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 from 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 advisers 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 advisers 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 indexes 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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