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What's the Big Idea? The Third Generation of Policies for Economic Growth

Economists and reform minded policymakers in Latin America are asking themselves, and are being asked, hard questions these days. The broad consensus is that two decades of reform have had too little to show for it. Sporadic and sputtering economic growth and stagnant real wages (especially for the unskilled) is not what was expected. This paper puts the Latin American experience in the global context and examines the processes that brought the region to this point. We look at the trends in policy advice on economic growth and how they were formed, and we address the question of what economists can do now to help the region move in restoring economic growth.

The Best and the Brightest

Imagine that you are an American and that it is 1962. You only know what you could have known in 1962. You are called on by an energetic young president to design a program to promote economic growth in Latin America. Perhaps you are a professional economist or perhaps simply an informed, savvy observer of the international and economic arenas.[1] What are the big economic facts of your lifetime that shape your views?

When you were in your thirties, you would have experienced the Great Depression. You would therefore know for a fact that a capitalist economy is unstable: stock markets can and do crash, unregulated banks fail, and unemployment can soar to very high levels and stay there. Whether or not

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1. Like John J. McCloy, who was involved in both the Marshall Plan and the early stages of the World Bank.

you believe activist government responses are effective, you know they are popular—the architect of those policies won four of the first six presidential elections of your life.

You would also have witnessed the amazing ability of a war economy to mobilize production. In 1939 output in the United States was barely above its predepression levels. That year only 355 locomotives were produced, but six years later 3,213 rolled off the assembly line. In 1939, 24,000 truck trailers were delivered, compared with 209,000 in 1944. Between January 1940 and December 1945, the U.S. Army procured 231,099 aircraft (roughly 40,000 a year, from a base of almost none); 88,410 tanks; 46,706 motor carriages for self-propelled weapons; 113,967 other combat vehicles; and over 20 million guns and rifles.[2] No one who lived through World War II could question the idea that governments can plan, mobilize, and direct enormous expansions in economic activity.

You would have seen Russia transformed from a politically and socially backward country convulsed by revolution, civil war, and famine into the Soviet Union, which against all odds grew into an industrial power with military might. It defeated Germany in World War II. Only last year, in 1961, the Soviet Union stunned the United States out of its complacency about being the world's technological leader by putting the first man into space. You would therefore know for a fact that socialism and central planning are capable of rapidly transforming a country from an agrarian backwater into an industrialized superpower.

You also observed the ascendancy of Japan. Although you would have been too young to remember the world's shock at the defeat of the Russians at Port Arthur in 1905, you would have witnessed the rise of Japan to the point at which it could conquer China and nearly all of East Asia. The rapid collapse of the British and Dutch armies would have destroyed any notions of inherent Western superiority. You would be aware that the Japanese can attack and credibly threaten the United States. You know for a fact that with aggressive national leadership, a poor, non-European country can grow very rapidly.

You would have witnessed the dramatic recovery of the industrialized world following an economically devastating war. The countries of Europe, supported by generous aid from the Marshall Plan, launched a

2. The U.S. Army also procured all the other accouterments of equipping an army in the field, such as 23 million helmets, a million watches, and—of particular interest to the author from Idaho—265 million pounds of dehydrated white potatoes (Smith, 1959).

recovery that generated growth rates far higher than those of the U.S. economy or their own prewar levels. From 1950 to 1960, per capita gross domestic product (GDP) grew annually by roughly its historical average in the United States (2.2 percent), but it grew by 8 percent in Germany, 6.2 percent in Austria, 5.6 percent in Italy, and 3.7 percent in France.[3] America's growth looked anemic in comparison. If these differences in growth rates continued for another twenty years, all of these countries would catch or overtake the United States. In 1962, therefore, you would know for a fact that foreign aid had helped Europe recover and rapidly gain on the United States.[4]

You would have seen the world's once leading economy unquestionably eclipsed as an economic superpower. When you were born, the United Kingdom was ranked first in the world in per capita GDP. By 1962, however, the United Kingdom—historically the bastion of free trade and liberal economic policies—was unquestionably falling behind. In a span of sixty years, the United Kingdom had fallen from 12 percent ahead of the United States, 30 percent ahead of the Netherlands, 46 percent ahead of Germany, and 61 percent ahead of France to a position well behind the United States, essentially even with Germany and the Netherlands, and only 15 percent ahead of France. You would know for a fact that free trade and liberal economic policies did not guarantee rapid growth.

If you paid attention to South America, you would know that while many of that continent's economies were open and liberal in the 1930s, they suffered huge shocks during the worldwide depression. Those that devalued relatively early were perceived to have weathered the storm better than those who attempted to follow strict orthodoxy (as argued by Carlos Diaz-Alejandro).[5] The war and its aftermath had produced something of a boom for these economies, with favorable terms of trade and protection of industrial sectors owing to a lack of available imports. The first round of import substitution industrialization was not so much a policy choice as a necessity. World markets in trade and finance were a fickle companion for Latin American countries.

3. The data are from Maddison (1995). These exact numbers would not have been available in 1962, but people did have a sense of the different performances.

4. The Japanese economy was no different. The country had been embargoed and bombed into a complete shambles, yet it took Japan only until 1953 to recover to its prewar (1938) per capita GDP, and by 1960 it had far surpassed that level. U.S. aid played a supporting role here, as well.

5. Diaz-Alejandro (1988).

You would think of the rest of the world—including China, India, other parts of Asia, Africa, and the Middle East—as the Third World, a somewhat undifferentiated mass of poverty and political turmoil. The Cold War would make some places more important than others, but regardless of strategic positioning, a no-longer-relevant colonial past and the absence of a single economic success in these regions would limit the insights they could offer you for designing a development strategy for the 1960s and beyond.

Finally, in 1962 you would not have lived through globalization, but rather would have experienced its reversal. Nearly every aspect of the world, including trade in goods, capital flows, and international migration, was substantially more closed in the early 1960s than it had been fifty years earlier, prior to the First World War. It would be difficult to see globalization as a force for anything, especially economic prosperity.

Big Facts Make for Big Ideas

If facts can determine ideas, then the big facts of your (hypothetical) life in 1962 practically compel three big ideas: government can be the driving force behind rapid industrialization; accumulation is the key to rapid economic growth; and integration into the world is necessary for certain key products, but it is neither necessary nor sufficient for rapid growth.

ROLE OF GOVERNMENT. Governments were the obvious driving force behind economic growth. Moreover, planning was essential—if not central planning, then strong guidance of the economy by the government. Strong planning ministries should be responsible for mobilizing and allocating the scarce capital available for direct investments in the necessary infrastructure of a modern economy. These investments were obviously beyond the capacity of any private investors. Government had to do more than simply mobilize the funds, however. Planning and coordination of the large capital investments were critical for their effectiveness.

ROLE OF ACCUMULATION. Development meant industrialization, and the inputs necessary for industrialization were clear: cement, electricity, modern transport infrastructure, steel, fuels, and chemicals. Establishing these facilities mandated individually large investments requiring long gestation periods. Investment was key. This emphasis on accumulation did not exclude human capital; it just took the same approach to education as

it did to steel. Large investments, spearheaded by the government, were required to create a labor force for industrialization.

ROLE OF TRADE. While some trade was necessary to acquire the inputs that a country lacked, an export orientation per se was neither necessary nor desirable. Exports markets were viewed as both unpromising in the long run and cyclically fickle, and one perceived benefit of rapid industrialization was a reduced reliance on international markets.

ROLE OF FOREIGN CAPITAL. Financial integration and foreign private capital were not considered advantageous. Most investment was through the government (or mobilized by the government). Private-to-public loans presented a danger, and large private-to-private flows were, at best, a nuisance. Since mobilization of domestic savings for investment was key, a proposal for an open capital market would have been a puzzle.

ROLE OF DEVELOPMENT ASSISTANCE. So how do these big ideas affect the advice you will give the president on how to help poorer countries—in Latin America and elsewhere—grow rapidly? After all, a successful set of tactics for development assistance must fit hand in glove with a strategy for economic growth. The two key issues are the nature of policy advice and the structure of assistance.

Your policy recommendations will clearly reflect the big ideas that dominate your world view. Are you going to tell Third World nations to pursue policies of limited government intervention, in which they address only cases of market failure and some targeted equity interventions? To pursue integration with the world economy? To rely on private markets to mobilize investment? Of course not. Only an ideologue completely out of touch with reality would recommend that. You are going to tell them what they, in fact, were told: governments must play a central role; accumulation is the key to rapid growth; and integration into the world economy is a necessary evil, at best.

Once that advice was given, a system of development assistance had to be designed to support the strategy. Since governments are the driving force behind industrialization, resources to governments are crucial. Direct government-to-government lending (or multilateral-to-government lending) is therefore a perfect approach. Similarly, creating investable resources that augment savings is essential to stimulate accumulation. Available foreign exchange can also represent a binding constraint on growth, however, since trade and exports are not particularly emphasized

while imports are needed for the creation of infrastructure and physical capital. The one and only product of a new institution like the World Bank provides exactly what is needed for development: investable foreign exchange made available to governments for development projects. The tactical design of development assistance thus fits perfectly with strategic ideas about development.

The big facts and big ideas prevalent in 1962 suggest optimism and confidence. Industrialization may seem a daunting technical challenge, but you know for a fact that it is not impossible. If Japan and Russia can do it, then there is no reason why China and India, Brazil and Egypt, Indonesia and Nigeria cannot. You know the problem. You know the solution (table 1).

Unfortunately, as the old saying goes, it's not what you don't know that hurts you, but what you do know that ain't so.

Sons of Wise Men: Wised Up

Fast forward. It is August 1982, and you are 52 years old (policymakers are getting younger). Mexico has just shocked the world by defaulting on its external debt. Your reaction to this event will be conditioned by what you have learned from your last twenty years of experience in the world. The interwar years, the Great Depression, and the experience of postwar Europe, which so strongly influenced your predecessor twenty years earlier, no longer stand as relevant models for today's low- and middle-income economies. You look elsewhere for lessons.

TABLE 1. Summary of Big Ideas in Economics, 1962

Area	*Prominent ideas*
Government	Plays a central role; acts as the driving force behind development
Accumulation	Is central to development process; coordination and scale problems require government involvement
Trade and integration	Has no particular advantage beyond the import of capital goods and the purchase of necessary inputs
Foreign capital	FDI is to be avoided, but government borrowing is acceptable, preferably from foreign sources
Development assistance and the role of multilaterals[a]	Provide project-based lending of investable foreign exchange and resources to governments

a. Primarily the International Monetary Fund (IMF), the World Bank, and the Inter-American Development Bank (IDB).

The first big fact is that by 1982, the failures of strong central planning—or at least its lack of universal success—were becoming obvious. While no one imagined that the Soviet Union was as close to collapse as events proved, Khrushchev's boast that "we will bury you" was already ringing hollow. China had clearly failed to develop rapidly under central planning, and the West became increasingly aware of the famines of the Great Leap years and the chaos of the Cultural Revolution. Other experiments with central planning were having similarly disappointing results. Cuba had not lived up to the promise of its 1959 revolution. Vietnam's postunification malaise could no longer be blamed exclusively on the war rather than on postwar economic policies. The slow growth rate in India proved that even soft government planning was no guarantee for high growth. India's per capita GDP grew less than 1 percent; only five countries outside sub-Saharan Africa grew slower.[6]

The second big fact is the persistently rapid growth of countries in East Asia. Japan's growth rate had slowed after the first oil shock in 1973, but the country continued to occupy center stage as one of the world's great examples of rapid industrial growth and technological prowess. And Japan was no longer alone. The original four Mini-Dragons—Hong Kong, Korea, Singapore and Taiwan—continued to grow at a historically unprecedented pace, and they all maintained extremely rapid growth of manufactured exports.

The third big fact is the failure of many countries to recover from the commodity shocks of the 1970s. While the combined boom in oil and commodity prices in the early 1970s effectively hid the structural and policy problems of many countries, the collapse in commodity prices damaged the growth prospects of many commodity exporters. Growth turned to contraction in a number of countries, including Côte d'Ivoire, Jamaica, and Zambia, even before the debt crisis. The collapse was not universal, however. Some Southeast Asian nations, especially Indonesia and Malaysia, weathered the storm, and the East Asian exporters of manufactured goods—all dependent on imported oil—adjusted and recovered with remarkable speed.

The fourth big fact, particularly in Latin America, is the increasingly disappointing performance of import substitution industrialization (ISI). The supposedly easy phase of ISI was over, but for some reason the infants

6. These five were Bangladesh, Haiti, Nicaragua, Papua New Guinea, and Venezuela.

were not growing up. Maintaining the existing industries was requiring more, and more complex, systems of trade regulation and financial support, and the expense left little scope for strategic investments in the next round. The import-dependent, capital-intensive nature of the industries created by ISI raised doubts about the fundamental premise of ISI as a growth strategy.

The fifth big fact is the debt crisis itself, which came as a shock. Brazil and Mexico had seen rapid growth in the 1970s, almost rivaling that of the Mini-Dragons. Brazil grew 5.8 percent from 1964 to 1979, while Mexico registered a slower, but still impressive, 3.6 percent. As growth slowed, however, debate began over whether the slowdown was a temporary shock or whether the Latin approach actually embodied deep problems. The explosion of a debt crisis gave credence to the argument that this was not temporary, but rather indicated a failure of the entire strategy of Big Push import substitution industrialization.

New Big Facts Make for New Big Ideas

The events of 1962 to 1982 suggested that the previous consensus had it exactly backward on almost every dimension of the development strategy. The new conventional wisdom reversed policy recommendations and called for a new role for multilaterals.

ROLE OF GOVERNMENT. The government is no guarantee of success, and it has in fact been the problem as often as it has been the solution. Overly large or excessively interfering governments hamper economic growth. Government failure is more pervasive than market failure.

ROLE OF ACCUMULATION. Accumulation remains important, but the emphasis has shifted from government investments in so-called social overhead capital to the centrality of productive investments by the private sector.

ROLE OF TRADE. Trade is the engine-of-growth. Exports offer not only gains in static efficiency, but dynamic advantages as well. Import competition, or at least the exposure of exporters to international markets, is a vital instrument for disciplining domestic producers.

ROLE OF FOREIGN CAPITAL. Foreign borrowing by governments is dangerous; foreign direct investment (FDI) should be encouraged.

ROLE OF DEVELOPMENT ASSISTANCE. With the reversal of big ideas about the role of government, accumulation, trade, and integration in pro-

moting rapid development, what was previously seen as a hand-in-glove fit between development strategy and development assistance now became a procrustean bed. Giving direct loans to governments in the form of foreign exchange for large projects was the wrong thing to do in every sense. If government is merely the handmaiden of the private sector, then why give loans to the government? Loans from the government to the private sector make no more sense, however; hence, private capital is the only answer. Furthermore, world trade and integration eliminate the foreign exchange constraint, since the right policies and prices can produce exports sufficient for import needs.

By this stage, most Latin American countries have moved beyond the point where development assistance takes the form of concessional transfers, but they remain engaged with the multilaterals (the International Monetary Fund, the World Bank, the Inter-American Development Bank). The shift from project lending to adjustment lending represents much more than a marriage of convenience; it is a match made in heaven between theory and institutional needs: the G-7 needed new money on the table to make debt deals possible so that the G-7-based money center banks could reduce their exposure, while the multilaterals needed immediate disbursements (and for that, projects are irrelevant.) Fortunately, the new strategy of getting prices right called for policy reform that could be justified as an "investment" to legitimize it all.

But in 1982, you are still optimistic and confident. The debt crisis is a temporary set-back, an unexpected shock that is not insurmountable.[7] Drawing on the lessons of experience (and to some extent research), you now know what works (table 2). But as before, what you do know that ain't so will come back to haunt you.

Grandsons of Wise Man: Loss of Certainty

Fast forward again, to 2002. What are the big facts of our lives? We are not talking about the results of the growth regressions with their confusing and conflicting partial associations, but the big facts. First we present the bad news, and then the (puzzling) good news.

7. The *World Development Report 1983* presents a base case forecast of 3.3 percent annual per capita growth in developing countries from 1982 to 1995 (World Bank, 1983).

TABLE 2. The Reversal of Big Ideas: 1962 versus 1982

Area	*Ideas prominent in 1962*	*Ideas prominent in 1982*
Government	Plays a central role; acts as the driving force behind development	Plays a central role, but acts as the main obstacle to development.
Accumulation	Is central to development process; coordination and scale problems require government involvement	Is central to development process; private sector investment is the key.
Trade and integration	Has no particular advantage beyond the import of capital goods and the purchase of necessary inputs	Exports bring dynamic advantages; import competition is necessary for disciplining domestic producers
Foreign capital	FDI is to be avoided, but government borrowing is acceptable, preferably from foreign sources	Government borrowing is to be avoided, but FDI is encouraged
Development assistance and the role of multilaterals[a]	Provide project-based lending of investable foreign exchange and resources to governments	Quick disbursing; policy-based lending to establish conditions for FDI and domestic investment.

a. Primarily the International Monetary Fund (IMF), the World Bank, and the Inter-American Development Bank (IDB).

The first big fact is the enormous slowdown in growth that has occurred throughout the developing world: the so-called lost decade(s) in Latin America has its counterpart on other continents. The median growth rate in low- and middle-income economies fell from 2.5 percent in 1960–79 to 0.0 percent (zero!) in 1980–98.[8] Latin America's rapid, or at least steady, growth disappeared in the 1980s, and it has only returned in fits and starts, punctuated by increasingly severe macroeconomic crises. The so-called Brazilian miracle, which saw that nation poised in the 1970s to become the world's next economic superpower, has turned into the Brazilian mystery, with twenty years of stagnation. Countries such as Argentina, Uruguay, and Venezuela, which were at roughly southern European levels of development by any measure in 1960, have stagnated or worse, and they certainly have not kept pace with Greece, Portugal, or Spain. Most Central American countries were embroiled in more or less open civil wars for two decades. Their emergence from civil war and the reestablishment of order has not led to the hoped-for economic booms. Even good performers like Costa Rica suffered slow economic growth for most of the period. Only Chile, after two deep recessions, has had consistent and steady growth.

The second big fact is that the long-awaited transition from stagnating Marxist central planning to a capitalist economy has gone horrifically

8. Easterly (2001b).

worse than anyone would have dared predict. While the consensus was that the economies would experience a dip in income as they restructured and as resources were reallocated from old to new activities, no one predicted in 1992 that income in many newly capitalist countries in 2002 would be less than half the level under the Communists. This has not been a homogenous experience. Countries with some prior integration with the West (such as the Czech Republic, Poland, and Slovenia) are doing reasonably well, and some countries that never really integrated with either East or West and that have maintained a strong state (namely, Tajikistan and Uzbekistan) are also doing reasonably well. But some have come completely unglued (former Yugoslavia), while a broad swath, including the two most populous states (Russia and Ukraine), have fallen into a deep crack between two systems.

The third big fact is the financial crises—or perhaps the single rolling financial crisis—of the 1990s: Mexico in 1994; Thailand, Korea, and Indonesia in 1997; Russia and Brazil in 1998; Ecuador in 1999; Turkey in 2000; and Argentina today. In each case, something caused a near or actual debt default or a large depreciation (or both). While some countries recovered strongly (Korea, Thailand, and perhaps Mexico) and others went back to at least muddling through (Russia and Turkey), Indonesia remains below its precrisis level and Argentina may not have yet hit bottom.

The fourth big fact of 1982–2002 is the collapse of sub-Saharan Africa, which has by now become so complete as to force itself into world consciousness. Nearly all of sub-Saharan Africa has been transformed from the heady optimism and enormous promise of early independence to almost unspeakable suffering. Many nation-states have descended at least once into chaos: Angola, Burundi, Ethiopia, Liberia, Mozambique, Rwanda, Sierra Leone, Somalia, Sudan, Uganda, and Zaire (now Congo) have all seen periods in which civil order collapsed completely. The largest African state, Nigeria, has alternated between unstable democracy and even more unstable military rule, and it shows little immediate promise. Despite its considerable oil wealth—or perhaps because of it—Nigerians are poorer today on a per capita basis than they were a quarter of a century ago. Nigeria is not alone. The economies of those African states that have escaped the fate of disintegration or civil war have not fared much better. Even relatively politically stable states like Côte d'Ivoire, Ghana, Kenya, Senegal, Tanzania, and Zambia are generally in

worse shape economically than they were twenty, thirty, or in some cases even forty years ago.[9] The political transition of South Africa was remarkably successful, but the country has not proved to be the regional engine of growth that many had hoped for. And now sub-Saharan Africa faces an AIDS crisis for which the only historical analogue is the Black Death. The only relatively consistent success cases—Botswana and Mauritius—do not give much hope for an entire continent. They have less than 3 million people between them, and Mauritius is not even on the continent of Africa. Today, despite its past successes (or maybe because of them), Botswana's HIV infection rate makes it the world leader in declining life expectancy.

The fifth and final big fact is that the world's two most populous countries, India and China, have grown rapidly. This is somewhat puzzling because in many ways these countries are slow, cautious reformers that remain among the more closed and restricted economies in the world. China, the country that proved that central planning could not work, has managed a cautious transition to markets under authoritarian and Communist control, achieving an economic performance that any of the suddenly democratic and capitalist transition countries would envy. India, the country that proved the failure of soft planning and state-led development, as well as the dangers of a strangulating bureaucracy, is increasingly seen as a success. The contrast in the two periods between India and China and the rest of the developing world is amazing. From 1982 to 1999 (when our data end), per capita income in China grew at 5.8 percent (up from 3.2 percent in 1960–81) and in India at 3.6 (up from just 0.08 percent in 1960–81). At the same time, the median growth of all other non-OECD countries over that same period was just 0.6 percent, down from 2.8 percent. Both India and China accelerated by more than 2 percentage points, while the rest of the world decelerated by more than 2 percentage points.

The End of Big Ideas?

The era of adjustment appears to be over. Argentina in 2002 will likely mark the end of the adjustment era, just as Mexico in 1982 marked the end of the government-led, inward-oriented Big Push. The notion that the latest big ideas are right but that they just were not implemented rings increasingly hollow, especially in Latin America. As Easterly shows, nei-

9. Freeman and Lindauer (1999, table 1).

ther a worsening of policies nor even maintenance of the status quo can explain the massive slowdown in growth in 1980–98.[10] Implementation was certainly not perfect (when is it?), but most of the augmented Washington Consensus growth determinants were better after 1980 than before, and yet growth in the developing world was 2.5 percent points slower in the later period.[11] The sporadic episodes of a return to growth (often following deep recessions) that are often cited as evidence that the market-friendly, outward-oriented, private-investment-led strategy is finally working are, in fact, a return to the growth rates most countries enjoyed in the 1960s and 1970s during the era of supposedly bad policies.

But if adjustment and the Washington Consensus are finished, what are the obvious, common sense, big ideas dictated by today's big facts (table 3)? Are there no lessons to be extracted from the failures and successes of the last forty years?[12] Perhaps this is just a false nostalgia for simpler times, but it seems harder than ever to identify the keys to growth. For every example, there is a counter-example. The current nostrum of one size doesn't fit all is not itself a big idea, but a way of expressing the absence of any big ideas.

10. Easterly (2001b).

11. The United Nations Conference on Trade and Development (UNCTAD) reaches a similar conclusion about the 1990s, noting that "in spite of interruptions, and also policy slippages (which have been generally due to problems of meeting fiscal targets), profound policy changes have occurred in countries undertaking SAF/ESAF programmes. The most extensive structural reforms have occurred in the deregulation of pricing and marketing, particularly in the important markets for agricultural products and inputs; the easing of trade barriers, particularly curtailing quantitative restrictions; reform of foreign exchange regimes; and liberalization of interest rates" (UNCTAD, 2000, p. 103). Despite this progress in reforms, improvements in economic performance were slight.

12. A major "white paper" on development assistance recommends the following strategy on aid for developing nations: (a) establish a better partnership, a clearer purpose, and a greater coherence in development aid; (b) increase the volume of aid; (c) meet the problem of mounting debts; (d) make aid administration more effective; (e) redirect technical assistance; (f) revitalize aid to education and research; (g) strengthen the multilateral aid system; (h) create a framework for free and equitable international trade; (i) promote mutually beneficial flows of foreign private investment; and (j) slow the growth of population. This may sound like a report delivered to the UN's recent International Conference on Financing for Development held in Monterey, Mexico, but it isn't. These bulleted points appeared in 1969 in the Pearson Commission Report (Pearson, 1969), written in response to World Bank president George Wood's request for a "a *grand assize* in which an international group of stature and experience would meet together, study the consequences of twenty years of development assistance, assess the results, clarify the errors, and propose the policies which will work better in the future." But they could have been written for Monterey.

TABLE 3. Are There Any Big Ideas Left?

Area	*Ideas prominent in 1962*	*Ideas prominent in 1982*	*Ideas prominent in 2002*
Government	Plays a central role; acts as the driving force behind development	Plays a central role, but acts as the main obstacle to development.	?
Accumulation	Is central to development process; coordination and scale problems require government involvement	Is central to development process; private sector investment is the key.	?
Trade and integration	Has no particular advantage beyond the import of capital goods and the purchase of necessary inputs	Exports bring dynamic advantages; import competition is necessary for disciplining domestic producers.	?
Foreign capital	FDI is to be avoided, but government borrowing is acceptable, preferably from foreign sources	Government borrowing is to be avoided, but FDI is encouraged.	?
Development assistance and the role of multilaterals[a]	Provide project-based lending of investable foreign exchange and resources to governments	Quick disbursing; policy-based lending to establish conditions for FDI and domestic investment.	?

a. Primarily the International Monetary Fund (IMF), the World Bank, and the Inter-American Development Bank (IDB).

The end of big ideas does not mean several things. First, the lack of big ideas is not another reversal of the big ideas. The fact that the transition to capitalism failed (so far) does not vindicate central planning. Countries like China, India, and Vietnam are growing rapidly with policies and institutions that are far from the Washington Consensus, but the boom in China and Vietnam was unquestionably initiated by moves toward liberalization.[13]

Second, the end of big ideas does not mean either "it just doesn't matter" or "anything goes." That countries have experienced episodes of growth with a wide variety of policies does not mean that any set of policies will do. The problem is not that everything seems to work, but that so little seems to work. Lots of countries initiate growth booms, but very few do not end in busts.[14]

Third, the end of big ideas should not mean that you have to get everything right in order to grow. This view maintains that countries have to achieve far more than the ten propositions in Williamson's original (or revised) Washington Consensus.[15] They must also tackle corruption, address inequality, and build credible institutions. In other words, the preconditions to development are already to be like a developed country. The experience of the past four decades rejects this interpretation, however. There may be few cases of success, but they show that getting everything right is not a fair representation of the conditions for rapid growth.

Fourth, the end of big ideas does not mean the end of the debate about what works. Rather, it should allow a less polemic, more nuanced discussion of country cases that is less tinged by the crude filter of big ideas. For instance, because Korea grew so rapidly for so long, any big idea had to encompass Korea before it could become conventional wisdom.[16] This led to long and perhaps not entirely fruitful debates. Was Korea outward oriented or protectionist? Export promotion policy suggested outward oriented, while import protection suggested protectionist. Was Korea government led or market friendly? Examination of the mechanics of

13. It is less clear what initiated the boom in India, since the acceleration in growth clearly predates the 1991 round of liberalizing reforms, which were themselves modest.

14. Pritchett (2000).

15. Williamson (1990, 1997).

16. The other three Mini-Dragons were not essential for validating a big idea. Two were city-states that could easily be dismissed as special cases, and Taiwan became increasingly out of bounds as China asserted itself.

government direction of the economy, government allocation of credit, and promotion of specific industries suggested government led; the use of the private sector (versus parastatal firms or government agencies) as the instrument of investment and the role of business councils suggested market friendly.[17] Was Korea's growth Big Push or private sector and productivity led? This issue sparked generations of debate about Korea's total factor productivity (TFP)—whether it was low, about that of the OECD countries, or fast by cross-country standards. Those who argued that Korea, like the Soviet Union, proved that Big Push accumulation can lead to rapid growth tended to find (or stress) a low TFP, while those who emphasized the private sector role found (or liked) a high TFP. Even when it was agreed that the Korean government intervened in growth, the question arose of whether that intervention was rules based or discretionary. These debates were often less about what Korea actually did than about what label to apply to Korea and then sell to other nations eager to emulate Korea's success.

The desire to interpret specific experiences as universal laws continues today. Since India and China are succeeding and since they are both huge countries, whatever accounts for their success is phenomenally important. Are they globalizers? Are they government led? Are they accumulation driven? By the same token, Argentina is the current big ideas booby prize. If the Argentine collapse is the result of bad government behavior, then globalization will get off with a warning for speeding. If, on the other hand, Argentina's collapse originates in the whims of the electronic herd, despite having followed all of the currently conventional big ideas as best as possible in a nonideal world (who doesn't have politicians?), then globalization looks bad for much of the nonindustrial world.[18]

But as Dani Rodrik suggests in his comments to this paper, striking a balance between "anything goes" and "universal laws" requires recognizing that there are some universal principles about desirable economic policies, but that these principles can be achieved in a number of different ways. The mistake is to confuse the underlying principle with a particular institutional form, which actually represents just one approach to implementing the principle.

17. Of course, Park Chung-Hee's assertion of a new policy direction for Korea, in which nearly all private sector leaders were arrested for profiteering and black marketeering as a means of persuasion, hardly qualifies as friendly (see Haggard, Kim, and Moon, 1995).

18. The term electronic herd is from Friedman (1999).

To return to the challenge of the opening section, suppose that now, in 2002, a young and energetic president of a Latin American country asks you to develop a strategy to promote rapid growth. Any push toward deepening market reforms will be seen as a continuation of the failed strategies of the present, while any push toward strategies that call for government intervention and leadership (or, to use the forbidden words, industrial policy) will be seen as a reversion to the failed strategies of the past. What is of even deeper concern than the lack of an obvious, dominant set of big ideas that command (near) universal acclaim is the scarcity of theory and evidence-based research on which to draw.

The Agenda Ahead

Our intention in writing this paper on big ideas is not to propose a new paradigm for economic growth. At most we encourage some rethinking of what the academic development community should be studying. Nor are we attempting to construct a history of ideas in the usual sense of tracing through what the discipline of economics has said about development. A number of reviews of the evolution of the thinking of development economists (or, more broadly, economists thinking about development) are already available.[19] There are even volumes of self-reflection by those pioneers who created the ideas in the first place, and they know more about what they meant than we do.[20]

What we are attempting is more problematic than a history of economic thought—or rather, the thought of economists. As Richard Eckaus points out in his comments on this paper, we probably slight ideas and theories to stress the influence of facts. We are examining the evolution of the conventional wisdom of the development community, which is a heterogeneous mix of politicians, senior policymakers (such as ministers of finance and planning), professionals in international agencies and foundations, policy-relevant academics (including economists) in their advisory role, and even journalists. The conventional wisdom of earlier times was undoubtedly less unanimous and more conflicted than captured in our simple narrative.[21] We also acknowledge that our theory on exactly how to

19. For example, Bardhan (1993); Meier and Stiglitz (2001); Stern (1989).
20. Meier and Seers (1985); Meier (1987).
21. This point is also made by Eckaus (in this volume).

define the conventional wisdom or the process by which it evolves and changes is no better than any other.[22] We suspect, however, that the usual histories of thought overemphasize the impact of thought on changes in the conventional wisdom and underemphasize the impact of history on thought.[23]

The conventional wisdom does not appear to be a slave of theoretical fashion, but rather it is grounded and pragmatic: big ideas are based on reality and the lessons of experience. But this conventional wisdom has to cope with a reversal of those same big ideas, reversals that are equally grounded in a new reality and new lessons of experience. Today, the new realities have led less to a new set of big ideas than to the loss of faith in big ideas. It is the natural response to the realization that the countries that listened least (namely, India and China) are booming along, while many of the countries that listened most suffered speculative attacks.

This suggests that a growth strategy needs to be both more guided by theory and more sophisticated in interpreting experience and evidence: maybe thought really can influence history. But the most recent round of research, the so-called growth regressions, has failed to pan out in producing useful advice, as the theory and empirics are no match for reality. We argue for an agenda that is less ambitious than an attempt to formulate a theory of everything. A useful agenda needs to acknowledge the complex, state-dependent, contingent relations between policies and growth. This, too, is an ambitious agenda, however, as it needs to establish a theory and empirics that allow for the practical relevance of empirical relationships within their conditions of applicability.

An Obituary for Growth Regressions

One would think that the development community would not need any big ideas since they have the results of growth research. Our first task, therefore, is to explain why most of the recent round of empirical research on growth has been less than successful in providing reliable guidance for questions such as how to accelerate growth in Latin America today. The basic flaw in growth regressions is that they confuse partial correlations

22. This is not a statement of modesty, but a critique.

23. Examples include developments in the academic literature such as the advent of rational expectations in macroeconomics or the new growth models. See Eckaus (in this volume) for a different interpretation of the role played by these advances in economic reasoning.

with (stable) parameters and confuse empirical variables (that might be associated with policies) with feasible actions to promote growth.

By now, there are thousands of papers that put economic growth on the left-hand side and other stuff on the right-hand side. This research produces empirical findings that are translated, more or less crudely, into policy recommendations: a partial correlation of lagged enrollment rates and subsequent growth is interpreted as proof that education is good for growth, which is then used as the basis for recommending more public spending on education; a partial correlation of trade and growth outcomes indicates that openness is good, which becomes a recommendation to reduce tariffs; a partial correlation of inflation and growth shows that low inflation is good, which leads to the adage that fiscal austerity will promote growth. Sadly, many of these recommendations have not worked in practice, in part because nearly all of the growth regression research is essentially irrelevant to policymaking and policy implementation. The findings do not constitute a credible basis for meaningful development advice, since they are not empirically stable and they are not about policy.

Estimates in the typical growth regressions are unstable over time and across countries. Clemens and Williamson show that the relationship between economic growth and measures of outward orientation or trade policy changes dramatically over time.[24] In some periods it is good to be open, while in others it is bad. Knack and Keefer show that social capital measured in the early 1980s is positively correlated with growth in the period 1980–95.[25] But if one regresses growth for the period 1960–80 on social capital, the opposite sign emerges: social capital is negatively associated with growth. Levine finds that the signs of coefficients relating financial systems to growth shift across the decades.[26] Similarly, regressions of the growth of per capita GDP and population growth yield a negative coefficient in the 1960s, a positive coefficient in the 1970s, and a negative coefficient in the 1980s.[27] General growth regressions that allow for parameters to vary over decades easily reject the hypothesis of parameter stability. Furthermore, the growth regression results are typically not constant across countries. Temple shows that the estimated coefficients on human capital in growth regressions are affected by sample

24. Clemens and Williamson (2001).
25. Knack and Keefer (1997).
26. Levine (1997).
27. Kelley and Schmidt (1994).

composition, in ways that imply enormous heterogeneity in the impact of education across countries.[28] The relationships that emerge from partial correlations of economic growth with "other stuff" are unstable because there is no stable parametric relationship between growth and other stuff; the regressions are not correctly specified and parameter stability is a specification test.

Lack of constancy across countries is expected for several reasons. First, there is no reason to expect coefficients to be linear (or log-linear). Many variables exhibit lower or upper threshold effects, so the range of policy improvement matters. Reducing inflation from 100 percent to 50 percent cannot be expected to have the same impact as reducing inflation from 50 percent to zero or even from 10 percent to 5 percent (an equal percentage reduction as from 100 percent to 50 percent). Second, the magnitude of various coefficients may not be constant across levels of development. A modest reform beginning at low levels of income might cause a substantial growth boost, whereas more dramatic reforms are needed at higher levels of income.[29] Third, outcome variables could have many sources of underlying variation, which could, in turn, have different effects on growth. There is no theoretical reason to expect all possible causes of higher investment or lower inequality or more schooling to have the same coefficient magnitude.

The example of schooling illustrates why different sources of the expansion of schooling might have differential impacts. Many theorists continue to peddle education as the panacea.[30] Yet research on the impact of the growth of human capital on economic growth has failed to suggest anything like a huge impact of education on growth, and the battle is to find any impact at all.[31] People should have been leery all along of the notion that there is a single, linear, context-independent relationship between education and output. If the expansion of schooling is driven by rising returns to education because of a dynamic, technologically progressive economy, then this expansion is likely to be positively associ-

28. Temple (1998).

29. Pritchett (2001a).

30. Some would suggest that the next big idea is that previous models ignored human capital in favor of physical capital and that while physical capital has diminishing returns, human capital does not. This is a misreading of both the past (no one ignored human capital) and the future.

31. Pritchett (2001b); Krueger and Lindahl (2001).

ated with growth.[32] If, on the other hand, the expansion of schooling is caused by a totalitarian government drawing children into school as a means of indoctrination into a narrowly drawn (and perhaps incorrect) economic ideology, then the expansion is unlikely to have the same impact on economic output (as in Cuba, for example).

It is not surprising that growth regressions are unstable across countries and over time, because any model in which growth is linearly (or log-linearly) related to any given variable across countries, time periods, levels of development, and circumstances is almost certainly misspecified. The economics profession, however, does not seem to have fully appreciated the limits that this parameter instability imposes on the usefulness of growth regressions for policy guidance. Should an economy have low tariffs for the future? Well, that depends on whether the future coefficient will be like the recent past or like some other historical episode—and the future need not be like the recent past. Similarly, if the impact varies across countries, the policy advisor must know what the impact is likely to be in the country in which he or she is now working. Without a metamodel that embeds and explains the instability in coefficients, the existing regressions provide little guidance for policymaking.

Even if one were to discover stable empirical associations, this still leaves the difficulty of deriving policy advice from growth regressions. The specifications embody other fundamental problems, in that the stuff on the right-hand side is almost never policy. Instead, much of the right-hand stuff is structural, including variables that are not under anyone's direct control and thus cannot have direct policy implications. Regressions of growth on tropical location, for example, or on a country's being landlocked, or on the degree of ethnic diversity do not readily translate into levers of intervention (one would hope).[33] Knowing that ethnic diversity reduces growth might suggest a search for feasible actions that could mitigate this impact, but the partial correlation by itself is merely a statistical fact, at best. Even if one pushes such a finding to interactive impacts, the results do not lead to obvious policy actions: if ethnic diversity is found to

32. See Birdsall, Ross, and Sabot (1995) for an application of this argument to Korea versus Brazil; see Rosenzweig and Foster (1996) on different regions of India.

33. Tropical location may be inversely correlated with growth and positively associated with poor health outcomes. But where are the policy-relevant findings from the growth regressions mapping the sequence from specific health interventions in tropical climes to more rapid growth?

be harmful to growth in the absence of democracies but not otherwise, for instance, it raises the question of who decides whether to be a democracy. Democracy is not a policy choice; it is an outcome, which leads to the next problem.

Most of the variables on the right-hand side of growth regressions are intermediate outcomes, determined by the interaction of policies and events. Take a variable like inequality. Suppose we believed that the evidence establishes a relationship between long-horizon growth rates and inequality.[34] Inequality is the result of a variety of past (accumulation) and present (demand for various factors) decisions, which determine the returns to various assets via market outcomes, plus a set of policies (about taxation and transfers, for instance). Inequality as an outcome does not constitute a summary statistic for any policy outcome. There are almost certainly policy actions that would improve inequality and improve growth, others that would cause both inequality and growth to deteriorate, and still others that would move the two in different directions. A positive partial correlation of inequality and growth implies only that lower inequality outcomes and higher growth outcomes tend to go together in the existing data. This suggests that any specific country may—in the sense it is not impossible—be able to implement actions (such as land reform or progressive income taxes) that would lower inequality and raise growth. Growth regressions showing a partial association between an intermediate outcome like inflation, the black market premium, investment, private investment, FDI, corruption, governance, rule of law, financial depth, and so forth have no direct policy implications and, at best, are suggestive of policies.

Finally, and most intractably, even growth regressions with right hand-side variables under direct policy control—say, the budget deficit or tariffs—are still describing a relationship between policy actions and outcomes, not policies and outcomes. A policy is a mapping from states of the world to policy actions. A tiny model of a toy economy shows how important the difference can be. Suppose that countries have normal times, but at times they might experience either a temporary shock (like a hurricane) or a permanent shock (such as a permanent drop in commodity prices).

34. The qualification of a long horizon is important. The finding that panels cannot be used to support an inequality-growth link (Forbes, 2000) in part reaffirms the basic lesson that one cannot use high-frequency data to make inferences about low-frequency phenomena, which, in turn, illustrates the potential dangers of panel data (Pritchett, 2001a).

The model is one in which running a deficit with no shock is bad, running a deficit to finance a temporary shock is good (in that it reduces the negative impact of the shock), and running a deficit to finance a permanent shock is bad (see table 4). In this toy economy the optimal deficit policy is the following conditional rule: observe the state of the world—if no shock, do not run a deficit; if hurricane, run a deficit; and if price shock, do not run a deficit. This is superior to the general policy of no deficits, but no deficits is superior to a policy of always deficits or the perverse policy of deficits to finance permanent shocks.

What would happen if a researcher ran growth regressions on this dataset? Are budget deficits good or bad for growth? The econometric answer depends entirely on the distribution of countries in the sample. If the sample includes one of each type of country, then deficits look modestly bad. If the sample is dominated by price shock countries, then deficits look terrible. If the sample is populated with relatively more real shock countries (for example, those hit by a hurricane), then deficits look good. Controlling for hurricanes and prices shocks does not change these results. Such controls are insufficient because one needs a full set of possible interactions between shocks and potential policy responses. In other words, one needs a full mapping of possible states of the world. Regardless of the sample composition, however, none of the regressions relating growth to policy actions gives the right policy for any country.[35]

The same problems arise with most regressions that relate institutions to growth performance. Of course, institutional performance matters: when Adam Smith pointed to the importance of rule of law, he was repeating a centuries-old commonplace, not breaking new ground. It is not clear, however, what the adage "institutions matter" might mean, and on one level it is obviously false: countries with similar levels of income have very different institutions; even the four Mini-Dragons display four very different institutional arrangements (in nearly every respect); countries with de jure identical institutions exhibit very different outcomes; and countries that adopt new institutions do not necessarily thrive. One response is that institutional form does not matter, whereas institutional performance does. Institutional heterogeneity simply indicates that different institutional forms can be made to achieve roughly equivalent

35. A related paper emphasizes that the solution to estimating casual relationships—namely, using instruments for policy actions—is not better at identifying the impact of policies than ordinary least squares (OLS), and it can, in fact, be worse (Pritchett, 2002).

TABLE 4. Mapping from Possible Shocks, Deficit Choices, and Growth Outcomes

Type of country	*Permanent shock (commodity price)*	*Temporary shock (hurricane)*	*Budget deficit*	*Growth outcome (percent per year)*
A	No	No	No	2
B	No	No	Yes	1
C	No	Yes	No	–1
D	No	Yes	Yes	1
E	Yes	No	No	–1
F	Yes	No	Yes	–3

performance. This is probably right, but it begs the whole question of the role of institutions.[36]

Promising Research

The growth research has not all been about simple regressions of growth on other stuff. A number of intriguing leads suggest future directions for research. One branch of such literature is episodic analysis, in which researchers examine episodes of more or less discrete changes in policy or intermediate outcome variables. Bruno and Easterly, for example, go beyond the simple regression relationship between inflation and growth (which, like many others, is fragile) and instead investigate episodes of high inflation and what happens as they are stopped.[37] They find that stopping inflation from high levels is expansionary, and it appears to leave countries with even higher growth rates than before the episode. Rodrik examines what he calls savings transitions in an effort to discover what happens to growth rates before and after substantial increases in savings rates.[38] Kamin and Edwards both analyze events before and after large devaluations.[39] Rather than finding any stable relationship between real

36. In de Soto's (2000) recent book, *The Mystery of Capital*, he points out that when systems of property fail, as in Peru and many other developing countries, wealth in real estate cannot be leveraged, although the formal property systems in these countries are exactly like those in countries where they do work. In his examination of the development of property rights in the United States, he finds that it was not the case that good law created good transactions; rather, transactions continued to be conducted outside the ambit of the existing law. Squatters—people using land illegally—gathered the political power to change the law to accommodate the informal transactions. The reality created the law rather than vice versa.

37. Bruno and Easterly (1989).

38. Rodrik (1998).

39. Kamin (1988); Edwards (1989).

exchange rates (RER) and import, export, and economic growth, these papers suggest that episodes of RER overvaluation have a dynamic all their own. Earlier works include the classic Krueger-Bhagwati studies of episodes of trade reform, which were crucial in undermining the ISI consensus (although they went too far in suggesting simple trade liberalization remedies).[40]

A second approach is to examine economic growth directly, either to attempt a close reading of the factors that initiated a growth boom or bust or to undertake cross-sectional work explaining changes in growth rates. While there are many case studies of growth in particular, few actually begin by identifying specific dates for the shifts in growth rates and then analyzing events around those dates to explain the shifts. This approach has the potential to help researchers go beyond the obvious, first-order, proximate causes (such as a terms-of-trade shock) and examine why countries with similar shocks had very different growth trajectories (for example, the effects of the copper price shock on Zambia versus Chile, of the oil price shocks on Nigeria versus Indonesia, and of the debt shock on Brazil versus Korea). Rodrik, for example, uses this approach to partially explain the cross-sectional pattern of changes in growth as a function of growth shocks intermediated by social institutions.[41]

Finally, although growth regressions are inherently hopeless, a few authors work from the growth partial correlations to plausible microeconomic mechanisms that explain the growth correlation, and then to the actual policy variables that could alter outcomes. Examples include the research into financial systems and economic growth presented by Levine and Zervos and by Levine, Loayza, and Beck.[42] These researchers establish a robust macroeconomic partial association (for example, between growth and a measure of financial depth, such as M2/GDP); they then explore a plausible mechanism (for example, how greater monetary depth improves the efficiency of the capital market); and they finally identify the policy-based determinants of the policy indicator (for example, the identification of the financial regulatory reforms which appear to have initiated the causal chain of events.)

40. Bhagwati (1978); Krueger (1974).

41. Rodrik (1999).

42. Levine and Zervos (1998); Levine, Loayza, and Beck (2000). See Levine (1997) for a full review.

The Next Agenda: A Diagnostic Tree for Policy Advice

As development academics, we do not need aspire to be like physicists, who make predictions to fifteen digits and write mathematical models of which the present universe is a special case. Suppose instead that we want to be as effective as medical doctors in diagnosing and treating problematic conditions. What would we need? First, we would have to ground our advice on a complete, coherent, and causal chain from a recommended decision or action to a desired outcome. This would require some sound general theories and empirical evidence to serve as the basis for choosing the preferred effective policy—not necessarily the single right policy action, but the preferred policy.

Second, policy advice would rely less on big ideas (such as government is bad/good or trade is bad/good) and supposedly unvarying growth relationships (such as deficits are bad for growth) and more on a policy decision tree. The prescription of medicine is not a practice of giving the same recipe to everyone because, on average, it improves health. Doctors do not make statements like "drugs are good" or even "penicillin is good." Doctors say things like, "if a patient presents with symptoms that are consistent with a bacterial infection, then treatment with a course of a broad-spectrum antibiotic is a recommended approach (in the absence of contraindications)." This approach requires having a diagnostic tree that allows the practitioner to move through symptoms until reaching a treatable condition. At the same time, not all treatable conditions merit immediate action. If a patient has an infection, is overweight, and has high cholesterol, the treatment will give priority to the infection.

What are the conditional branches of a decision tree for growth recommendations? Claiming we know this with any certainty would obviously be self-refuting, but we suspect that the tree would encompass at least five elements: current level of income, current status of growth, linkages with the world economy, government strength, and government capacity.[43]

LEVEL OF INCOME. The ability of countries like China, India, and Vietnam to grow rapidly from very low incomes, combined with the frustrating fact that many countries reach a plateau and thereafter are unable to sustain rapid growth, suggests that the same set of policies for creating and

43. Durlauf and Johnson (1995) employ a decision-tree approach in their modeling of cross-country growth behavior, but they provide only a limited set of branches for defining alternative growth regimes.

sustaining an episode of rapid growth is not relevant at all levels of income. This is obvious, but the discussion of stages of growth is anathema in academic circles (except for poverty traps, but there are almost no models of Big Push traps or exhaustion of ISI traps).

STATUS OF GROWTH. Another branch (these are not necessarily sequential) is the current status of growth. Policies that will sustain growth once it is initiated are probably different from those that will initiate a growth episode after a long period of stagnation, or those that would initiate an immediate recovery from a recession or a sudden shock to growth. Improved banking regulations serve as an example. They might sustain a boom in one environment (perhaps India), but they might exacerbate a crisis if imposed during a credit crunch (as in Indonesia). Being clearer about the horizon over which various growth policies are expected to operate could avoid mistakes in which policies supposedly conducive to long-run growth perpetuate a crisis.

POTENTIAL LINKAGES WITH OTHER ECONOMIES. Geography and political linkages matter. The degree of binding reciprocal trade, investment, or labor flow arrangements that is possible and desirable depends critically on who one's neighbors are. Living near thriving neighbors with sound institutions creates an opportunity to borrow or even lock-in good policies that is not available to others.

GOVERNMENT STRENGTH. The issue of government strength is a difficult one, given that the long literature on strong versus weak states has not done a terrific job of identifying which is which, except ex post. Nevertheless, many of the success countries clearly have strong states that were able to lead, while many of the countries that failed feature weak states which attempted to implement the same policies that worked for the strong states, but with very different outcomes.

GOVERNMENT CAPACITY. Government capacity can be distinguished from government strength in that a country might have a strong but thin government that was capable of adopting and implementing policies that required little depth (such as exchange rate policy) but could not implement policies that required performance from a large civil service (such as education). This determines the attractiveness of policies that require the use of detailed discretion in implementation.

Trade policy serves as an example in which all sides of the debate might be right, just at different nodes on the diagnostic tree. A low-income country that is growing modestly, with arms-length linkages, a

strong government, and a strong capacity, could have some success with limited protection (for example, Taiwan in 1965). For a low-income country that is growing modestly, with arms-length linkages, a weak government, and weak capacity, a move to trade liberalization that limits bureaucratic discretion can reinforce growth (such as India in 1991). For a middle-income country with poor growth, a weak government, and strong capacity, a move toward deep integration (if possible) can help buy policy commitment and stabilize investor expectations (as in Turkey in 2000). These scenarios are not intended as pearls of wisdom, but rather as examples of the kind of conjectures that need to be empirically validated.

Conclusion

Recent books like William Easterly's *Elusive Quest for Growth* emphasize the many wrong turns that academics and the development community have taken in their well-meaning attempts to improve the lot of the world's poor.[44] Many of the ideas generated by conventional wisdom have not panned out. But they were not the result of either cupidity or stupidity. Today we stand on the shoulders of giants. but in a nonexperimental science like economics, giants face backward: examining the past for hints about the future. The road to development is extremely complex, and the ultimate guide to that path must therefore be more complex than an arrow pointing confidently in one direction.

44. Easterly (2001a).

Comments

Dani Rodrik: Lindauer and Pritchett's fascinating account of the transformation that development thinking has undergone in recent decades holds an unstated irony: as economists have become more and more important in designing and making policy in developing countries, economics itself seems to have become less important. I say this for the simple reason that much of the Washington Consensus—in both its original and augmented versions—cannot be directly deduced from sound economic analysis. Any graduate student in economics knows that liberalization, privatization, openness to trade, and the other strictures in John Williamson's original compilation can be expected to produce economic benefits only if certain other conditions are satisfied (such as completeness of markets, absence of externalities, and full information). The relationship between the so-called second-generation reforms and economic analysis is even more distant. Nothing in economic theory leads one to think that Anglo-American corporate governance or flexible labor markets, to pick just two examples, produce economic performance that is unambiguously superior to, say, German-style insider control or institutionalized labor markets.

When economists qua policy advisors put their weight behind these specific recommendations, they cannot be doing so on the basis of economic theory or clear-cut empirical evidence. More often than not, their advocacy turns out to be based on one or more of the following pathologies.

—Poor empiricism. South Korea has done better than North Korea, and Burma's path is hardly one that leads to prosperity. Markets thus do better than central planning, and trade is better than autarky. But it takes a huge leap of faith to go from these truths to the specifics of the Washington Consensus and its different variants. Even in areas where the empirical record seems clear, close analysis reveals major flaws. Try running a standard growth regression with trade barriers on the right-hand side, and see if it yields the "expected" negative and significant coefficient on the first try. A

similar leap of faith was involved in the 1950s and 1960s, when many newly independent countries tried to emulate the success of the Soviets.

—Hand-waving and implicit political-economy theorizing. Economists qua policy advisors are great believers in simplicity, rules-of-thumb, uniformity, and arm's-length relationships between governments and their private sectors. Why? These opinions are not based on economic theory or empirical evidence, but rather on unexamined and untested assumptions about rent seeking and what governments can and cannot do without becoming hostage to it. This is often a case of economists practicing public administration without license.

—Lack of institutional imagination. No Western-trained economist would ever have come up with China's household responsibility system or township and village enterprises—institutional innovations that lie at the core of the Chinese miracle. Privatization and across-the-board liberalization would have been the recipe offered by any economist from Washington (or Cambridge, Mass.) who was asked to offer advice to the Chinese government in 1978. Only with hindsight are the Chinese innovations seen as the functional equivalent of much more demanding reforms.

The right conclusion to draw from this is not that economics is useless or that economics works differently in different places. Rather, the discussion points to an important distinction between universal economic principles, on the one hand, and their implementation and institutional embodiment, on the other. The latter can vary greatly in form from place to place.

Regarding the universals, I have in mind things such as providing property rights and the rule of law (so that both current and prospective investors can expect to retain the return to their investments); recognizing the importance of private incentives and aligning them with social costs and benefits (so that productive efficiency can be achieved); and managing financial and macroeconomic policies with due regard to debt sustainability, prudential principles, and sound money (so that inflation, macroeconomic volatility, financial crises, and other pathologies can be avoided). Note, however, that these universal principles of good economic management come institution-free. They do not map into unique institutional arrangements or policy prescriptions.

The principle that property rights should be protected implies very little about what is the best way to do this under a society's existing institutional preconditions. It certainly does not imply that a system of private property

rights and Anglo-American corporate governance is the right approach for all countries at all times. China, for instance, has managed to elicit a tremendous amount of investment and entrepreneurial activity through a hybrid system of property rights and a legal regime that is as far as the Anglo-American system as one can imagine. Similarly, the principle that private incentives should be aligned with social costs and benefits hardly results in unconditional support for the policies of trade liberalization, deregulation, and privatization that are the cornerstones of the Washington Consensus. The easiest exercise in the world for a graduate student in economics is to write down a model in which trade restrictions or financial controls are welfare enhancing. Finally, debt sustainability, fiscal prudence, and sound money are also obviously compatible with diverse institutional arrangements. The current obsession with independent central banks, flexible exchange rates, and inflation targeting is nothing other than a fad.

The real puzzle, to me, is why the economics practiced by the World Bank, the IMF, and assorted academic policy advisors has diverged so much from the economics of the seminar room. Until the reasons for this become clear, the discipline of economics needs to be taken more seriously—and economists as policy advisors less seriously.

So Lindauer and Pritchett are right: the economics profession does not need another big idea in development. Instead, it is time to figure out how to turn sound economic thinking into useful, context-contingent policy recommendations.

R. S. Eckaus: Because the authors are insiders but not ideologues—which is, in itself, a rarity—their paper offers an interesting and useful assessment of where the thinking about development economics has been and where it might be going. In my brief comments, I follow their footsteps chronologically, quibbling slightly about those Big Ideas of the past and suggesting some additions to their sources. Finally, I add some suggestions as to where the discipline should go in the future.

The paper, on the whole, accurately characterizes the prevailing views of the 1950s, 1960s, and early 1970s and highlights the apparent dominant economic facts for each period. I want only to add a little emphasis and stress another element. In nonexperimental sciences, facts are seldom offered in the clear, completely indisputable manner that the paper suggests. In economics, facts are reflected through the theories we carry

around, as if those theories were prisms with different indices of refraction that separate and display the colors in the light differently. So what Lindauer and Pritchett describe as irrefutable facts of the times were actually readings of events that were generated by the theories with which they were interpreted. Lindauer and Pritchett's list of the influences on each period's perceptions of the problems and policies in developing countries should include the power of the going theories.

Lindauer and Pritchett are correct in citing the skepticism of the 1950s, 1960s, and early 1970s that markets would always work efficiently and equitably. As the paper points out, that skepticism was largely a heritage of the experiences of the 1930s and 1940s. It was the rationale for the widespread expectation of the times that a "mixed economy," combining elements of government economic activism and market direction, would provide the most reliable basis for economic prosperity.

The prevailing theories contributed to this view of the world. Keynesianism had just become victorious in the 1950s, and it was the established prism through most of the 1960s. Skepticism about the universal efficacy of markets runs deep in Keynesian economics, which carries the conviction that wages do not clear labor markets and capital markets are unreliable. The lessons of the period's economic growth theory were limited and ambiguous. Neoclassical growth theory had an elegant sound to it in explaining the past, but it was not at all useful for policy purposes. Harrod-Domar growth theory had the advantages and disadvantages of simplicity, but at least one could do something with it. It was all about saving and investment, and one could appear to calculate how much of both were necessary for any particular growth rate. It also appeared that with only modest elaborations, it could be extended to calculate foreign exchange requirements and the two gaps between savings and export capabilities.

My own theoretical prism leads me to suggest that the Big Ideas of the 1950s and 1960s were not all wrong. Rather, they caught the essence of the situations that existed in the most prominent developing countries at the time. The world was still recovering from the economic disorders of the Great Depression and World War II. Domestic capital markets were still reorganizing and taking shape. International private capital markets hardly existed outside of the Northern Hemisphere. Development assistance was limited to the World Bank and to the forerunners of the U.S. Agency for International Development. Those agencies concentrated on supporting infrastructure investments in part because the developing countries des-

perately needed those investments, and it was hard to find either international or indigenous private investors in industry.

Lindauer and Pritchett, and others as well, characterize the development thinking of the 1950s and 1960s somewhat unfairly as focusing exclusively on physical investment. Other elements of development were recognized as being important. Perhaps the most persuasive examples are the preoccupation with promoting entrepreneurship, which gave rise to a generation of Ph.D. theses, and the support for intensive investment in education, especially in Africa. In an influential article published in 1961, Paul Rosenstein-Rodan estimates the amount of foreign assistance that could successfully be applied in developing countries.[1] Only very modest amounts would have been allocated to many countries, however, because of what was perceived to be their lack of political and social "absorptive capacity."

I would date the emergence of the next set of Big Ideas rather earlier than Lindauer and Pritchett suggest. By the mid-1970s, a series of books and articles appeared calling attention to the successes of the policies of foreign trade liberalization and domestic deregulation in Korea, Taiwan, Hong Kong, and Singapore. The continued protection of domestic markets and state sponsorship of private industry in these countries was generally overlooked. The successes of those countries had a profound and continuing influence on development policy.

The Big Ideas of the late 1970s and 1980s were partly a reaction against the extensive and intrusive government regulation and control that characterized many developing countries in the previous decades. They may also have been generated, to some extent, by revulsion at the corruption and extensive rent seeking that was facilitated by the government controls. The prescriptions to leave economic development to the marketplace were also supported by the popularity of the emerging economic theories. Rational expectations theory was everywhere, and it carried the view that there are perfect markets all around. That theory has no room for an activist role of government in economics, so no development theory with a role for government policy could be derived from it, including the role that governments in developing countries had played in the 1950s and 1960s.

This period was also the heyday of the perfect capital markets theory, which would lead one to think that the private international banks actually

1. Rosenstein-Rodan (1961).

knew what they were doing when they made their loans to developing countries. That idea contributed to the belief that international grants to developing countries were, at best, unnecessary and, at worst, pernicious.

Many development economists were, at the time, rather conflicted, I believe. On the one hand, they did not want to be left out of what appeared to be the successful new waves of economic theory. On the other, the practitioners were dealing every day with the problems of imperfect information and imperfect markets, which abound in developing countries.

By the 1980s, doubts began to emerge about the applicability and success of the second set of Big Ideas and the theories that supported them. The rapid increase in energy prices and the recurring and widespread international debt crises made rational expectations more evidently implausible. The so-called lost decade of the 1980s was a failure of the hypothesis of perfect capital markets. The bank lending of the 1970s and 1980s was more like a mass delusion and speculative bubble. Likewise, the methods by which the debt crises were resolved were commitments imposed by the monopoly power of the private banks and financial institutions of the developed countries.

I doubt that events in the centrally planned economies of the former Soviet Union and Eastern Europe had much effect on development thinking in Europe and the United States. By the 1980s it was clear to most observers that those economies were going rather badly and should not serve as models for any other country. However, some policymakers in developing countries were—and still are—under the illusion that the former Soviet Union and Eastern European countries could serve as models. They have yet to be disillusioned.

Lindauer and Pritchett do not give enough weight to the political instability of many developing countries as a factor that helped make it clear to economists that politics and political events often trump economics. A Big Idea in political science in the 1950s, 1960s, and 1970s—namely, that there is something called political development—has simply disappeared.

As the paper says, all the Big Ideas of the past have been called into question, and economists have rightly become skeptical of panaceas, although it is still true that there is no end to fads and fashions. I disagree with Lindauer and Pritchett that "so little seems to work." Rather, different policies work at different times in different places and even at different times in the same place. It would be a mistake to reject everything in the Big Ideas of the past, although it is necessary to be discriminating in their

application. When they were articulated, they were relevant for particular countries, or just for particular sectors in particular countries, at particular times. That is true now, as well. Central direction is still desirable for some important features of the economy, depending on the country. Policies calling for government backing for medical care, education, environmental protection, and some types of transport facilities, at least, would probably find general support. These areas coexist with sectors in which market mechanisms should be allowed to work. Yet economists now recognize that there are more types of market imperfections than the conventional monopolistic elements and external economies that were the early focus of attention. Vigorous participation in foreign trade and international investment is desirable, but it has to be watched and often controlled carefully to avoid some disagreeable features and concomitant instabilities. Simple rules for foreign exchange rates and central bank policies are not successful, and careful activism is required.

Perhaps the most important lesson to be learned from the experience with the Big Ideas of the past—one which should have been known without that experience—is that there is no one-size-fits-all policy for all situations and all times, however tempting the idea. Economists are not the only ones who like to generalize. Historians do, too, for example. Economists are more dangerous, however, because their theories are better developed, which gives them more confidence. So when their hands are on the levers of power, albeit usually distantly, they are more willing to commit themselves and others to their guiding principles.

Unlike Lindauer and Pritchett, I believe that some major new themes and guidelines for policy are emerging in the discipline. While they are not advertised as Big Ideas that pretend to generate comprehensive policies, they do embody important innovations in development policy backed by economic theory. Skepticism about the "perfect markets" view of the world is again on the rise, but this new skepticism is more specific about the kinds of imperfections and their implications Contract theory, information theory, and some game theory are helping advance this area. A policy example is community-based lending, which has become popular in recent years and which has been explained and supported by innovative economic analysis. Another example is the use of incentives in government programs, which is being examined with better and larger microeconomic datasets. Such analysis has improved understanding of market problems and consumer behavior. While these innovations may not quite

qualify as the next Big Ideas, they are producing interesting and useful results.

As the paper says, careful studies of particular sectors for particular countries have always been a part of development economics and should continue to be so. Development economists do not have to apologize for this. That is what occupies most economists who are working on advanced countries. Those studies may not add up to anything like the Big Ideas of the 1950s, but they can generate important lessons.

Finally, one Big Idea that is lacking in modern economics is an emphasis on the need for more and improved data of all kinds: microeconomic and macroeconomic. There is a real contrast, in this respect, between the way economists and scientists approach analogous problems. I have had occasion to watch physical scientists approach large and complicated problems in atmospheric chemistry and climate modeling for which they cannot do experiments. To provide the data necessary for their modeling, they have established projects to collect information on a global and atmospheric scale. By comparison, economists, for the most part, have tried to be clever with whatever bits and pieces of information the various government agencies have decided to provide, very often without any idea as to how the information will be used. A lot of time is spent trying to find ingenious ways of overcoming this handicap, when the cost would be better spent on improving the data.

I have always thought that although development economics is not the only game in town, it is the most interesting and the most important. So let us keep doing it—better!

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