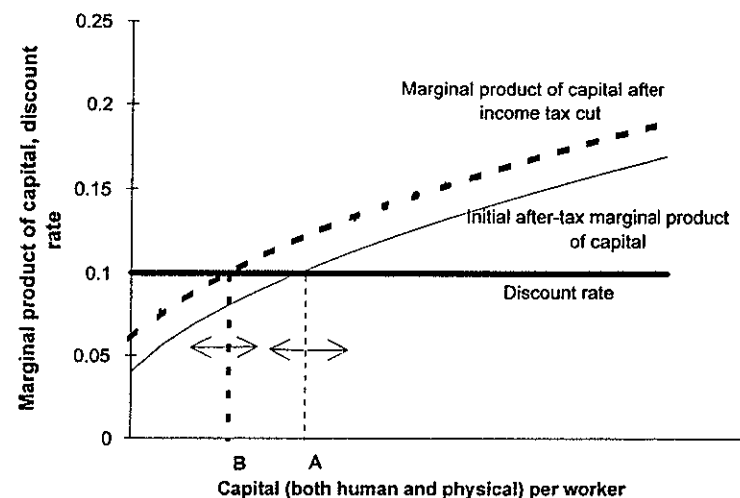
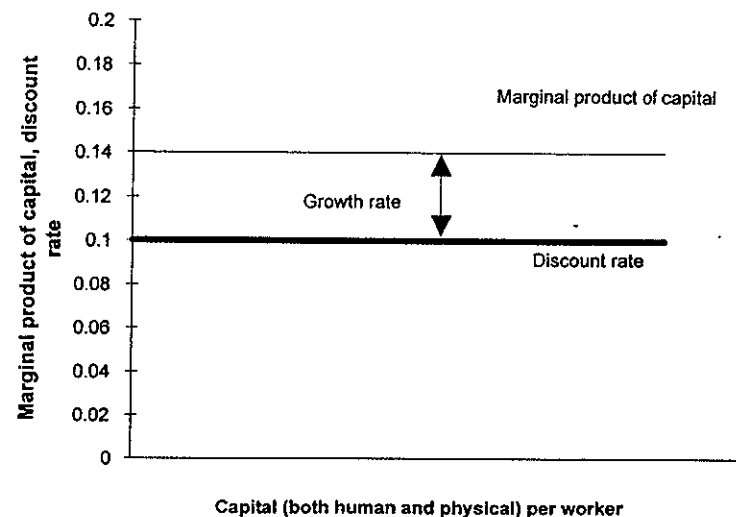


FIGURE 3B

GROWTH DEPENDENT ON FUNDAMENTALS WITH CONSTANT RETURNS TO CAPITAL



Nehru and Dhareshwar's (this issue) detailed construction of capital stock numbers show that capital accumulation played the central role in the growth miracles in East Asia. Over 1960-90, total factor productivity growth was about the same in high income countries (1.1 percent per annum) as in East Asia (1.2 percent). The exceptional growth of East Asia is mostly explained by its exceptional capital per worker growth of 5.1 percent per annum. (Young (1992) argues that capital accumulation accounts for ALL of the per worker growth in Singapore.) Only *three* developing countries (China, Kenya, and Mauritius) had TFP growth account for more output growth than was accounted for by capital stock growth.

How do we jointly interpret these two apparent facts of Nehru and Dhareshwar—that (1) capital growth is endogenous, and (2) capital accounts for most of growth in a growth accounting sense? On one hand, we see that the mystery of East Asian success is at least partly solved—it was old-fashioned capital investment. But if investment is endogenous, the mystery is only deepened: why was investment so high? What set in motion the virtuous circle whereby investment raised growth and growth raised investment?

Another interesting fact highlighted by Nehru and Dhareshwar is that TFP growth is highly unstable over time. The correlation across countries of TFP growth in 1960-73 with TFP growth in 1973-90 is only 0.13 (see Figure 4)⁶. Only China and tiny Cyprus are striking outliers in both periods. Economies like Zaire, Jamaica, Guatemala, Mexico, and Cote d'Ivoire had high productivity growth in 1960-73, but fell to negative productivity growth in 1973-90. Latin American countries in general had enjoyed relatively high TFP growth in 1960-73—in fact it was higher than East Asia over that period! Easterly, Kremer, Pritchett, and Summers (1993) similarly point out that the persistence of per capita growth rates is low, and that this low persistence is not explained by changes in country policies. The implication is that *permanent* differences between countries are relatively small compared to the enormous changes in growth rates from one decade to the next. This finding throws cold water on any notion that growth is mostly explained by deeply rooted country characteristics, like institutions, quality of government, or culture. The unpredictability of growth rates is again confirmed; it's easy to see why prediction is so difficult if there is little basis to extrapolate from past performance.

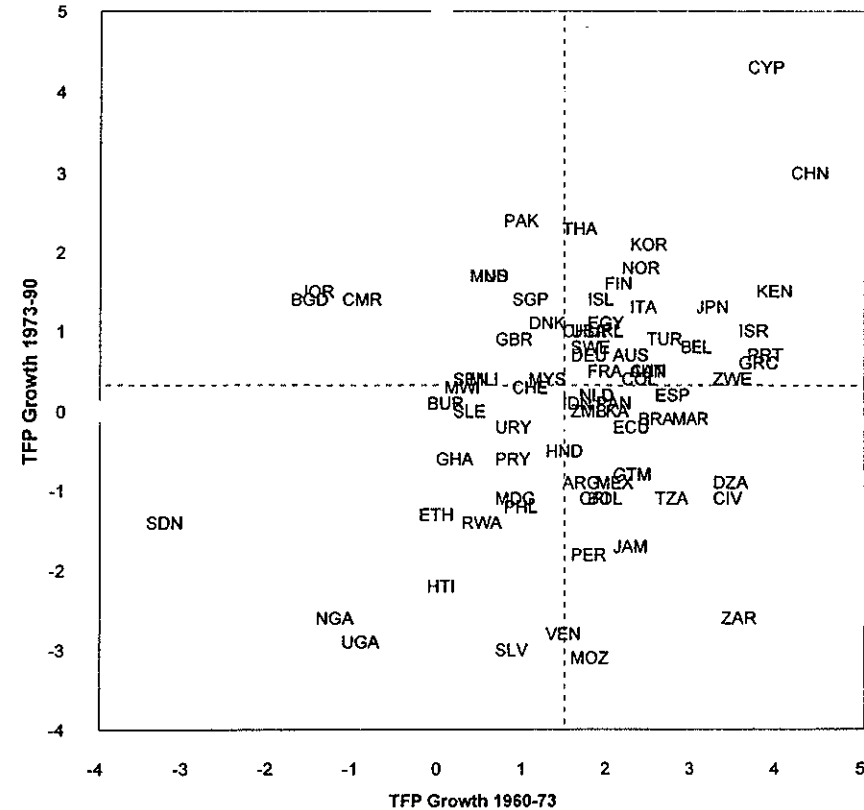
Theoretical papers

The state of the "new growth theory" is much more advanced than of the "new growth empirics". This is not the fault of the empirical researchers—the data to test the richness of the new theories is simply not there, and so researchers are forced to resort to very indirect measures of the variables of interest. And once again, some of the new theories imply that predicting growth will be difficult⁷.

Several of the theoretical papers in this volume feature multiple equilibria. One of the simplest and most elegant is that of *Mantel* (this issue). While much of the focus of the new growth literature has been on nonconvexities in the returns to capital, Mantel suggests that variations in the rate of intertemporal preference (the discount rate) are at least as interesting⁸. Mantel establishes the conditions under which the consumer's intertemporal problem is still well defined with a variable rate of time preference. He then shows that this variable rate of preference can lead to multiple equilibria. Specifically, having an initial capital stock that is too low could lead a country to perpetually decline, while a higher initial capital stock would lead to perpetual growth.

FIGURE 4

PERSISTENCE OF TFP GROWTH PERFORMANCE



Note: Dotted lines show median TFP growth in each period.
Source: Nehru and Dhareshwar (this issue).

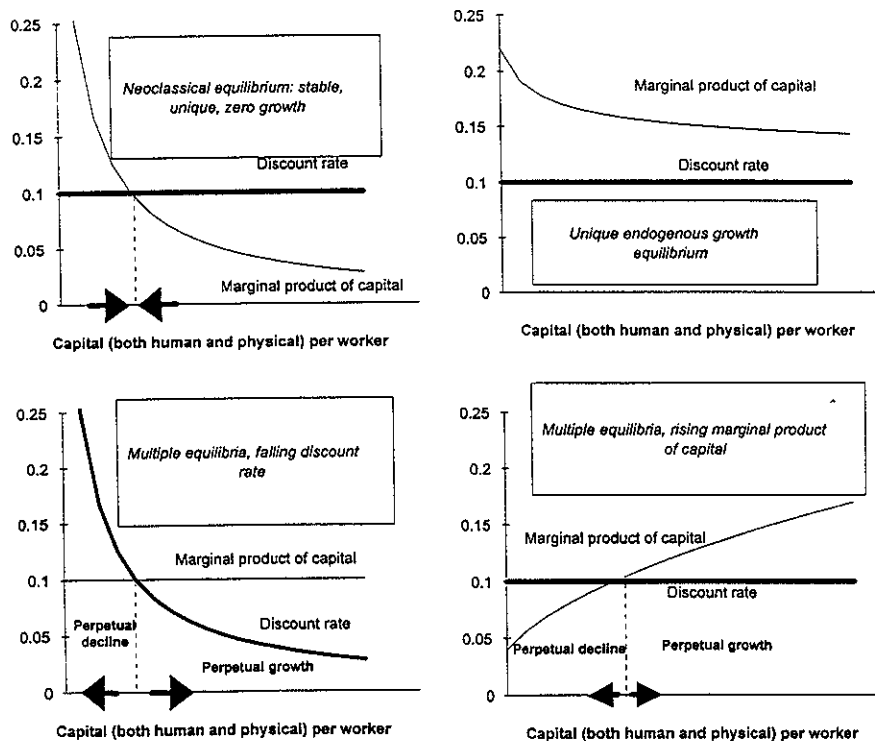
Figure 5 shows the contrast between Mantel's mechanism and that of others in the literature (including the alternatives shown earlier in Figure 3). The neoclassical model (upper left-hand graph) predicts a stable steady state of fixed capital per worker, because the marginal product of capital curve cuts the fixed discount rate from above. The neoclassical endogenous growth model in the upper right-hand panel (like Rebelo (1991) and Jones and Manuelli (1991)) predicts sustained growth because the return to human and physical capital never falls below the discount rate (in Rebelo (1991) it is simply constant at a level above the discount rate; the declining return to capital here is from Jones and Manuelli (1990)). The lower right-hand panel shows the generic

increasing returns models described in Figure 3 earlier: there is a steady state of zero growth where the return on capital is equal to the discount rate, but it is not stable. An economy will rise or fall depending on where it starts. Exactly the same thing will occur with the mechanism of falling discount rates in Mantel's model, one version of which is displayed in the lower left-hand panel in Figure 5. The rate of return to capital is now assumed to be constant (a la Rebelo (1991)), but the discount rate falls as in Mantel. As Mantel discusses, a falling rate of discount with higher capital is plausible if we think that the rich save more. Even with constant returns to capital, we get multiple equilibria because of the falling discount rate. The steady state where the return to capital equals the discount rate is unstable, just as in the model with a constant discount rate and increasing rate of return to capital.

De Gregorio (this issue) has a simple and insightful example of a model with increasing returns to capital. The return to education rises with the existing stock of human capital because there is an externality from the average human capital level of the society to the productivity of each individual's human capital. The externality is

FIGURE 5

EXOGENOUS AND ENDOGENOUS GROWTH, ALTERNATIVE SCENARIOS



sufficiently powerful to generate increasing returns (this mechanism is the most common one in the theoretical growth literature to generate increasing returns and endogenous growth—it is essentially identical to the increasing returns for physical capital in Romer's (1986) original endogenous growth article). The model implies coordination externalities and multiple equilibria. At a low human capital equilibrium, there will be no incentive to accumulate human capital because no one else has it and its returns are low. At a high equilibrium, the returns are high because others also have high education. Human capital will be accumulated forever, generating sustained growth. Which equilibrium will hold depends on initial conditions (see the lower right-hand panel of Figure 5 again).

In this multiple equilibrium context, De Gregorio shows how a market imperfection like liquidity constraints could cause the economy to get stuck in the lower equilibrium⁹. In fact, the lower stagnant equilibrium could become the *only* equilibrium with credit constraints. The intuition is that households are unable to borrow against their future human capital to finance their education, making the jump to the higher (more desirable) equilibrium impossible.

Fischer and Serra (this issue) discuss a similar human capital externality to De Gregorio's. In their model, there is also a spillover from the average level of education in a society to the returns to individual human capital accumulation. However, their focus is on the effect of human capital externalities on inequality within a society. In their model, the poor benefit more than the rich from the spillover from the average education level. The poor therefore have a stronger incentive to accumulate more human capital than the rich; it follows that the poor will tend to catch up to the rich¹⁰.

The intuition behind this result can be illustrated with the old real estate adage "don't have the best house in the neighborhood". There are strong spillovers from the quality of one's house to the property values of the neighbors' houses. The individual with the best house in the neighborhood does not have much incentive to improve her house further, because the value of her house will not increase proportionally to the improvement; it will be held down by the lower average quality of her neighbors' houses. The individual with the worst house in the neighborhood, on the other hand, will capture high returns by upgrading his house to the neighborhood average. Such incentives usually guarantee that house quality within neighborhoods is relatively homogeneous. The insight of Fischer and Serra is that the human capital externality creates similar incentives for convergence within societies. De Gregorio's credit constraints may help explain why this convergence does not occur as strongly as Fischer and Serra's model predicts.

Levine (this issue) develops other details of the case for financial obstacles to development. He sets out a model in which there are spillovers within firms; specifically, benefits from investment by one firm member to the rest of the firm. However, a negative shock could force a firm member to prematurely liquidate her investment in the firm, resulting in the loss of returns on her investment not only to her, but also to the rest of the firm. Developed financial markets would allow the individual to insure herself against negative shocks *without* having to liquidate her investment. The average returns to capital in firms in economies with developed financial markets should therefore be higher. In addition, well-developed financial intermediaries will provide services of project identification and mobilization of capital for "good" projects, which again suggests that economies with advanced financial intermediaries will have higher returns to capital, and grow faster than economies with more primitive financial intermediaries. There is only one catch: there are fixed costs to the formation

of financial intermediaries that can do all these good things. Economies that are poor will have trouble affording good financial intermediaries. The quality and quantity of financial intermediation will rise with per capita income. The natural implication is that economies could get stuck at low levels of income with poor financial intermediation and poor returns to capital, while advanced economies will benefit from a virtuous circle of high income and high quality financial intermediation.

Azariadis and Drazen (this issue) examine another interesting type of endogenous growth: endogenous *population* growth. Their analysis convinces one that population growth is yet another right-hand side variable in growth regressions that really should itself be a dependent variable. In their model, there is a rural agricultural sector and an urban industrial sector. In the rural sector, financial and labor markets are absent. The family unit takes the place of the missing markets: children are a means of saving for old age, and are also a means of obtaining labor for agricultural production. The division of output between parent and child is determined not by altruism, but by the relative bargaining position of the two. Fertility decisions by parents depend on the economic returns as well as the utility of having children. The process of industrialization erodes the economic centrality of the family unit and lowers fertility incentives. Specifically, industrialization creates opportunities for the children of rural families, because they now could migrate to the city to take better-paying industrial jobs. This raises the bargaining power of children and so raises their share of the family output. The typical response of enterprises to higher labor costs is to cut back on labor; in the family enterprise, cutting back on labor means having less children. Higher urban wages causes rural fertility to decline. The Azariadis-Drazen model also predicts that fertility in the urban sector will be below rural fertility because impersonal labor markets replace family labor markets. Migration from the rural to the urban sector thus has an additional, negative composition effect on fertility.

These predictions conform to the stylized facts of industrialization experiences. It has always been well understood that industrial development led to rising labor costs in agriculture, inducing the shift from agriculture to industry. The insight of Azariadis and Drazen is that these rising labor costs reached within the family unit itself. Not only did industrialization price agriculture out of the labor market, but it also priced the rural family out of having children.

Possible new directions for research

Has the new wave of growth research already crested? Some fatigue has already set in with the multiplicity of models with multiple equilibria. Many doubt that more can be learned from yet more cross-section regressions with the existing set of cross-country data from Summers and Heston (1991), Barro (1991), Levine and Renelt (1992) and others.

Yet promising new mines of theory and empirical work have barely been scratched. One is the work started by Krugman (1991) on economic geography. His models of geographic spillovers predict that clusters of economic prosperity and poverty will form. This sounds at least casually like what has happened in Africa (poverty) and East Asia (prosperity). Yet little research on economic geography has been done¹.

One related area where some research has begun is on the economics of cities and regions (see for example Glaeser, Kallal, Scheinkman, and Shleifer (1992)). Many of

the agglomeration externalities that are central to the new growth literature would operate powerfully at the level of a city (see Lucas (1988) for an argument along these lines). In any case, analysis of development successes would surely be enriched if we looked at other levels of aggregation than just nations. National aggregates may conceal much interesting variation. For example, Anaheim, California had per capita output growth of 6 percent in the 1950s, when per capita growth in the US was barely above 1 percent². We get excited about a city success when it happens to coincide with a nation, like Singapore, but we may be missing many interesting examples of success and failure within countries.

Most of the models in the new literature on growth (including all of those in this issue) deal with closed economies. Promising ideas for open economies by Romer (1990, 1993) and Grossman and Helpman (1991) have not been pursued much empirically.

For example, Romer's (1990, 1993) work on the nonrivalrous character of ideas has important implications for the worldwide movement toward integration of countries. Romer suggests that the special character of ideas is that they can be applied an infinite number of times at zero marginal cost. This is not the same as saying that others can use them for free. The "owners" of ideas may be quite successful at excluding others from using them without paying, either through legal protection, through secrecy, or simply because they understand them better than anybody else. When two or more countries are joined in a common market, Romer's theory would predict that owners of "ideas" will realize large gains from gaining access to a larger scale for their operations.

The traditional neoclassical idea is that integrating economies will hurt the relatively scarce factor in each economy. For example, the neoclassical view is that integrating labor-rich Chile with the labor-scarce U.S. would hurt labor in the U.S., worsening the U.S. distribution of income between factors (although there has been little empirical support for this prediction). The Romer model's prediction is that income distribution will worsen even more than the neoclassical model predicts, because there will also be a shift in the U.S. in favor of human capital away from unskilled labor because of the scale benefits to owners of ideas. The Romer model would confirm the prediction of Robert Reich (1991) that the ever-more-integrated economic future belongs to manipulators of ideas.

More attention to economic geography and to the consequences of increased economic integration are only two of numerous directions that growth research could take. The papers in this volume communicate the excitement at what research on economic growth has already achieved. But part of the excitement is that the more research done on economic success, the more elusive and complex seem to be the secrets to that success.

Notes:

- ¹ Quoted in Lipton and Sachs (1992).
- ² One of the author's first trips as a development economist was to Kingston, Jamaica in the early 80s, staying in a gigantic government-built hotel complex on the Kingston waterfront, where the only guests at the height of the tourist season were the author, his 4 fellow consultants, and a few thousand mosquitoes.
- ³ For one example of endogeneity of right-hand side variables, see the regression of the level of per capita income on the investment rate and population growth in the otherwise beautifully-reasoned paper

- of Mankiw, Romer, and Weil (1992). Is Haiti's income low because its saving rate is low and its fertility is high? Or is Haiti's saving low and its fertility high because it is poor? The latter seems at least as plausible as the former.
- 4 Besides the misprediction of Japan's potential in the second paragraph, see also Easterly, Kremer, Pritchett, Summers (1993) and Easterly (1993) for other famous mistaken forecasts. Mainstream Western economists were pessimistic about such later successes as Korea, Hong Kong, Taiwan, Singapore and Mauritius in the 60s, while they were optimistic about India, Sri Lanka, Argentina, and most African economies.
 - 5 Empirical evidence seems to show both that policies have strong effects, on growth *and* that there is a large random element in growth – growth is very unstable over time even though policies are stable. See Easterly, Kremer, Pritchett, and Summers (1993).
 - 6 Figure 4 omits Iran and Iraq, which are outliers for obvious reasons. Including them would make TFP growth even less persistent in the figure.
 - 7 This is not the same as saying the theory is nontestable, which would simply make it useless. Most of the new growth theories do yield sharp testable predictions even when they predict multiple equilibria and instability of growth rates.
 - 8 There was a rich literature on variable rates of time preference in the 1960s, but this literature has not been exploited much in the context of the new growth literature, even though it is closely akin in spirit. This justifies Mantel's quip that the new growth theory is like "Grandma's dress" that goes out of fashion and then comes in again.
 - 9 The new growth literature has revived the concept of the "poverty trap" that was prominent in the earlier development literature (Nelson (1956)). De Gregorio's result is akin to that of Becker, Murphy, and Tamura (1990), who in turn have a mechanism similar to that of Nelson.
 - 10 Fischer and Serra show that the convergence result also requires some conditions on the shape of the externality to human capital function. Also, note that their model implies *constant* rather than *increasing* returns to human capital, and so does not generate multiple equilibria like de Gregorio's.
 - 11 An interesting exception is Chua (1993), who found that one's growth rate depends not only on one's own characteristics but also those of one's neighbors. De Long and Summers (1991) tested for spatial correlation among residuals in growth regressions, but found no evidence for it.
 - 12 As a counterpoint to the earlier Flint, Michigan example, note that Anaheim's success may have had something to do with an amusement park (but this time with a warm climate!): Disneyland was opened in 1955. The figure for Anaheim comes from Greenwood (1981).

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