Development Economics in the Wake of the Washington Consensus: From Smith to Smithereens?

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Books Reviewed

Baumol, William J., Litan, Robert E. and Schramm, Carl J. (2007). *Good Capitalism, Bad Capitalism and the Economics of Growth and Prosperity*. New Haven, CT and London: Yale University Press.

Chang, Ha-Joon (2008). *Bad Samaritans: The Myth of Free Trade and the Secret History of Capitalism.* New York: Bloomsbury Press.

Collier, Paul (2007). *The Bottom Billion: Why the Poorest Countries are Failing and What Can Be Done About It.* Oxford: Oxford University Press.

Reinert, Erik S. (2007). *How Rich Countries Got Rich ... and Why Poor Countries Stay Poor.* New York: Carroll and Graf Publishers.

Rodrik, Dani (2007). *One Economics, Many Recipes: Globalization, Institutions, and Economic Growth.* Princeton, NJ: Princeton University Press.

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The dismal science is in a foul mood. Pity the "orthodox" economist who sets foot in debates about development economics these days, for the clearest consensus of the books reviewed here is that the old orthodoxy in development thinking should be quickly put behind us. The real world has not been kind to the policy prescriptions of the 1990s. Latin America failed to grow rapidly, despite enthusiastic implementation of so-called "Washington Consensus" reforms. Meanwhile, the Chinese economy's dynamic growth is galling in light of the country's insistent rejection of so many mainstream precepts. Orthodox economists may take solace that they are unlikely to be burned at the stake, as were some particularly misfortunate free-trade advocates following the War of

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Spanish Succession (Reinert: 167). But many of the conventional prescriptions for development are at risk of being put to the torch.

What will come in their place? Here there is considerable confusion, muddled further by an uncertainty about which "orthodoxy" is in fact being challenged: laissez-faire economics, Ricardian trade theory, "neoliberalism," the Washington Consensus, or some confusing mix of all the above. But some important road signs for the future emerge from the cacophony of ideas presented in the five books reviewed here, which are without exception thought-provoking, highly accessible to non-economists, and excellent candidates for an advanced-level seminar on the political economy of development.[1]

If you have not been reading about economic development recently, put aside everything you thought you knew. The development of development theory over the past decade has been remarkably rapid, charting a path from a "neoliberalism" supposedly inspired by the free-market mantras of Adam Smith to a far more nuanced approach. Even the most conventional of the approaches here, by William J. Baumol, Robert E. Litan, and Carl J. Schramm, is critical of past growth research and contemporary growth prescriptions. For all their differences, then, the authors agree on more than one might expect. Five points recur across most of the books:

1. Free trade is being challenged in theory and practice, if indeed it was ever going to survive developed-nation protectionism. The emergent consensus is that poor nations should be allowed to employ smart domestic-industry protections.
2. There is a clear divide between economics as theoretical science and economics as practice. Economics the science is useful in establishing the general first-order principles that guide policy choice; economics the practice depends on a willingness to employ flexibly second-best solutions that better fit real-world conditions.
3. The notion of increasing returns (that in "some economic activities costs fall as the volume of production increases" [Reinert: 36]) is in vogue. Promoting innovative industries that exhibit increasing returns, and then nurturing them, is an idea whose time has come (again).
4. Institutionalism is passé. Institutions remain important, of course, but the operative approach is a much less prescriptive one, focused on the broader incentives institutions provide, as opposed to the form they assume.
5. The optimistic 1990s zeitgeist (a flat world with walls tumbling down) has given way to a much more skeptical view of globalization. The world is composed of three groups of countries: rich, not so rich, and chronically miserable. Each requires its own solutions. Cookie-cutter, one-size-fits-all solutions are out. Tailored, handmade, and country-specific remedies are in. But even countries that make progress in their own way may be facing a far more hostile global environment than they were two decades ago. Globalization is no panacea, by a long shot.

In the wake of neoliberalism's demise, all would seem to agree with Dani Rodrik's clever paraphrasing of Churchill: economists need to be a bit humbler, for they have much to be humble about. But these common themes aside, the books fit into two groups: those who would raze existing development theory, and those who seek a more modest remodeling of its basic approach. Erik S. Reinert and Ha-Joon Chang are fired up by what they see as the almost criminal, unquestioning propagation of Smith and Ricardo's economic theories. The remaining authors (belonging to a somewhat more conventional strand of economists) recognize the failures of the past two decades, but appear more concerned with finding policy substitutes than with the theoretical battle itself.

Razing Development Theory

Let us begin with the more strident critique, laid out in popular form by Chang and more academically, but no less passionately, by Reinert. Of the two books, Reinert's is the more informative read – a calculated effort to systematically lay low the prevailing orthodoxy in a pincer movement, attacking conventional wisdom in both theory and policy.

On one flank, Reinert criticizes economics as a discipline for discarding a rich tradition of qualitative, case-study-oriented research in its quest to become a "hard" science. To Reinert, the central pathology of modern economics is that it privileges formal mathematical modeling over practical policy. This physics-oriented economics has proven "unable to capture qualitative differences between economic activities that end up as quantifiable differences in income ... They also miss the synergies, linkages and systemic effects that constitute the glue that bonds economies and societies together" (Reinert: 28). Related is a plea for common sense: a shoeshine boy in a Lima slum cannot be considered to be engaged in an economic activity qualitatively identical to that of Microsoft. When such activity specificity is taken into account, some theories (such as Ricardian trade theory) are downright dangerous. Trade can be beneficial, Reinert concludes, but only if both trading partners are on relatively equal footing.

The mathematization of economics has created the "same type of handicap for [economists] as someone writing a thesis on various types of snow would have if she or he chose to write in Swahili" (Reinert: 46). Because economists are so focused on quantitative factors and fancy equations, they are unable to take into account qualitative differences (such as differences in knowledge, entrepreneurship, and capabilities) and they eschew useful qualitative methods (such as the use of taxonomies). Reinert makes this point with rich examples of the nuanced arguments of prominent Enlightenment thinkers such as Antonio Serra and neglected contemporaries of today's better-known mainstream economists. Few of these forgotten scholars, Ferdinando Galiani, Friedrich List, Werner Sombart, Gunnar Myrdal, and Moses Abramovitz among them, would pass muster in the standard curriculum of most of today's leading economics graduate programs.

On the second flank, Reinert analyzes the history of implemented (as opposed to advocated) economic policies. This points to startling facts, including that in the century following the publication of Smith's *The Wealth of Nations* in 1776, England collected more customs duties than France, usually depicted as the far more stalwart protectionist. More recently, prescriptions based on the "magic of the market" have perpetrated disturbing deindustrialization in countries such as Mongolia, Peru, and Russia. Ostensible free-market "successes," such as Ireland, Chile, and Finland, Reinert notes, have benefited from restrictions on capital flows, state-run industrialization initiatives, and nascent industry protection, which all fly in the face of the prevailing wisdom.

The two prongs of Reinert's attack meet in a call for a less doctrinal approach, one that takes into account the importance of a diversity of economic activities, marked by increasing returns and synergies between sectors of the economy. Together, such synergies "produce the cumulative causations or reactions that create ... economic development" (Reinert: 35–6). Reinert offers a vivid example of Delft's economy in the 1650s, which benefited from the synergies between textile manufacture, glass lens production, and a naval industry. Demand for glass lenses in both textile and naval uses led to knowledge jumping between fields, and often over into both art and science, as exemplified by the close relations between painter Jan Vermeer and scientist Antoni van Leeuwenhoek, who both experimented with lenses in their work. A diversity of economic activities leads to unpredictable synergies, but is likely to lead to far greater wealth creation than agriculture or other decreasing-returns activities. Serra was already cognizant of this fact in the 17th century.

The current orthodoxy, Reinert concludes, cuts off avenues of development that were used for centuries by today's rich countries. As a result, in many poor countries, we observe today not progress, but "retrogression and primitivization." Rather than a Marshall Plan, which not only brought massive inflows of aid, but also permitted nascent industry protection in post-World War II Europe, today's development policies are more akin to its failed predecessor, the Morgenthau Plan. Such free-trade, open-market policies destroy the most advanced industries in the least advanced trading nations. In this way, they replicate the results of the Morgenthau Plan's postwar effort to deindustrialize Germany, which not only succeeded in weakening industry, but as a result also dragged down the agricultural sector, to catastrophic effect.

Ha-Joon Chang's work moves in a similar direction, pointing to the real-world consequences of theoretical debates. His book appears to be aimed, however, at the lay audience that is currently devouring economics bestsellers, thereby weakening its appeal to academics. Chang uses trite phrases such as the "Unholy Trinity" to describe the International Monetary Fund, World Bank, and World Trade Organization. More damaging to his argument are a few cases of historical error. In one case, he writes:

> In the 1960s and 1970s, per capita income in Latin America was growing at 3.1% per year, slightly faster than the developing country average. Brazil, especially, was growing almost as fast as the East Asian "miracle" economies. Since the 1980s, however, when the continent embraced neo-liberalism, Latin America has been growing at less than one-third of the rate of the "bad old days." (Chang: 28)

The implicit claim that neoliberalism is to blame for the "lost decade" of the 1980s obscures the historical record. In the Brazilian case he mentions, "neoliberal" reforms such as the opening of protected markets and the privatization of state-owned enterprises began only in the early 1990s. Further, these reforms were implemented in response to the distressingly lengthy crisis of the 1980s, itself generated by the debt-fueled, state-led growth of the 1970s. "Neoliberalism" can be blamed for the slow growth of the 1990s, perhaps. But to lay the blame for the 1980s at its door is to misrepresent history, and ignores neoliberal reforms' origins in the hyperinflation and debt crisis bequeathed by state-led development.

Despite such flaws, there are interesting facts to be gleaned from this book, such as Daniel Defoe's secret life as an economist (and a spy), Thomas Jefferson's stint as (a highly patent-averse) patent commissioner, and the "borrowing" of Edison's lightbulb technology by today's Dutch multinational Philips. Chang's critique of the "official history" of globalization is strident, even if the evidence provided is an unsystematic smorgasbord of anecdotes. But he does provide valuable and highly accessible discussion, for example, of the importance of state-owned enterprises in correcting market failure and overcoming problems of natural monopoly, of the historically unprecedented constraints imposed by today's intellectual property agreements, and of related themes that point to the weak underbelly of many conventional policy prescriptions.

Many of the most engaging arguments draw on Chang's earlier book, *Kicking Away the Ladder* (2002), so entitled after Friedrich List's criticism of Britain for "preaching free trade ... while having achieved its economic supremacy through high tariffs and extensive subsidies," in effect kicking away the very ladder it used to climb to success (Chang: 16). The title of *Bad Samaritans* points to a related issue: although some wealthy-country economists may be preaching "neoliberalism" knowing full well that it is a self-serving myth, more pernicious still are those "bad Samaritans" who preach these mantras because they believe in them whole-heartedly. Beliefs matter, in other words, and as a result it would be wise to get our theories and history right, long before teaching and applying them. The obvious confusion among development economists about which prescriptions in fact are effective should lead to more considered reflection before these are pushed on the poor.

But this recommendation of humility must be practiced as well as preached. Neoliberals are guilty of excess. But neither Reinert or Chang offer adequate reflection on past excesses in pursuit of their preferred policy path. After all, the Washington Consensus and its associated theoretical baggage did not emerge from a historical vacuum. History teaches us that in some cases, state-sponsored industrialization and industrial protection ground to a halt on their own failings, rather than being destroyed by excessive exposure to cold, heartless international markets. By way of example, while Reinert complains that opening Brazil's economy destroyed its nascent computer industry (Reinert: 181), he gives short shrift to the costs that inefficient industry imposed on other Brazilian businesses and consumers, relegated for years to the technological hinterlands by policies that cosseted a select few.

To his credit, Reinert does argue for a balance between excess protection and excess competition. After all, for every England or South Korea that pulled itself up by protectionist bootstraps, there are the Maoist Chinas or Mobutan Zaires that fail to benefit. Perhaps the most important problem this points to is the state. For, once the state is brought into industry-level choices, one of the biggest problems is keeping it neutral and honest. Indeed, at least some of the time, flawed policies are not imposed from abroad, but rather by an unholy domestic trinity of interest groups, politicians, and bureaucrats responding to their own self-interests rather than the broader good. That said, both Reinert and Chang offer an important corrective to often oversimplified and unexamined rhetoric in favor of "free" trade, privatization, and other "market solutions."

Remodeling Development Theory

The three remaining books begin with heterogeneity as a key consideration in development. Baumol et al. argue that capitalism is far from monolithic, especially in terms of the role of the state; Rodrik argues that while economics provides overarching principles that should guide policy, local knowledge and needs must be taken into account; and Collier asserts that the problems of the very poorest are singularly different from those of the rest of the world. For our purposes, the broad lessons of the three books can also be somewhat uncomfortably pigeonholed into three income categories: Baumol et al. is particularly useful for countries at the middle to high income level, Rodrik for middle to low, and Collier for the miserable bottom of the barrel.

According to Baumol et al., capitalism comes in at least four forms (entrepreneurial, big-firm, state-directed, and oligarchic), with the type significantly influencing possibilities for growth and poverty reduction. These distinct possibilities allow the authors to argue that a mix of entrepreneurial and big-firm capitalism is more likely to lead to growth, and, thus, is normatively preferable.

The problem with conventional economics, they argue, is that it does not take into account entrepreneurs, focusing instead on the effects of inputs (capital and labor) and total factor productivity (the increase in productivity of the inputs) on growth. While Solow received a Nobel Prize for showing that increases in total factor productivity were important drivers in industrialized countries' growth (in other words, that technology matters), innovation was exogenous to his model. Baumol et al. therefore suggest that innovation and innovators must be better incorporated. Policymakers need a simple framework for promoting a "successful entrepreneurial economy," in which entrepreneurs share space with larger firms that can refine, produce, and market their innovations.

Up until this point, much of the analysis of Baumol et al. echoes Reinert's emphasis on the importance of synergies and innovation. Methodologically, they too rely on arguments that are "heavily historical, logical, and even anecdotal rather than statistical" (Baumol et al.: 41), while also noting many limitations to statistical analysis. They criticize the ambition of the Washington Consensus (Baumol et al.: 54–6). In addition, like Reinert, they emphasize the importance of Romer's work on "increasing returns," which suggests that stimulating investments in specific innovative sectors of the economy will lead to productivity gains, and hence to self-reinforcing patterns of growth.

But for all this overlap, it is worth noting that a common diagnosis leads the two books to quite distinct prescriptions. Rather than emphasize foreign trade, Baumol et al. focus on local economies and institutions, and seem to assume largely innocuous effects from foreign competition (which may even serve the useful purpose of keeping local industry from going soft). Within this universe, they come to a seemingly conventional set of guidelines for policymakers: "it must be relatively easy to form a business"; "institutions must reward socially useful entrepreneurial activity"; "government institutions must discourage activity that aims to divide up the economic pie rather than increase its size"; and "government institutions must ensure that the winning entrepreneurs and the larger established companies ... continue to have incentives to innovate and grow."

In light of the role these recommendations foist on government, however, Baumol et al. are emphatic about the risks of industry protection. In advanced industrial economies, they worry about the diversion of entrepreneurial talent into "unproductive or destructive sources of wealth" (Baumol et al.: 229). Among these, litigation and interest-group politics are two key sources of ossification which can send an economy off the rails. Further, they emphasize the importance of innovative rather than replicative entrepreneurship, the latter "borrowed from abroad." In this, they echo Chang's concerns with the potentially development-retarding effects of intellectual property (IP) requirements, but draw somewhat distinct conclusions. Rather than worry greatly about the constraints IP protection poses for developing countries, they are concerned with the likelihood that replication (rather than innovation) will propel lower-income economies into industries with far less potential for "increasing returns."

Implicitly, one take-away lesson is that incentives matter, in everything from macroeconomic policymaking down to the level of the university technology licensing office. More explicitly, though, the recurring lesson is that countries can benefit from productive and mutually beneficial symbiosis between entrepreneurs and corporations. But how to achieve this tenuous balance remains largely unspecified. Ultimately, despite three thought-provoking chapters exploring these themes in Japan and Europe, the US, and developing countries, the conclusions vary enormously in breadth. They delve deeply into developed nations: in the case of the US, they exhaustively discuss the effects of Sarbanes-Oxley, bankruptcy law, and tort reform. With regard to the developing world, they are content to conclude that the discussion must become more systematic and more exhaustive, but "no one is yet in a position to provide such a definite elaboration of these matters" (Baumol et al., 184).

This reluctance to offer detailed policy prescriptions is one possible reaction to the crumbling of the prescription-laden Washington Consensus, and indeed, there is much to be admired in the self-proclaimed "humility" of the approach of Baumol et al. Another tactic is to develop contextualized solutions that draw on common economic principles. This is Rodrik's objective in his fascinating collection of essays. Although most of these have previously been published, this volume neatly ties them together with a concise introduction, as well as new synthetic chapters that elaborate on three key themes: growth, institutions, and globalization.

There is not space here to do each theme justice, but Rodrik's overarching argument is that, for all the criticism of the prevailing orthodoxy, neoclassical principles remain quite valid, thank you very much. Greater attention to context is needed, "not because economics works differently in different settings, but because ... environments differ in terms of the opportunities and constraints they present" (Rodrik: 4). First-order principles such as property rights, sound money, and competition can be achieved through quite distinct policies, and all come "institution-free," that is, without a single institutional framework that can or should be applied universally (Rodrik: 29).

Perhaps the best example of this "plasticity" of institutions is the fact that communist China performs better on rule-of-law indices than capitalist Russia. For all the institutional differences the choice of economic system entails, private entrepreneurs nonetheless felt more "secure [in China] not because the government was prevented from expropriating them, but because, sharing in the profits, it had no desire to expropriate them" (Rodrik: 189). In light of Chinese growth over the past generation, it is hard to argue "that a more standard, 'best practice' set of institutional arrangements would have necessarily done better"

(Rodrik: 24). Incentives, in other words, may be more significant than the form assumed by institutions per se.

Even as he has remained close to its proponents, Rodrik has been an important critic of the propagation of the Washington Consensus and its second-wave reforms, and has even offered a mea culpa for propagating "institutions fundamentalism" (2006). As a result, there is an earnest sincerity that underlies his reasoning: we have messed up, now let us try to find our way again. But at no point does he sacrifice his commitment to neoclassical economics, and as a result, he manages the seemingly impossible, squaring the circle between the critiques of the conventional wisdom while rescuing some of its most important foundations. This permits him occasionally to make statements that are reminiscent of Chang:

> [Developing countries] are being asked to implement an agenda of institutional reform that took today's advanced countries generations to accomplish. The United States, to take a particularly telling example, was hardly a paragon of free-trade virtue while catching up with and surpassing Britain. (Rodrik: 240)

But at the same time, Rodrik credibly goes further in defending some aspects of globalization, even as he argues for change:

> China and India would not have done nearly as well without access to relatively open markets of goods and services in the advanced countries. But their success was also due to their governments' concerted efforts to restructure and diversify their economies. (Rodrik: 2)

In other words, let us take the good and the bad, and avoid radical oversimplifications. But how to diagnose adequately the situation at hand without oversimplifying, on the one hand, but also without getting bogged down in details, on the other?

Drawing on work with Hausmann and Velasco, republished here, Rodrik argues that the Washington Consensus failed to generate growth precisely because it was (almost by design, given the universal pretensions it acquired over time) not targeted at the most important local-level constraints on growth. Three types of constraint are likely in low-income economies: "the cost of financing economic activity may be too high, the economic (social) return to economic activity may be too low, or the private appropriability of the (social) returns may be too low" (Rodrik: 89). Economists must diagnose which of these areas is the biggest constraint, and hence which reforms will generate the "biggest bang for the reform buck" (Rodrik: 89).

By diagnosing the biggest obstacles to growth, targeting them, and then zapping them with reform, it should be possible to move in fractal-like fashion from the most important issues of today down through the bottlenecks of tomorrow. A key message is that the type of binding constraint will shift over time: as financing issues are resolved, policy analysts may discover that returns to economic activity are the newest challenge. Sequencing the resulting reforms adequately is thus very important, lest previous reforms be undermined by inadequate institutional protections. But so too is policy targeting: aiming the policy response as close as possible to whatever distortions are identified, rather than engaging in broad-scale reforms across a wide range of issues.

Rodrik's strongest argument is that this diagnostic approach is beneficial because "it employs economists in their proper capacity: as evaluators of trade-offs instead of as advocates" (Rodrik: 95). But much as I found myself nodding along in agreement, three potential shortcomings are nonetheless worth noting. The first, which by now should be apparent, is that there is simply no reasonable guarantee that any two economists will ever identify and prioritize the same bottlenecks. Rodrik helpfully provides a model of potential constraints to growth that can be diagnosed from the ground up. But the skeptical reader wonders at the wisdom of expecting neutrality of any observer (even an economist). Even if they agree on the diagnosis, moreover, there is no guarantee they will agree on the relevant prescription.

Meanwhile, even if we assume such a diagnosis is possible, a second issue is the assumption that imposing reform is a simple choice. While economists can provide an invaluable service by identifying key bottlenecks, even when there is little overt opposition to reform there is little guarantee that reform will be undertaken if there is not a clear window of political opportunity. Politicians are not always driven by tomorrow's greater good. In light of these political complexities, rational sequencing seems a rather naive hope, especially in democracies, where a multiplicity of actors will simultaneously pull the agenda in multiple directions.

Finally, the medical metaphor of the diagnostic approach hides important differences between bottlenecks that seem likely to generate significant selection bias by these policy "doctors." Are measurable bottlenecks, such as financing shortfalls, more likely or less likely to make it to the top of the priority list than less quantifiable reforms, such as anticorruption initiatives seeking to improve the return to economic activity? One suspects that quantifiable concerns will be privileged. A related concern is that in some realms it is not exactly clear how you would go about reform: allocative efficiency requires the rule of law, but how, exactly, do we go about rule-of-law reform? More than a decade of rule-of-law reforms inspired by the Washington Consensus leave that answer more muddled than one might hope (Carothers, 2006).

These concerns aside, however, this is a thoughtful and wide-ranging consideration of the state of development economics. Equally important, Rodrik allows some grounds for optimism: economic theory may be rusty and in need of serious overhaul, but we need not start over from scratch. There is much that can be salvaged from the old school, and while self-critical, modest recognition of its limitations is needed, at its best, economics still does provide clarity of thinking and a set of evaluative policy tools that are enviable.

Collier's brilliant little book on the poorest of the poor nations, though, is a reminder that economics works best in combination with a clear cognizance of political and policymaking realities. Merging the three, Collier provides a crystal-clear diagnosis of the key issues facing the "bottom billion" of the world's 6 billion people, who are trapped in a "train that is slowly rolling backward downhill," worse off at the turn of the millennium than they were in 1970. This is without question the most urgent call to arms of all the contributions reviewed here, combining fast-paced prose, down-to-earth economics, an awareness of political, geographical and historical context, and realistic policy prescriptions.

Policymakers will appreciate Collier's no-nonsense style, which combines a clearly defined target (growth), subject (58 low-income nations), and problem (a lack of convergence with richer nations, complicated by four types of "traps"), as well as clearly defined solutions (various combinations of four policy instruments).

Throughout, he summarizes the results of his past research in highly accessible terms, and what his prescriptions lack in country specificity, he makes up for in two ways: first, by arguing that the bottom billion require distinct solutions from the rest of the world because their inability to grow is the result of a unique array of problems, many of which are not solely economic; and second, by recognizing the political and geographical dimensions to any "solution." In exemplary fashion, Collier's work thus suggests that multidisciplinarity, rather than trench warfare within economics itself, may be the best approach to what ails development theory.

By splitting out these nations from the rest of the world, Collier is able to argue that four "traps" are of fundamental importance to the bottom billion: the conflict trap, the natural resources trap, the trap of being landlocked with bad neighbors, and the trap of bad governance in a small country. In the countries of the bottom billion, 73 percent of the people have been through civil war, 29 percent live in countries where natural resource revenue dominates, 30 percent are in landlocked, resource-scarce countries in a bad neighborhood, and 76 percent have been through a prolonged period of bad governance and poor economic policies (Collier: 79). For those facing one or more of these problems, economic policy alone is simply insufficient.

Even for those who manage to escape these traps, Collier is brutally honest: "the global market is now far more hostile to new entrants than it was in the 1980s. The countries newly escaped from the traps may have missed the boat, finding themselves in a limbo-like world in which growth is constrained by external factors" (Collier: 6). "Good governance and policy help a country to realize its opportunities, but they cannot generate opportunities where none exist, and they cannot defy gravity" (Collier: 64).

So what to do? Reversing globalization is not an option. The last time globalization (trade in goods, flows of capital, and migration of people) was halted, between 1914 and 1945, the results were "ghastly." Deepening globalization is not much of an option, either: trade alone is not going to help the bottom billion, since on current trends in these countries, it seems more likely to lead to resource traps than to export diversification. Meanwhile, capital and labor mobility are "more likely to bleed them of their scanty capital and talent than to provide an engine of growth" (Collier: 175). Aid alone also is not the solution, because it does not supply the private capital which is needed to build basic private-sector infrastructure, and even worse, may crowd out productive activity.

The solution, Collier argues, is some specific combination of aid, security, laws and charters, and trade for each trap. Consider conflict traps: trade is not much use, and aid needs to be phased in over years, rather than dumped in one fell swoop once conflict ends. Most important, then, are security, including a lengthy external military presence, and charters, which permit the international community to infringe upon national sovereignty in support of the broader good. This will inevitably generate controversy, but as so often throughout the book, Collier forcefully points to his past research for justification, showing that civil wars typically cost around US$64 billion to their region, which is much greater than the likely cost of intervention.

As a second example, consider Collier's recommendations for addressing the resource trap. This is a clearly economic problem, leading to the so-called "resource curse" of bad governance, and Dutch disease, by which other productive activities become less competitive and are weakened by the presence of an extractive industry. But the solution Collier offers is not economic. Under such conditions, aid misses the point, and trade is not a reasonable solution because Dutch disease impedes export diversification. Security is not really an issue, leaving laws and charters, especially between developed nations, as the best way to address the problem. This may sound utopian, but Collier believes that a charter between commodity purchasers encouraging greater transparency in the use of the export revenues might help to empower morally courageous reformers within these countries. Alternately, "we can sit on our hands while our oil companies compete with the Chinese in the bribery game" (Collier: 179).

As for countries that have managed to emerge from these traps, but remain in limbo, Collier argues that trade protection is essential so as to nurture local industry. Interestingly, this is not protection against the West, but rather, against the Asian giants. In this regard, he echoes the seemingly growing consensus that free trade is no remedy for the ills of the poorest, and adds the terrible fact that the bottom billion's position is weak even by comparison with yesterday's poor.

How to achieve these solutions? Obviously, no single nation can resolve the issues raised here. But part of the problem, Collier notes, is that neither the development "biz," made up of development professionals at the various multilateral organizations, nor the development "buzz," made up of rock stars and activists, are really addressing the right issues. The "biz" is shortsighted, risk-averse and somewhat self-serving: the World Bank has large offices in the middle-income regions, for example, but not a single person resident in the Central African Republic. The development "buzz," meanwhile, "is at times a headless heart" (Collier: 4), which not infrequently sees global capitalism as the cause of the world's problems.

But even once some consensus has been reached on the solutions, a problem persists: "Remedying the problems of the bottom billion is a global public good, and so, like the provision of all such public goods, it is going to be difficult" (Collier: 183). Within governments in the developed nations, top leaders must crack heads together, since responsibility for each of the four instruments is typically located in ministries with distinct priorities. A coordinated approach will also be required between nations, and Collier argues the G-8 is the most fitting institution to achieve it. Agree or disagree with his prescription, Collier wisely calls attention to the need for concerted political effort and to the uphill battle it entails.

Whither Development?

Where do these very different books leave us at the end of the day? Development economics seems to be moving away from pretensions to nomothetic "covering law" science. This may leave it reduced, at best, to Rodrik's first-order principles. But this more modest approach is preferable to inaccuracy, especially in light of the obvious consequences of economic theory in the real world.

I close here with reflections on four issues, beginning first with the objectives of development. Economists have the singular disciplinary advantage of being able to agree on a quantifiable metric for development: economic growth. Several authors nonetheless expend considerable space defending growth as the appropriate metric for development, a task made easier by reference to

Friedman's (2005) moral defense of growth as the shortest path to other development goals. None, however, raises the possibility that some aspects of development may be as important as ends in their own right as they are as means toward growth (Sen, 1999). Given how little economists can say with certainty about how growth happens, one wonders if the newfound humility in the field should not prioritize some ends (such as education and health) as goods in their own right, even if they are not considered the most pressing constraints on growth in the short to medium term.

Further, it is not clear whether some forms of growth are better in the long run. By way of example, for all the trumpeting of China's success, the authors remain largely mute when it comes to the sustainability of this growth, the political ramifications of growth under an authoritarian state, or the desirability of development with such high environmental costs. Pulling 400 million Chinese out of poverty is undeniably one of the great development success stories of our time. But the point is that even such a simple criterion as growth conceals normative choices, both in choosing the path to growth and in the extent to which growth is given priority not just by economists, but by society at large.

A second issue is historical experience and the path dependence of policy choice. None of the authors here gives much thought to issues of context that may have contributed to the failure of the Washington Consensus, which figures so prominently in their arguments. Latin America, for example, did not do poorly in the 1990s solely because of its adherence to Consensus policies, but also because it was grappling with the legacies of some mix of civil war, the economic and political hangover from authoritarian rule, the effects of financial contagion (which were to some extent amplified by some Washington Consensus policies, such as openness to capital flows, but also ameliorated by others, such as floating exchange rates), and the difficulties of democratic transition and consolidation. Would "neoliberalism" have worked equally poorly under other circumstances? I do not wish to throw the Washington Consensus a lifeline, but the failure of economists to consider the potential counterfactual seems oddly out of character.

A third concern is the practical extension of the new thinking about development. Most of the authors use nuanced, careful language to avoid being drawn into cookie-cutter, one-size-fits-all prescriptions. But how will the recipes offered here be translated by governments and multilateral organizations in practice? One suspects an unavoidable trap for international bureaucracies: even when loaded with caveats, lessons applied across countries tend, by bureaucratic inertia, to be reduced to lowest-common-denominator prescriptions.

The final issue is how political science can contribute to the debate about development. In the past two decades, the field of development studies has been almost entirely abandoned by political scientists. As the acerbic debate shows, this may have been a hidden blessing. And yet, one wonders where the flesh and blood have gone in the field. The governing metaphor (with some variation in nuance) is medical: growth bottlenecks can be diagnosed, priorities drawn up, and plans prescribed.

But as past experience illustrates, this is a dangerous presumption: the only thing we can count on with any certainty is that governments will take the recommendations that are politically convenient and discard the rest. Prebisch, for example, influentially argued in the 1950s that Latin America needed a combination of agricultural reform and import-substitution policies. Not surprisingly, given the region's highly skewed land ownership, the end policy result was import substitution alone. While some of the authors recognize that first-best reforms are not always possible, the analysis often seems to assume that there will always be a clear choice between alternatives, and that the choices will not be imposed from below (for example, coca farmers' strikes in Bolivia) or from abroad (for example, New York banks).

As political economists have frequently warned, furthermore, the priorities of technocrats are often rewritten and redesigned in their passage through the political sausage factory and into the hard world of policy implementation. Even if one were to buy into the promise of technocratic economic policy recommendations that simultaneously are cognizant of local political reality and grounded in the best theoretical traditions (whatever these might be), one wishes for a bit more recognition of politics. Here policy scholars can make a difference, (re)introducing themes such as the differential treatment given to different policy types (Lowi, 1964, 1972); the nonlinear and often opportunistic nature of policy choice (Kingdon, 1984); problems of state capture (Migdal, 1994); and the uncertainty of policy implementation, even under highly auspicious conditions (Pressman and Wildavsky, 1973).

More broadly, political science has much to offer in thinking through the relations between economic and political structure, and the complex role of democracy and democratic institutions. With regard to the former, Reinert goes furthest here in criticizing the World Bank's tendency to assume that poverty arises because of weak institutions, rather than recognizing that institutions are often a reflection of poverty, and that "mode of production, technology and institutions" are all closely linked (Reinert: 55). Political science has long assumed relations of complex causality with regards to institutions, and thus may have much to offer to this debate.

With regard to democracy and democratic institutions, one would hope that this would prove to be to political scientists' comparative advantage, and that as a field we would have much to say about how to go about "strengthening the rule of law, solidifying democratic institutions, establishing participatory mechanisms, and erecting social safety nets" (Rodrik: 94). Certainly, political scientists have given considerable thought to various dimensions of the debate, and especially to how various combinations of institutions and incentives shape regime and government stability; the efficiency, efficacy, and equity dimensions of policy; and the resoluteness and decisiveness of policy choices.

Finally, the epistemological debate in political science seems to offer a path forward for today's economists. It has been little over a decade since the "perestroikan" revolt took place in political science, and many of the wounds are still chafing slightly. But, partly as a result, there is also a degree of ecumenical openness in political science that seems alien to the economists reviewed here. Given development economics' newfound emphasis on contextualization, development policy also seems especially likely to benefit from two traditions in comparative political science: area studies and cross-national comparative study. These offer both substantive foundations for policy choice and an example of successful methodological pluralism.

In sum, it is perhaps time for political science to be a bit less modest about what it can bring to the table. Economists are at a moment of transition in which they appear open to critical reflection, and there is evidence of a need for greater attention to political context. Multidisciplinarity appears to be one promising antidote to what ails development theory.

Note

1. I hope the reader will excuse the choice of books solely by economists in this leading journal of political science. But there is a clear paradigm shift ongoing in the field of economics, which will have repercussions in the policy and political debates of the coming decades. More parochially, this shift offers clear opportunities for political scientists, as I note in this essay.

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