

# Diagnostics before Prescription

Dani Rodrik

**F**ew branches of economics have wielded as much influence on the world of policy as development economics. Virtually every major development strategy of the last 50 years is associated with some pioneering research that provided its intellectual underpinnings. Consider some of the key milestones. The dominant import substitution policies of the 1950s and 1960s were the practical realization of the ideas of Prebisch (1959) and Singer (1964) and were based on the famous Prebisch-Singer thesis on the declining terms of trade for primary products and the dynamic benefits of manufacturing. The emphasis on development planning in those same decades was greatly influenced by Rosenstein-Rodan's (1943) "Big Push" framework, with its stress on increasing returns to scale and the need to kick-start growth through large-scale investments, and the planning model of Mahalanobis (1955), which argued that economic development could be accelerated by government encouragement of heavy industry.

When such models were discarded in the 1980s in favor of more outward- and market-oriented strategies, it was in no small measure because of the research published during the 1970s by Balassa (1971), Bhagwati (1978), Krueger (1978), and Little, Scitovsky, and Scott (1970). The "Washington Consensus" of the 1990s, despite its appellation, represented the common views of a group of *Latin American* technocrats and policymakers, many of whom had trained at top economics departments in the United States. The influential "Human Development Reports" of the United Nations Development Programme, which rank the well-being of countries according to a combination of GDP, health, and education statistics, were inspired

■ *Dani Rodrik is Professor of International Political Economy at the John F. Kennedy School of Government, Harvard University, Cambridge, Massachusetts. His e-mail address is <dani\_rodrik@harvard.edu>.*

doi=10.1257/jep.24.3.33

by Amartya Sen's (1999) broad vision of development and his emphasis on human capabilities. The U.N. Millennium Project, the "action plan" designed to achieve the Millennium Development Goals, was the brainchild of Sachs et al. (2004). The emphasis on improved governance in the current wave of economic reforms is motivated by North's (1990) ideas on institutions.

So if we were to measure the achievements of what has come to be called "macro"-development economics by its real-world impact, the verdict would be quite clear-cut: it has been a stunning success.

But further reflection should give us pause. For one thing, if all these economists of the first rank have seen their ideas turn into practice, shouldn't the problem of global poverty have been solved? Clearly, the world is still full of poor people, and the problem of underdevelopment remains one of the intractable challenges of the global economy. One possibility is that the research in question has systematically failed and has in fact led policymakers astray. I think this interpretation of the research record is too harsh, and I will advance an interpretation below that is more considerate to development economists. But either way, this puzzle needs to be addressed.

A second curious feature is the apparently cyclical nature of the research in development. Each new generation of work is a self-conscious reaction to past thinking, and is superceded in turn by a similar reaction to itself. The import substitution strategy was designed to correct what Prebisch (1959) and others saw as an excessive bias towards free trade. The Washington Consensus approach in turn sought to steer the ship of state away from protection and towards free trade. Similarly, the strategic emphasis in development seems to move from a "growth" focus to a "poverty" focus, and then back again. A superficial reading of this intellectual history suggests there is little real advance in knowledge, just fads and fashions. Again, I think this verdict is too harsh, for reasons I will elaborate below.

A final source of dissonance has to do with the real successes in development. There has not been a greater instance of poverty reduction in history than that of China in the quarter century since the late 1970s. Yet can anyone name the (Western) economists or the piece of research that played an instrumental role in China's reforms? What about South Korea, Malaysia, or Vietnam? In none of these Asian cases did economic research, at least as conventionally understood, play a significant role in shaping development policy. The same is true of other long-term successes elsewhere, such as Botswana and Mauritius. Even Chile, whose economic success is sometimes (inappropriately) attributed to advisers with roots at the University of Chicago, distinguished itself only after the country discarded some of the disastrous policies of the "Chicago boys" and worked out its own, partly heterodox strategy—a combination of economic liberalism, an undervalued currency, capital controls, and a generous helping of social policies.

So what is going on here? I think many of these paradoxes arise when applied economists and policy advisors mistake models and arguments that are valid only in specific circumstances for universal remedies. Once nuanced, fine-grained, contextual research gets transformed into simple rules of thumb, two things tend to

happen. First, the research loses relevance and effectiveness. Second, the research develops in its “vulgar” form the potential of doing actual damage by being applied in inappropriate circumstances. So we get the excesses of import substitution, the Washington Consensus, and (no doubt soon) the improved governance agenda.

The original researchers who instigated each of these strategies were themselves quite aware, at least in most cases, of the nuances of their arguments and the specificities of their policy proposals. Reading many of the original articles and books from today’s vantage point, one is left with great respect for the minds at work and for the evidence on display. The reader who expects facile generalizations that have not stood the test of time will in fact be disappointed. The bigger surprise is that there is often only a tenuous relationship between these works and the caricatured message for which they often stand as a short-cut reference. As long as we read these previous paradigmatic works as partial representations of underdevelopment’s syndromes and not as attempts to provide a complete picture, they do represent cumulative knowledge rather than reactions or fads. Raul Prebisch, Anne Krueger, and Jeffrey Sachs are all correct—at different times and under specific circumstances.

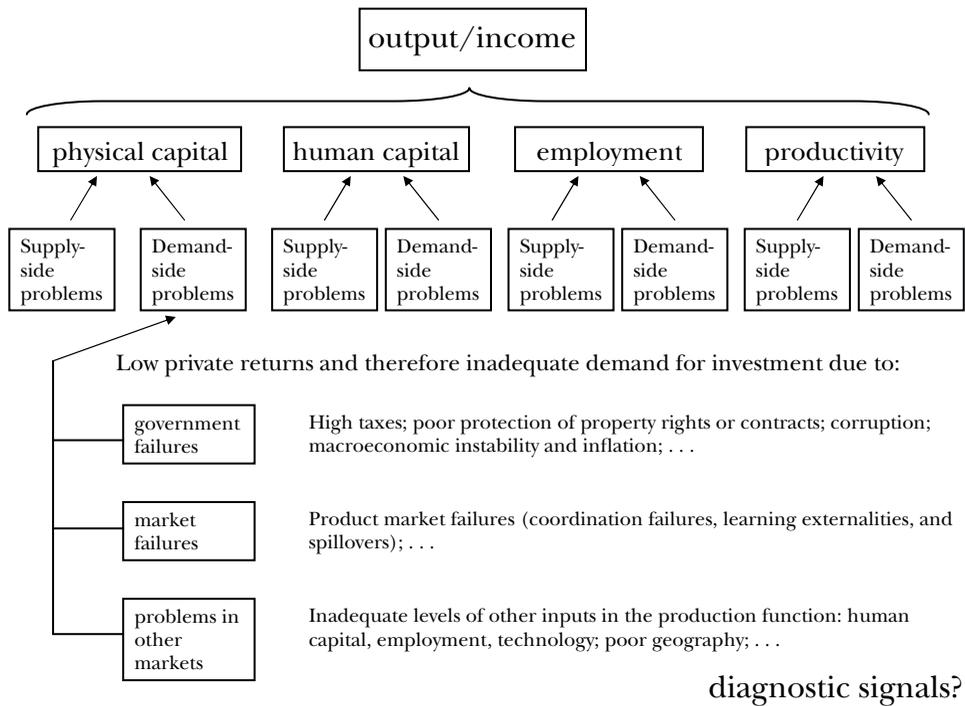
The message is that development economists should stop acting as categorical advocates (or detractors) for specific approaches to development. They should instead be diagnosticians, helping decisionmakers choose the right model (and remedy) for their specific realities, among many contending models (and remedies).

In this spirit, Hausmann, Velasco, and I have developed a “growth diagnostics” framework that sketches a systematic process for identifying binding constraints and prioritizing policy reforms in multilateral agencies and bilateral donors.<sup>1</sup> The original Hausmann, Rodrik, and Velasco (2008) paper was largely an attempt to show how it is possible to sift through what may seem like a bewildering array of problems to hone in on the most likely culprits for growth failures through a combination of simple theory and suggestive empirics. Hausmann, Klinger, and Wagner (2008) provide an update and a helpful guide to the state-of-the-art in this area.

Growth diagnostics is based on the idea that not all constraints bind equally, and that a sensible and practical strategy consists of identifying the most serious constraint(s) at work. The practitioner works with a decision tree like the one shown in Figure 1. (The figure shows only a few of the details to give a flavor of the actual analysis.) The researcher asks at each node what kind of a diagnostic signal the economy would emit if the hypothesized constraint were indeed the binding one. For example, in an economy that is constrained by the supply of capital, as in the neoclassical growth model, the cost of capital would be inversely related to investment, and any increase in transfers from abroad (whether in the form of remittances or foreign finance) would ignite a domestic investment boom. Sectors

<sup>1</sup> It has been impressive—and at times frightening—to see how rapidly the “growth diagnostics” methodology was adopted and disseminated, even before the original article was published. The paper was written and first circulated in 2005. The published version is Hausmann, Rodrik, and Velasco (2008). A list of country studies using the approach with links to the original papers can be found at <http://www.hks.harvard.edu/fs/drodrik/GrowthDiag.html>.

Figure 1  
**From Growth Theory to Policy Diagnostics**



Source: Author.

that are the most capital-intensive or most dependent on external finance would be those that are growing the slowest. In an economy constrained by investment demand, on the other hand, as in models of institutions and growth, poor private investment would respond primarily to profitability shocks in goods markets, and it would be consumption that responds to foreign capital inflows (this is the case shown in Figure 1). Even though the evidence will rarely settle such questions decisively, it is often possible in practice to reduce a long catalog of failures to a considerably shorter list of most severe culprits.<sup>2</sup>

The second step in growth diagnostics is to identify remedies for relaxing the constraint that are appropriate to the context and take cognizance of potential second-best complications. An excessive degree of inward orientation, to take one prominent example, can be alleviated by reducing import barriers (Chile), subsidizing

<sup>2</sup> In an executive program for senior World Bank economists that we run at the Harvard Kennedy School, I use the decision tree to lead a discussion on South Africa's binding constraints. Every year, I am surprised at how quickly these practitioners dismiss some of the conventional culprits that typically preoccupy them in their country work (such as poor governance, macroeconomic instability, bad infrastructure, lack of openness to trade) and come to focus on a few problem areas (typically, lack of competitiveness in tradables and high cost of labor).

exports (South Korea), setting up free-trade zones (China), and many other ways. The appropriate choice of remedies may well make the difference between success and failure. Yet the importance of this step, and the ingenuity involved, are often obscured by a tendency to rely on textbook solutions or “best-practices” (Rodrik, 2008). As I will elaborate below, China owes a great deal of its success to a willingness to experiment pragmatically with heterodox solutions.

Successful countries are those that have implemented these two steps in an ongoing manner: identify sequentially the most binding constraints and remove them with locally suited remedies. Diagnostics requires pragmatism and eclecticism, in the use of both theory and evidence. It has no room for dogmatism, imported blueprints, or empirical purism.

### **When Economists Overreach: The Debate on Inward versus Outward Orientation**

In her 1997 presidential address to the American Economic Association, Anne Krueger (1997, p. 3) described the development strategy prevailing in the early decades after World War II in the following terms: “These were a mixture of touristic impressions, half-truths, and misapplied policy inferences. In hindsight, it is surprising how some then-accepted stylized ‘facts’ were so uncritically accepted and held sway for so long.” Krueger then went on to describe how subsequent research in the 1960s and 1970s had displaced such views and replaced them with a new consensus on the importance of neutrality in price incentives and of outward orientation. “[I]mproved understanding of trade and development,” Krueger (p. 3) wrote, “came about in large part through research which effectively demonstrated the falsity of these premises.” The research Krueger discusses includes several sets of comparative country studies: Little, Scitovsky, and Scott (1970), which was done for OECD; a group of NBER studies summarized in Krueger (1978) and Bhagwati (1978); and a number of World Bank studies. As Krueger recounts, this body of work was remarkably successful in transforming prevailing views on development strategy and in ushering in an era of policy reform. This new understanding ultimately became a cornerstone of the “Washington Consensus,” with its emphasis on deregulation, privatization, and stabilization. This episode is probably as close as economics has ever come in the last half century to fostering not just an intellectual revolution, but also a policy revolution all across the globe.<sup>3</sup>

But in fact the new “consensus” could be faulted on exactly the same grounds that Krueger had used in dismissing prevailing views on import substitution and “big push” development strategies. By the time the underlying research had

<sup>3</sup>As Timothy Taylor reminds me, “inflation targeting” comes a close second. As powerful as the impact of the academic research on inflation targeting was, its influence was limited in practice to high- and middle-income countries. The Washington Consensus, by contrast, became the marching orders for economic policymakers all over the world.

filtered through to the general consciousness and metamorphosed into the *Weltanschauung* of the 1980s and 1990s, it too was little better than “a mixture of touristic impressions, half-truths, and misapplied policy inferences.” Here are three of those half-truths: A first claim is that successful countries are those that open themselves up to trade and rely on the forces of comparative advantage, as the East Asian countries have done. A second claim is that import substitution and infant-industry promotion does not work, as the experience of Latin American countries and others such as Turkey and India was taken to demonstrate. A third claim is that government intervention is futile because rent seeking and incompetence undermine even well-meaning political leaders. Each of these statements holds a grain of truth, but no more. The actual reality was considerably more complex.

The East Asian countries had actively shaped their comparative advantage through policies aimed at speeding up structural transformation. Many of those policies—subsidies, trade restrictions, financial market interventions, public ownership—did not look all that different from those in place in countries following import substitution strategies. Many countries that followed inward-looking strategies, including Mexico, Brazil, and Turkey, had also grown quite rapidly from the 1950s into the late 1970s—actually doing better under import substitution than they did after they opened up their economies to trade in the 1980s and 1990s (Rodrik, 2007, chap. 1). The simplistic view that the Asian economies had outperformed and outgrown the rest because of less intervention in trade or greater neutrality in incentives was unsupportable on the basis of the underlying evidence.

The paradox is that no one who had paid close attention to the research underlying those broad conclusions should have been surprised. The complexity of the South Korean and Taiwanese experiences had been laid bare in the very same OECD and NBER studies that later authors would cite in support of the Washington Consensus. Let me give two examples.

The Little, Scitovsky, and Scott (1970) project undertaken for the OECD calculated “effective rates of protection”<sup>4</sup> for a number of countries so as to compare their trade regimes in an objective manner. Among the countries included were Taiwan, an archetypal outward-oriented country, and Mexico, a leading case of import substitution. If we look at the evidence in this volume closely, we find that the average level of effective rates of protection in manufacturing seems to have been higher in Taiwan than in Mexico (Table 5.2). Moreover, there was also greater variation in effective rates of protection across activities in Taiwan than in Mexico (p. 185). It is hard to square this evidence with what eventually became firmly rooted pieces of conventional wisdom, namely that outward-oriented countries had lower trade protection or that they exhibited a higher level of efficiency in resource allocation (conventionally measured).

<sup>4</sup>The effective rate of protection is a measure that tracks the effect of trade protection on the domestic prices of both outputs and intermediate inputs and provides a summary indication of the protection received by value added in an activity.

A second example comes from South Korea. The Frank, Kim, and Westphal (1975) study in the NBER series directed by Bhagwati and Krueger meticulously quantified the incentive regime in this country, only to find—to the surprise of its authors—that anti-export bias (measured by the ratio of effective exchange rates for imports to exports) was not significantly lower in Korea during the 1960s than it had been during the previous decade. In fact, the relative price of exportables was higher in 1959–60 than at any time during the 1960s. In light of this evidence, it is difficult to see how we can attribute South Korea's export boom and rapid growth starting in the mid-1960s chiefly—or even in part—to trade reforms of the early 1960s (for further discussion, see Rodrik, 1995).

The point of these examples is that the results of the research were not nearly as clear-cut as later renditions would make them seem. It was in fact possible to construct a different account of East Asian growth (as well as of the disappointing performance elsewhere) based on the very same evidence presented in the underlying country studies of the NBER–OECD–World Bank projects. In Rodrik (1995), I relied heavily on the Frank, Kim, and Westphal (1975) book to sketch an argument for South Korea and Taiwan in which trade policy plays a largely supportive and secondary role. My account of the import substitution experience—why countries following this strategy did well for a while and why they collapsed later on—is also based on evidence from these country studies (Rodrik, 1999). My stories may be wrong. But they are not inconsistent with the evidence presented in the NBER–OECD–World Bank projects; in fact, they are partly based on that evidence.

Some of the major findings in the NBER–OECD–World Bank research were as incontrovertible as they were important. For one thing, these studies demonstrated that the actual pattern of incentives generated by the policy regimes in place—as measured by the dispersion in effective rates of protection, for example—had been much more haphazard than what any policymaker, regardless of underlying beliefs in infant industries or import substitution, could have rationally wanted to achieve. Second, exchange-control regimes based on a combination of inconsistent monetary and fiscal policies and foreign currency rationing had been economically very costly, leading to stop-go macroeconomic cycles, periodic crises, and slow growth. But beyond these lessons, it was difficult to be sure about much else. In particular, the findings did not allow clear verdicts on the respective merits of low versus moderate levels of trade protection nor on the desirability of government intervention in favor of specific industrial activities.

That the country evidence was complex and could be read in different ways should not be all that surprising. Indeed, Bhagwati (1978) and Krueger (1978) ended up publishing separate synthesis volumes for the NBER project in the 1970s, apparently in part because they couldn't agree on the conclusions from the research they had directed. In hindsight, what is surprising is that such a strong consensus emerged on one particular reading of the evidence. To what can we attribute this?

I am not sure I have a very good answer. Part of the explanation has to do with the broader intellectual climate of the 1980s. This was the time of the Reagan and Thatcher revolutions: markets were in and the state was out. But another important

reason is that development economics is an applied, policy-relevant discipline, and as such is prone to get simplified and routinized in practice. The practitioner in an international organization or aid agency needs clear rules of thumb, not a lot of ifs and buts. When asked what to do, the mantras of “import substitution” or of “liberalize, stabilize, privatize” present a clear mandate for action. “We don’t know” and “it depends” are unlikely to be met with equal enthusiasm.

Researchers and academics have an important responsibility here: they have to resist the temptation to substitute prepackaged solutions for nuance and skepticism. The record suggests they have not always been very good at this. Despite their scientific demeanor, economists are subject to the same cognitive biases as others: overconfidence, tendency to join the herd, and proclivity to overlook contradictory evidence. As a consequence, too often they become associated with (and promoters of) universal blueprints only loosely grounded in theory and evidence.

Predictably, the consensus on the efficacy of trade liberalization as a consensus instrument for powerful economic development has dissipated over the last decade. For example, many Latin American countries took a big leap toward trade liberalization in the 1990s, along with other substantial steps in the market-oriented, deregulate-and-privatize spirit of the Washington Consensus approach, but failed to experience a corresponding surge of economic growth. The currently prevailing view, as reflected in the World Bank’s (2005) report on the lessons from the 1990s or by the blue-ribbon Commission on Growth and Development (2008) chaired by Michael Spence, accepts the importance of outward orientation but places much less emphasis on trade liberalization and is much more willing to condone a measure of industrial promotion in order to achieve and sustain high growth. The overreaching has been corrected, but not without cost. The Washington Consensus of the 1990s has left lots of frustration and unrealized expectations in its wake.

## **The Role of Experimentation, in Empirics and in Policy**

In the early 1980s, an astonishing 50 percent or more of China’s economic regulations were explicitly marked as “experimental” (Heilmann, 2008). The Chinese leadership was essentially saying: “We don’t have a very clear idea about what will work, so we shall try this for a while and see what happens. If the results are good, great! If not, we scrap the measures and go back to the drawing board.” It was almost as if Deng Xiaoping and his entourage had internalized IBM founder Thomas Watson’s famous dictum: “If you want to succeed, raise your failure rate.”

This experimental approach to development policy, which has been so spectacularly successful in China, stands miles apart from the Washington Consensus or other strategies discussed previously. The latter are the product of a *presumptive* mindset. They start with strong priors about the nature of the obstacles to development and the appropriate fixes. They are typically operationalized in the form of a long list of reforms (which are sometimes categorized, not unfairly, as a “laundry list”). They emphasize the complementarity among reforms, rather than their

sequencing and prioritization. They exhibit a bias towards universal recipes, “best-practices,” and rules of thumb.

The experimentalist approach, by contrast, starts with relative agnosticism on what works and what doesn’t. It is explicitly diagnostic in its strategy to identify bottlenecks and constraints. It emphasizes experimentation as a strategy for discovery of what works, along with monitoring and evaluation to learn which experiments work and which fail. It tends to look for selective, relatively narrowly targeted reforms. It is suspicious of “best-practices” or universal remedies, looking instead for policy innovations that provide a shortcut around local second-best or political complications (Rodrik, 2009).

Until recently, there was no good way to fit China’s economic reforms within accepted development paradigms. After all, China cannot be easily categorized either as a free-market economy or as a planned one. It is an economy that has grafted a market system on top of a heavily regulated state sector—but its dual nature has been a source of strength rather than weakness (Lau, Qian, and Roland, 2001). Its strategy conforms neither to import substitution, nor to the Washington Consensus, nor to the new governance agenda. The best way to describe the strategy would be to call it “eclectic” or “pragmatic.” China’s unconventional policies may be too obvious to miss, but a similar mix of orthodox and heterodox elements characterizes all successful growth experiences, such as South Korea in the 1960s and 1970s, Mauritius in the 1970s and 1980s, or India during the last couple of decades (Rodrik, 2007). In all these cases, there was sufficient reliance on markets and the price system for liberalization-minded economists to walk away in the belief that growth was the result of conventional reforms. Yet government intervention has also been rampant in these instances, allowing advocates of industrial policies and government-directed industrialization to draw diametrically opposite conclusions.

The Chinese experience highlights the highly contextual nature of appropriate development policies. Different constraints on growth bind at different times, necessitating varying solutions over time. So China followed a strategic and sequential approach targeting one binding constraint at a time, first in agriculture, then in industry, then in foreign trade, and eventually in finance. It adopted pragmatic, often heterodox solutions to overcome political constraints and second-best complications. For example, to insulate government revenues from the effect of price reform it relied on *dual-track pricing*, in which government production quotas and controlled prices are maintained in place but additional production can then be sold at a market price. Under the *household and contract responsibility system*, farmers and businesses were allowed to retain their profits, giving them the incentives to produce and invest without explicit privatization. *Township and village enterprises* served to align the interests of their owners (local governments) with entrepreneurs, and helped to get around weaknesses in judicial enforcement of contracts. *Special economic zones* were allowed export incentives in certain areas, without removing protection for state firms (and hence safeguarding existing employment to some extent). *Federalism “Chinese-style”* provided a clear separation of the central government from local and regional governments in certain specific

dimensions, in a way that could generate incentives for policy competition and institutional innovation. Again, none of these policies are easily characterized as free market or as central planning. They operated instead on a boundary of altered incentives and political constraints. The process of China's policy reform consisted of diagnosing the nature of the binding constraints and identifying possible remedies in an innovative, experimental fashion with few preconceptions about what works or is appropriate.

Such an approach is no longer as alien to development economics as it once was. One reason for this change is the increased emphasis on diagnostic frameworks in growth analysis, as I outlined earlier. Another reason is the spread of randomized experiments in microdevelopment. Both of these approaches exhibit a healthy distrust of received wisdom about what works and what doesn't work and instead focus on contextual solutions. Stripped of methodological purity, much of what the randomized evaluators do is in fact very similar in spirit to growth diagnostics (Rodrik, 2009). In both cases, the process consists of 1) identifying specific failures that produce economic disappointment, like poor health and educational outcomes or low growth; 2) generating policy innovations to remove those failures; and then 3) finding ways of credibly testing for the effect of the proposed remedy. Those who conduct randomized development experiments often emphasize testing, but in the absence of the first two steps, the results of their exercises would be devoid of much interest! Although the growth diagnosticians typically cannot resort to randomized evaluations, it would be silly to think that they cannot learn about policy impacts through monitoring and other kinds of evaluation. None of the Chinese policy experiments were subjected, as far as I know, to randomized evaluations, yet it is evident that the Chinese leadership drew the right economic lessons from them for the most part.

Policy learning is all about updating one's priors, and as I have argued in Rodrik (2009) there are many different ways of doing this. Experimental methods of policy evaluation that nail down identification (albeit in a hyperspecific context) are not always clearly superior to other empirical methods, in view of their problems with whether the results can be extrapolated to other places and times. If macrodevelopment economists have to be humble about what they already know, microdevelopment economists have to be humble about what they can learn (Deaton, 2009).

Ideally, diagnostics and randomized experiments should be complementary; in particular, diagnostics should guide the choice of which random experiments are worth undertaking. Any developmental failure has hundreds of potential causes. If the intervention that is evaluated is not a candidate for remedying the most important of these causes, it does not pass a simple test of relevance. Yet the tools of diagnostics remain surprisingly underresearched.

Consider the challenge of increasing educational attainment in developing countries. The roots of the problem may lie in credit constraints, poor school quality, low returns to education, health issues, and many other potential causes. Each one of these causes in turn can be addressed by an endless number of interventions. Moreover, the underlying constraints and appropriate remedies are likely

to differ across different settings. Narrowing the field down to a manageable set of possible remedies requires a combination of theoretical reasoning and judicious use of earlier surveys and empirical work.

Randomized field experiments, which are legion in this area, have demonstrated considerable success with specific interventions. Importantly, some of these interventions—on school subsidies or remedial education, for example—have been replicated in a number of different contexts (Kremer and Holla, 2009). Still we have very little guidance from this literature on how we proceed to identify education interventions that are most suited to and likely to be most effective in a particular setting. We get even less help on diagnosis in other areas such as reducing corruption or increasing manufacturing productivity, which have received only spotty attention from randomizers. The best among randomized trials in development economics are of course informed by some diagnostic process, but curiously, microdevelopment economists are often not very explicit about the steps needed to identify the most serious failings in a given context. Nor are they very clear about how one narrows a very large list of potential solutions to a smaller number of interventions most likely to be effective.

## The Frontier of Diagnostics

Development economists have too often fallen in the trap of believing in the “one right way,” a universal fix for underdevelopment or (more commonly these days) a single best way of learning about what works and what doesn’t. The result has been overreaching followed by disappointment and revisionism. The main message of this paper is that there is great value in pluralism. Each model of development is a partial representation, relevant in some settings and less so in others. Each empirical finding is a product of the specific context in which it was derived. The best way to avoid the fads and cycles of the past is to give up on a Holy Grail that produces development at all places and time, and instead to invest in learning how to navigate these varying realities. What we need is a systematized way of choosing among them for the context at hand. Diagnostics is the next frontier, and offers a most fertile area for research. The field of development economics will have really advanced when graduate courses in economics teach not only a series of models and empirical applications, but also a method for figuring out which among them are relevant in what setting.

■ *I thank David Autor, Jim Hines, Chad Jones, and especially Timothy Taylor for comments and suggestions that greatly improved the paper.*

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