One Economics, Many Recipes
A Crooked Timber Seminar on Dani Rodrik's Book

Edited by Henry Farrell

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One

Introduction

Dani Rodrik's new book, *One Economics, Many Recipes: Globalization, Institutions and Economic Growth* is a major contribution to debates on globalization, economic development and free trade. It brings together much of his existing work bringing together an important critique of the Washington Consensus with positive suggestions about how best to encourage economic growth, and how to build a global system of rules that can accommodate diverse national choices. We're pleased and happy that both Dani and several other guests have agreed to participate in a new Crooked Timber seminar. This seminar is published under a Creative Commons license (see below). The PDF includes all text from the seminar (somewhat reformatted), but not URLs to web pages – for those, you need to go to the online version, which is available at `http://crookedtimber.org/category/dani-rodrik-seminar/`

The (non-CT regular) participants in the seminar are, in alphabetical order:

- Dan Drezner blogs at `http://www.danieldrezner.com/blog/`. He is an Associate Professor at the Fletcher School of Law and Diplomacy, at Tufts University. He has written two academic books on international political economy (looking at sanctions and globalization), as well as a Council of Foreign Relations report and numerous articles. He possesses specific expertise on the intersection between celebrity culture and global politics.
- Jack Knight is Sidney W. Souers Professor of Government at Washington University in St. Louis. He is author of a widely cited book on institutional theory, *Institutions and Social Conflict* as well as numerous articles. He has a new book co-authored with Jim Johnson on rational choice, pragmatism and deliberative democracy, which will be published next year.
- Adam Przeworski is Carroll and Milton Petrie Professor of European Stud-

ies and Professor of Politics at New York University. He is the author of several monographs and numerous articles on topics including social democracy, democratic transitions and economic development. The interview available at `http://politics.as.nyu.edu/docs/IO/2800/munck.pdf` gives a good overview of his life, politics, and academic work.

- Dani Rodrik blogs at `http://rodrik.typepad.com/`. He is Professor of International Political Economy at the Kennedy School of Government of Harvard University, where he teaches on international development issues. He has written two books, copious numbers of academic articles and policy papers, and was recently awarded the inaugural Albert O. Hirschman Prize of the Social Science Research Council.
- Mark Thoma blogs at `http://economistsview.typepad.com/`, which has quickly become established as one of the key forums for debate of economics and politics on the Internet (with occasional interjections by Paul Krugman and others). He is professor of economics at University of Oregon, where he has published numerous articles on aspects of macroeconomics theory.
- David Warsh is the editor of "Economic Principals," available at `http://www.economicprincipals.com/`. He previously covered economics issues for *The Boston Globe* and *Forbes Magazine* for 25 years, and is the author of a widely acclaimed (and rightly so) intellectual history of the new growth theory in economics,*Knowledge and the Wealth of Nations*.

Two

David Warsh — Through the Hourglass

From his title on, Dani Rodrik is at pains to identify himself as a neoclassical economist, bred in the bone. He writes, "If I often depart from the consensus that 'mainstream economists' have reached in matters of development policy, this has less to do with different modes of analysis than with different readings of the evidence and with different evaluations of the 'political economy' of developing nations." Not to start an argument, if the book were about professional cooking, he might have called it *One Chemistry, Many Recipes (and Plenty of Chefs)*. True, economics is not very much like chemistry, but the reason for Rodrik's emphasis on the primacy of theory, I think, has less to do with the presence of economics' many competitors in the development game — political scientists, sociologists, lawyers, business executives, savants of all sorts — than with what happened in mainstream economics itself in the twenty-five years since he began his career.

Rodrik was just finishing graduate school when something called "the new growth economics" took off. To all intents and purposes, it began with a famous lecture in Cambridge, England, by Robert Lucas, of the University of Chicago. Lucas' reputation was as a monetary theorist, not an expert on growth. He had never traveled abroad before he spoke. Yet "The Mechanics of Economic Development" is the fifteenth most frequently cited of all papers in major economics journals since 1970, according to one careful survey. It was the first salvo in a barrage of papers that brought about a profound reshaping of the concerns of young economists. The new growth literature returned the wealth of nations (and lack thereof) to the center of their theoretical and empirical investigations, relegating to a lesser position the interest in distribution that had been dominant since Ricardo, and overshadowing for more than twenty years developments in traditional macroeconomic problems such as inflation, unemployment and the business cycle.

There are many versions of what the new growth economics was all about, including those of Paul Romer, "The Origins of Endogenous Growth," *Journal of Economic Perspectives*, Winter,1994, and Olivier Blanchard, Chapter 30, "The Story of Economics," * Macroeconomics*, 5th edition, Prentice-Hall, 2007. Here's my interpretation: Lucas' powerful rhetoric depended on asking a series of questions, not answering them. In Robert Solow's model of economic growth, dominant for thirty years, nothing much could be done about growth, aside from sound macroeconomic management. In Lucas' view, which was expressed in the formalisms that were becoming standard in economics in the 1980s, technological improvements (and the institutional arrangements that made them possible) could not be taken for granted. He pointed to immigration flows to pose a puzzle: why did people throng to cities, and to highly developed countries, instead of the other way around? Why didn't capital move freely to less-developed areas around the world? Lucas was interested in trade and growth. The hypothesis he advanced was that the engine of growth was human capital accumulation; that people went to cities because that was where the valuable skills were (just as money was in the banks).

Of course, practically as Lucas spoke, the pattern was reversing. Highly skilled people and capital were going back to poorer areas - to Ireland, to China, to India. Soviet-style communism was crumbling. Globalization was swinging into high gear. But meanwhile he had opened the door to a different answer, advocated chiefly by his student, Romer: that knowledge, nonrival and often partially excludable, was important, too, and that it possessed quite different economic attributes than goods do. Economists argued about it for a time, before the best among them agreed to stipulate that knowledge had an economics of its own, quite different from the economics of things. They left behind a handful of researchers to work it out, and moved on to the investigation of the institutional apparatus and social framework that created and distributed new ideas around the world.

Some years later, David Kreps described this sort of an evolution in terms of an hourglass, an image he attributed to Romer. Before World War II, Kreps said, economics had consisted of many different fields that had grown up in substantial isolation from one another: trade, development, economic history, labor markets, public finance and so on. These semi-autonomous discussions, with their focus on typologies and institutions, sounded like so many regional dialects, some of them all but incomprehensible to others. But after the war, as the advancing salient of economics — mainly macroeconomics and general equilibrium theory — embraced more formal methods, mathematical models in particular, the new techniques gradually were extended into one area after another (a movement often experienced as "colonization" by those previously working in the field). Gradual standardization of methods was the result. Previously disparate sub disciplines gradually came to sound more like branches of a single common (methodological rather than topical) tongue, wrote Kreps. And when, as was always the case, the new dialect of mathematical modeling lacked the appropriate vocabulary to discuss important topical features of the landscape, "rather than speak in an unfashionable dialect, some things were just not discussed," at least by those who felt as if they were on their field's cutting edge. Gradually techniques were developed to bring these familiar topics under the lens of the new mathematical economics and broaden the discourse.

> Hence the image of an hourglass. The vertical axis represents time, and the horizontal axis the scope or breadth of economics. As time passes, we first see a narrowing of topical concern as the language is unified and then a widening of concerns as the language develops. Throughout, it is important to note, the discipline of economics did not entirely abandon subjects such as institutional economics. Pockets of resistance to the evolution persisted; in some overseas locations and in some domestic departments, they dominated. The hourglass describes roughly the development of orthodox or mainstream economics, and, (at that), primarily in the United States. (Daedalus, Winter, 1997)

The excitement over the new growth theory has now passed on to "new" institutional economics and, more generally, to the "new" (meaning more formal) political economy. But while it lasted, the excitement had at least two unfortunate side-effects. The first was to greatly overshadow development economics as a discipline. Beginning in the years after World War II, development economics was an important new field, populated by original scholars who were thought of as giants — Albert Hirschman, Alexander Gerschenkron, W.W. Rostow, W. Arthur Lewis, Theodore Schultz, John D. Black, Peter Bauer. But while those scholars attracted plenty of fans, they didn't train many students of the next generation who could compete as stars with those who identified themselves as (mathematically trained) growth economists; Mancur Olson, Peter Timmer and Anne Krueger were probably the best-known among those who won prominence. Others, like Henry Rosovsky, left the field for other responsibilities.

The other side-effect in the 1980s and 90s was to add a little momentum to the impulse to send a lot of bright young theorists out into a world that was rapidly opening up. They were to serve as "country doctors." The content of their black bags consisted mainly of a understanding of the fundamentals of sound macroeconomic management described in 1990 by John Williamson as "the Washington consensus" and ordinarily interpreted as One Size Fits All. Central to the bedside manner of these advisers was their ability to speak fluently of the latest advances on the research frontier; detailed knowledge of the country in question was not required. Jeffrey Sachs, Lawrence Summers and the late Rudiger Dornbusch were among the most prominent. Countless teams were mobilized by the International Monetary Fund and the

World Bank as well. And even in a case where advice was being given by a theorist with intimate knowledge of the local economy, Andrei Shleifer, a Harvard University professor who had grown up in the former Soviet Union, the project collapsed amid charges of corruption among its leaders, a disaster for both US foreign policy and for Harvard.

Rodrik's heart has been in development since he started, at Princeton's Woodrow Wilson School of Public Policy more than twenty-five years ago, but he recognized immediately that he couldn't hang out a shingle except as a full-fledged practicing member of the community of mainstream economics — hence the Princeton PhD. Throughout the years in which the triumphant "Washington consensus" dominated, Rodrik taught, published and worked around the world on development problems, mostly behind the scenes. He retained his Turkish citizenship. In 1996, he moved from Columbia University back to Harvard, where he had been an undergraduate, and quickly built the Kennedy School's MPA in International Development into a leader in the field.

Rodrik's task now, as I understand it, and that of the many other economists in his community around the world, is to rebuild development economics on the firmer foundation of the new, more cosmopolitan economics — all the various "new" economics that have emerged since the 1970s, when game theory and decentralized stochastic general equilibrium models became part of the normal graduate student toolkit. He co-chairs Harvard's Growth Lab with Philipe Aghion, Harvard's leading growth theorist, and the two teach development together. In Many Recipes, he repeatedly demonstrates his familiarity with the finer points of theory (and they become fine very quickly), taking account, for example, explicitly and implicitly, of a couple of important applied papers in the lit — Murphy, Shleifer and Vishny on the concept of the "big push," Romer on the significance of an export-processing zone in Mauritius.

He tackles the literature even more directly in "Why We Learn Nothing Regressing Economic Growth on Policies," a paper too technical to be included in the book but available on his website, even more powerful for its elegant take-down of the economists' equivalent of digging holes and filling them in. These cross-country regressions were a outgrowth of the new theories of growth (if particular policies could affect the growth rate, why not compare their effects in countries all around the world? Especially since a great compilation of data on production, income and prices in 188 countries, knows as the Penn World Table, had just become available?

Rodrik's chapter on growth diagnostics, with Ricardo Hausmann and André s Velasco, is the heart of the book. There may be other ways to write the decision tree they have devised to identify the constraint on growth of a given country and identify the most binding one — it seems to me that human capital formation is understressed — but as a means of disciplining conversation about the experiences of disparate countries, I suspect it cannot be surpassed. It is, in embryo, a contribution equivalent in many ways to the Penn World Table, which also was devised by a handful of economists working at a distance from Washington, in that case at the University of Pennsylvania. Growth diagnostics deserves to be taken up and systematically elaborated by the World Bank, the United Nations or some other agency concerned with global development, as a means of getting beyond the platitudes of "the Washington consensus."

I especially like the concept of national "self-discovery" that Rodrik identifies as being the nub of the problem in, say, El Salvador. Clearly innovation is what is required if low levels of entrepreneurship and private investment are to be reversed — new markets for new goods, especially since the successful products can be scaled up for sale in the world market (which is the essence of new growth theory). But new ideas are easy to imitate, hard to protect. What it that El Salvador can uniquely trade in the world economy? Not just coffee, surely; that grows just as well in a dozen other countries around the world. Rodrik is particularly good at drawing out the implications in chapter four, "Industrial Policy for the Twenty First Century." Most significant instances of product diversification are the result of collaboration between governments and the private sector, he says, in East Asia and in Latin America. (He might have added that the same is true of the United States and Europe, too.) What's needed is to understand much better the differences between success and failure.

And this brings me back to Robert Lucas. What was so startling in 1985 about "The Mechanics of Economic Development" was the frank and surprising admission by the leading theorist of the University of Chicago that building new skills was at the heart of the problem. Sure, Lucas imagined an austere world, in which there were only two goods, potatoes and computers. And yes, the two models he brought to bear were highly mathematical. But both led ineluctably to a possible role for government in fostering growth. In the first model, a subsidy to schooling would enhance development, he noted. In the second, an industrial policy focused on "picking winners" might improve matters. Picking winners was easy in the model he had written down, Lucas wrote: "If only it were so in reality!"

In those days, Harvard's Kennedy School of Government and the business school across the river had a joint seminar discussing industrial policy that produced, among others, Robert Reich and Ira Magaziner. But Lucas' talk changed all of economics. On my reading, Rodrik understands that — it is part of what he means when he writes that "social phenomena can best be understood by considering them to be an aggregation of purposeful behavior by individuals — in their roles as consumer, producer, investor, politician and so on — interacting with each other and acting under the constraints that their environment imposes." Precisely because that he understands that mainstream economics, Chicago and Cambridge, now presupposes that industrial policy is as much a part of sound macroeconomic management as monetary or industrial policy, he is a regular rock star among development economists, quite able to stand toe-to-toe and slug it out on practical matters with any theorist.

Three

Henry Farrell – Many Politics, Many Recipes

A good way to start thinking about Dani Rodrik's genuinely excellent new book is to contrast its statement of objectives with a programmatic statement from another new book on international economics, Roberto Unger's *Free Trade Reimagined*. First of all, Rodrik:

> First, this book is strictly grounded in neo-classical economic analysis. At the core of neoclassical economics lies the following methodological predisposition: social phenomena can best be understood by considering them to be an aggregation of purposeful behavior by individuals — in their roles as consumer, producer, investor, politician, and so on — interacting with each other and acting under the constraints that their environment imposes. This I find to be not just a powerful discipline for organizing our thoughts on economic affairs, but the only sensible way of thinking about them. If I often depart from the consensus that "mainstream" economists have reached in matters of development policy, this has less to do with different modes of analysis than with different readings of the evidence and with different evaluations of the "political economy" of developing nations. The economics that the graduate student picks up in the seminar room — abstract as it is and riddled with a wide variety of market failures — admits an almost unlimited range of policy recommendations, depending on the specific assumptions the analyst is prepared to make … the tendency of many economists to offer advice based on simple rules of thumb, regardless of context (privatize this, liberalize that), is a derogation rather than a proper application of neoclassical economic principals.

Now Unger:

> [rethinking the traditional debate over free trade] is an intellectual task for which the present methods of economics are inadequate. It would be tempting to adopt a strategy of caution, insisting that economics, purged of abusive applications and restored to analytic purity, provides help, and imposes no obstacles, to such a campaign. In this book I reject that claim: its modesty does not make up for its falsehood. The practice of economic analysis inaugurated in the late ninetheenth century by Walras, Jevons, and Menger, which came to be labeled "marginalism" and which guided the mainstream of subsequent economic theory and culminated in the theory of general equilibrium, is not only insufficient to the execution of the task. It is also, in certain decisive respects, incompatible with it. If economics continues to swing between purity of analysis, retreating from all controversial explanatory and prescriptive ideas, and abuse of application, unjustifiably equating abstract conceptions like the idea of a market economy with particular contingent sets of economic arrangements, it will not open the way. It will stand in the way. There are many past and present varieties of economic analysis, from the old institutional economics to the new behavioral economics, that suggest different methods and directions. However, they have not developed — and maybe they cannot develop — into ways of dealing with the problems that are central to the argument of this book. Their characteristic inability to imagine the possible forms of economic life cramps their insight into its actual forms.

These quotes seem to reflect positions that are starkly opposed to each other. Rodrik is proposing that we can and must develop new ways of understanding trade and growth from within conventional economic theory — it's the only sensible way of laying out the issues. Unger claims that not only can conventional economic theory not help us to do this, but conventionally unconventional forms of economic theory such as behavioural economics can't do this either. Nonetheless, I think that Rodrik is closer to Unger than he presents himself as being (similarly, Unger is much closer to Rodrik and conventional economic reasoning than *he* says he is, but that's the topic for another blogpost). Rodrik provides an account of economic institutions that in many places rests less on neo-classical analysis than on a non-conventional account of institutional experimentation in a world characterized by uncertainty. Indeed, both Rodrik and Unger give us visions of international economics that deviates from the conventional account in two ways. First, they both provide *pragmatic* accounts — they are both much less interested in a Procrustean fitting of the world to an abstract theory than in figuring out what bits of theory help us to understand real life problems. Second, both explicitly acknowledge the *primacy of politics* — the market shouldn't be seen as a replacement for political decision making, but rather embedded in a political context where important collective choices are made through democratic means.

Enough about Rodrik and Unger — what does *Many Economics, Many Recipes* have to tell us? First, even I don't think that Rodrik delivers a systematically neo-classical book, he does very useful work, especially in the early sections, in clarifying the subtleties of the neo-classical approach, and in disentanglng it from the brutish simplifications of the so-called Washington Consensus. As Rodrik points out, there is *no necessary reason* that the latter follows from the former. Not only are the Consensus's prescriptions for institutional change - market liberalization, privatizating everything that moves, deregulation — not the only ways that we might think about reforming economies to improve economic growth, but they don't seem to work very well. Our experience of economic reform suggests that many of the countries that have embraced the Consensus most enthusiastically (mostly in Latin America) have done, by and large, rather dismally, while countries that have adopted different sorts of reforms have done much better. This isn't conclusive proof that the Washington Consensus is wrong — there are lots of confounding factors — but it is strongly suggestive. Nor, as Rodrik discusses at length, is the Washington Consensus the only — nor the most attractive — way to apply neo-classical principles to economic development.

Rodrik's alternative approach is likely to annoy *both* adherents to the aforementioned Consensus *and* many of their critics. While Rodrik argues that the drafters of economic reform need to be sensitive to context, he wants them to be sensitive in quite specific ways. He would like them to use neo-classical tools of analysis, but to think first about (a) which problems need to be tackled when, and (b) to think carefully about the possible unanticipated repercussions of reform. First, he suggests, you need to analyze what are the most important constraints on growth in a particular economy. Second, you need to analyse the specific distortions that are causing these constraints. Third, you need to think about policy instruments that can target these problems narrowly rather than trying to change everything at once. Fourth(he doesn't state this as part of his decision tree, but it's an important part of his argument), you need to make sure that whatever reforms you advocate don't have unanticipated repercussions elsewhere. The broader effects of reforms in a specific area (such as property rights or incentives to grow food) will vary between countries with different institutional settings, and second-best solutions that don't badly disrupt other parts of the economy are likely to be vastly preferable to first-best solutions that do. Finally, he argues (foreshadowing the second and third parts of the book) that short term spurts in economic growth aren't that uncommon, but they don't necessarily mean all that much either. What you need if you want to create long term economic growth is to institutionalize economic success by (1) encouraging diversification of trading sectors through appropriate public sector strategies, and (2)ensuring that domestic institutions of conflict management are strengthened.

This is an unusual set of prescriptions for an economist to be giving, but it doesn't deviate far from neo-classical orthodoxy if it deviates at all. It merely applies it in somewhat unorthodox ways. Where Rodrik begins, in my opinion, to really stray from the traditional account is in Chapter Four, the beginning of the section on institutions and industrial policy where he fleshes out what he means by appropriate public sector strategies and strengthening institutions of conflict management. After taking some entertaining swipes at the fainting spells that industrial policy produces in most economists (he points out in passing that many currently fashionable policy recommendations such as export zones are in fact industrial policy under a new label), he gets down to talking about why he thinks that states should be engaged in industrial policy, and how they should be doing it.

The why comes from standard economic reasoning — it's an incentives problem. Discovering new activities or products that can profitably be produced in a given economy is costly, and only a small fraction of the benefits can be captured by the entrepreneur who succeeds in finding such an activity or product. Therefore, we may reasonably expect that what Rodrik and his colleague Ricardo Hausmann call 'self-discovery' — the discovery not of fundamentally new products, but of products that are suited well to production under local conditions — will be undersupplied. Thus, there is a case for the government to come in and subsidize investments in non-traditional industries.

Rodrik's specific account of how this should be done, however, doesn't really rely on standard economic reasoning. And this is for good reason — economics, despite some significant advances, still has fundamental difficulties in understanding innovation because it involves decision making under uncertainty rather than risk. Thus, Rodrik resorts instead to a thoroughgoing pragmatism, grounded in common sense rather than in economic theory, to make claims about how self-discovery can best be promoted. Rodrik is certainly still sensitive to incentive problems, such as the risk that any industrial development agency will be subject to regulatory capture. However, equally (and arguably more) important in his account is the need for a process of pragmatic deliberation in which businesspeople and bureaucrats engage with each other to figure out what errors government is making, and how it can engage in targeted financial and logistical support and coordination for new activities that these sets of deliberations identify as likely candidates.

In short, the core sections of *One Economics* set out the virtues of a kind of pragmatic deliberation that can better foster self-discovery. Where standard economic theory enters in, it is as a corrective to the risks of regulatory capture. Attention to incentives is important if we are to design institutions to minimize the likelihood of collusion among the bureaucrats and business people who are involved in deliberation. But as Rodrik notes, there is a balance to be struck — trying to make regulatory capture impossible would rule out the information flows and processes of experimentation and argument that allow the government to help address the underlying problem of self-discovery. Attention to static incentives can help us avoid certain pitfalls, but actual discovery involves complex processes of deliberation and conversation. Interestingly, Rodrik borrows some of his arguments about deliberation from Charles Sabel, who is vehemently opposed to standard economic theory (he is a strong constructivist who doesn't believe that anything resembling stable interests or identities exist). While I don't think that Rodrik's argument requires him to buy into Sabel's stronger claims (there are more rational-actor-friendly accounts of pragmatic deliberation than Sabel's out there) his use of Sabel's ideas suggests that he is less wedded to the neo-classical approach than his opening statement would suggest.

This is in no sense whatsoever a bad thing (when you're looking to give practical advice to policy makers, you shouldn't let abstract theory get in your way when it's unhelpful or irrelevant) — but I'd like to see more in the way of micro-level grounding. If neo-classical economics doesn't provide us with a good grip on how self-discovery is likely to occur, there are other theories out there that at least provide some initial ideas of how best to think about innovation. Arnold Kling has already claimed Rodrik as a neo-Austrian. Economic sociologists also have some interesting things to say about these issues. Finally (and my personal preference) the more mathematically grounded variants of complexity theory (people working on the consequences of network topology for innovation, and agent based modelling as a means of capturing the importance of heuristics) have interesting things to say that might help flesh out Rodrik's practical advice, and give it more analytic bite.

It's also useful to highlight a more subtle way in which Rodrik deviates from the usual economic account — his discussion of the relationship between institutions and democracy. Here, even if the difference is one of normative position rather than explanatory focus, it's still quite important. The new institutional economics is strongly biased towards functionalist explanations (in which institutions come into being to fulfil certain broadly valuable functions), and towards explanations that fill inconvenient holes in conventional economic theory without challenging the fundamental emphasis on the primacy of markets as a means of social choice. This approach is the result of attempts from Coase through Williamson to(rather uneasily) North to explain how institutions can support certain kinds of functions that the market needs to work, without at the same time undermining basic claims about the virtues of freely functioning markets. This approach regularly slides from examining how political institutions may support market exchange to the tacit or explicit normative claim that political institutions are primarily valuable and good *insofar* as they support market exchange. It thus pushes for a quite narrow vision of politics (in which the state limits itself to protecting property rights, supporting impersonal exchange and so on), and in which democracy is seen as being rather ambiguous (it is good insofar as it limits the predatory aspirations of the state, but bad insofar as it allows for either interest groups or populist politics to interfere with market processes). Markets always come first.

While Rodrik favors some of the same institutions as do standard new institutional economists (e.g. effective property rights), it seems to me that his overall emphasis is quite different. This comes out most clearly in his discussion of trade, where he stresses that it is a means to an end rather than an end in itself. Democracies may legitimately choose to value other things than trade expansion, such as environmental and labour standards, and they should be allowed to do so. In Rodrik's words:

> Trade serves at best as an instrument for achieving the goals that societies seek: prosperity, stability, freedom, and quality of life. Nothing enrages WTO bashers more than the suspicion that, when push comes to shove, the WTO allows trade to trump the environment or human rights. And developing countries are right to resist a system that evaluates their needs from the perspective of expanding world trade instead of alleviating poverty.

It may be that there is a greater trade-off than Rodrik acknowledges between democracy and economic growth (Adam Przeworski suggests that Rodrik's empirical claims about this relationship are hard to substantiate given the virtual impossibility of establishing the direction of causation from available empirical evidence). But even if this were true, Rodrik's fundamental claim for the primacy of democracy rests less on its economic benefits than its normative attractiveness. Over the very long run, people should support a kind of global federalism (which Rodrik distinguishes sharply from world government), because it would allow them to exert democratic control over choices that are currently denied to them under existing multilateral institutions. Over the shorter term, trade rules should be changed so that they accommodate diversity better.

> Reversing our priorities would have a simple but powerful implication. Instead of asking what kind of multilateral trading system maximizes foreign trade and investment opportunities, we would ask what kind of multilateral system best enables nations around the world to pursue their own values and developmental objectives.

Furthermore, these different choices and values are worthy of respect *precisely insofar as they reflect democratic processes of choice*. Rodrik emphasizes that national standards should receive presumptive respect only if they are made by democracies, and thus reflect some reasonably fair process of choice and deliberation.

I hope Rodrik won't be offended if I say that this is the kind of claim one expects to hear from a political theorist, not an economist. It suggests quite emphatically that politics (more precisely democratic politics) should have primacy over markets. It points to the need for a set of international institutions that are not only better geared to support economic development, but that are democratically accountable either through national governments, or (perhaps in the future) through a combination of national governments and supranational democratic bodies. Reading through my own particular set of cognitive biases, it seems to me that a more accurate title for the book would be "One Economics (plus some extra-economic reasoning, Many Politics, Many Recipes." Or perhaps instead, Rodrik should write another book or long article that draws together the threads of his claims about deliberation and democratic choice — while there seems to me to be an underlying consonance between his prescriptions for industrial policy and his arguments about how best to reform the multilateral trade system, much of it is buried in footnotes and asides. Either way, this is a provocative and important book that should be read not only by economists, but by political scientists, political theorists, economic sociologists and anyone with an interest about how global economic processes do and should work. Most of these ideas have already been published elsewhere — but when brought together they pack a normative and analytic punch that they didn't as individual pieces. Good stuff.

Four

John Quiggin – If So Many Recipes Can Work, Why Do So Many Fail?

Dani Rodrik's book opens with a discussion of the policy approach that dominated the development debate for much of the 1990s, and to some extent still does. The term 'Washington consensus' was coined by John Williamson of the IIE, to described the views of Washington-based institutions (IMF, World Bank and US Treasury in the 1980s, but escaped from its creator and came to encompass a program of dogmatic adherence to a revived version of 19th century economic orthodoxy, commonly referred to as neoliberalism.

The 'Washington consensus' was notable for the extent to which it conflicted with earlier policy prescriptions for developed countries that encouraged state-led development. Even more striking is the extent to which it turned on recent, and in some respects short-lived, changes in thinking about economic policy in developed countries, most obviously the neoliberal surge associated with the Thatcher government in the UK. In this respect, at least the approach adopted in the 1990s was consistent with that of the past. Consistently, the dominant 'recipe' for economic development has involved rigid adherence to the policy approaches fashionable in developed countries at the time, or perhaps a few years previously.

The variety of policies that have been adopted at different times in developed countries provides another way of looking at Rodrik's argument. Looking at the experience of the developed world, it is immediately obvious that a wide variety of economic institutions are compatible with high and rapidly growing levels of income.

This is most obvious in relation to the longest-running policy dispute of all; that between free traders and protectionists. Britain's 19th century economic pre-eminence was achieved under the banner of free trade, but it was challenged and ultimately overtaken by Germany and the US, both of which relied on protection.

In most European countries,the long boom from 1945 to the 1970s was ushered in by a wave of nationalisations, and was accompanied by a steady growth in the role of government. But the reversal of these privatisations in the 1970s and 1980s was followed by a recovery from the economic dislocation of those decades. Particularly in the UK, many claimed a cause-and-effect relationship.

The US system of employment-based health care provides another example. The historically contingent outcome of policy processes in the 1930s, it differs radically (and in the views of most commentators, for the worse) from those in other developed countries. Yet it has delivered adequate health care to the majority of Americans, and the allocation of up to 15 per cent of GDP to this sector has not obviously harmed economic performance.

In summary, the experience of the developed countries supports Rodrik's central thesis. A wide variety of policy views have prevailed in different developed countries at different times, and all have proved compatible with high incomes and sustained economic growth over long periods. Disputes over the relative merits of alternative policy frameworks have mostly remained unresolved, since the statistical evidence is insufficient to settle them. Mainstream economics yields many policy recipes and no easy way of deciding between them.

But if so many recipes work, why are there so many failures? Given the availability of technology and capital from developed countries, standard growth theory predicts convergence of income levels between countries, but, as Rodrik shows, this has not happened. Instead, the number of countries falling behind the developed world in relative terms is about equal to those catching up.

A partial explanation is that some countries are caught in a low-level trap in which basic preconditions for growth like civil order and a functioning education system are missing. Under such conditions, the choice of economic policy may not really matter.

Second, some recipes really are bad, so bad that they have never been tried in the developed countries. Complete autarky, as implemented in North Korea, is an obvious example. Some of the experiments undertaken under the influence of the Washington Consensus, in Argentina for example, also appear to fall into this class.

These are partial explanations, but do not seem adequate to explain the variety of outcomes that has been observed. Rodrik's central argument is that different initial conditions require different policy approaches.

Again, the experience of the developed countries tends to support this view. Attempts to transplant policies from one country and institutional setting to another have had, at best, mixed success. There's plenty of room for debate about how tight these cultural/historical constraints are, but it's impossible to ignore them.

More specifically, Rodrik distinguishes between situations where the cost of capital is too high, situations where the appropriability of returns to economic activity are too low and situations where returns to economic activity are low because of inadequate human capital or similar factors. He suggests methods for identifying the binding constraint, and focusing on policies designed to relax that constraint, rather than the across-the-board liberalisation favoured by simplistic versions of the Washington consensus. This seems like a sensible approach, though there are no doubt plenty of difficulties in implementation.

Having looked at the extent to which policy fashions in developed countries affect the development debate, it's worth observing that the process sometimes goes in reverse. That is, the success of some developing countries in achieving high growth is used to argue that some part of their policy should be replicated in developed countries. All sorts of policies have been supported in this way, but the dominant fashion of the 1990s was to use the (presumed in advance) success of the Washington consensus to argue for the inevitability and desirability of free-market reform in developed countries.

This kind of thing has not entirely gone away, despite the failure of the Washington consensus in the countries that embraced it most fully. As Rodrik notes, we see the same kind of talk about China, even though rapid growth has coexisted with large-scale state ownership, financial suppression and restrictions of all kinds. However, such talk will convince only those who want to be convinced.

Five

Mark Thoma – Setting the Stage for Growth

Dani Rodrik's new book, *One Economics, Many Recipes: Globalization, Institutions, and Economic Growth* takes on a problem of fundamental importance, how to stimulate and sustain economic growth in underdeveloped countries and lift people out of poverty.

Past attempts to solve this problem can, for the most part, be identified with one of two polar extremes, solutions that involve pervasive and persistent government intervention, and solutions that rely upon extreme *laissez faire* market-oriented policies. Neither of these approaches has been very successful, and the book argues for a different approach that combines these extremes and allows market forces to operate in an environment shaped by government policy. Under this combination approach the government in partnership with the private sector uses industrial policy and institutional change to strategically kick-start, coordinate, and sustain economic activity.

If the barriers to development are difficult to identify, what should you do? One approach is follow a set formula such as the Washington Consensus. This provides a recipe to follow that is grounded in economic principles, relies upon markets to direct development activity, and is intended to be robust enough to work in a wide variety of circumstances. Unfortunately, there is little evidence that such a formulaic market-based approach works across the broad sets of conditions and institutions that exist in undeveloped countries. And the opposite approach, a heavy-handed, top down, highly interventionist, dictatorial approach does not seem to be able to find the keys to successful growth either.

The message is that too much reliance on either the government or the private sector has not, in general, produced the desired outcome of sustained long-run growth. To overcome this, the book recommends prescriptions that improve the information flow between the private and public sectors to reveal the important barriers to development. This calls for a collaborative effort between the government and the private sector devoted to identifying and removing the biggest impediments to entrepreneurial activity. A main point of the book is that although there are certain broad principles that guide the choice of industrial policies and institutional design, there is no one recipe that works for all countries. The endpoint is sustained economic growth, and the prescriptions are firmly grounded in traditional economic principles, but the exact path a country takes to reach its long-run objectives depends upon its unique circumstances and generally involves a combination of orthodox and unorthodox institutional practices.

While the first stage seems relatively easy to bring about, getting to the second stage, i.e. sustaining growth, is more difficult (the book lists over eighty instances of growth spurts, but only a few of those have been sustained over a long time period). As the book says, "sustaining growth is more difficult than igniting it, and requires more extensive institutional reform" and much of the discussion in the book is devoted to explaining a systematic approach to institutional design that promotes the necessary dynamism to sustain growth over the longer term.

Unfortunately, the general principles that explain the difference between the countries who make it to the second stage and those who do not are unclear. One of the book's messages is that such systematic differences are difficult to identify due to unique conditions in each country, but since making it to the second stage is the goal of development policy, I still wish we had a better sense of the factors that explain why most countries are unable to make the transitions needed to sustain economic growth.

Perhaps the book's discussion of a paper by Imbs and Wacziarg (2003) in the section on institutional design is related to this problem of determining which countries will survive the transition into the second stage. The paper estimates a typical development pattern and finds that development follows two distinct stages, an initial stage where sectoral employment and production become less concentrated and more diversified, followed by a second stage where this reverses and there is increasing sectoral concentration as the economy grows. In addition, the turning point is estimated to occur, on average, at relatively high levels of per capita GDP. Thus, graphing sectoral concentration against GDP per capita reveals a U-shaped pattern and, as Imbs and Wacziarg stress, the U-shaped pattern "is an extremely robust feature of the data." Based upon this they conclude that "Countries diversify over most of their development path."

This conclusion is based in part upon the result that the minimum of the U-shaped development path is at a relatively high level of income, but there is quite a bit of variation in the minimum across countries (partly explained by openness), and it is lower after 1980. In addition, the minimum is the point when the forces that are increasing concentration begin to dominate the forces that are decreasing it, but that is not necessarily the point where these forces begin to change.

What I am suggesting is that perhaps this process of clearing out unproductive, unprofitable firms is an essential part of getting to the second stage, and that this process must begin fairly early in the development process, earlier than the minimum point of the U-shaped curve. Initially, the clearing out doesn't fully offset the growth spurt and there is increasing sectoral diversification overall, but eventually the forces of consolidation come to dominate the forces of diversification as successful firms gain strength and this causes sectoral diversification to end as the economy passes by the minimum point on the U-shaped development curve. Without this process in place to clear the path for stronger firms to emerge, and without it beginniing fairly early in the devekopment process, growth stagnates before the country ever reaches the minimum point on the U-shaped development curve.

Perhaps it is the failure of this cleaning out process to operate due to government ownership of some firms, government protection of certain favored sectors, regulation, labor restrictions, etc., that is a factor in preventing countries from getting to the second stage. The book recognizes barriers such as these can impede development and one of the key guiding principles the book gives for partnerships between the public and private sectors, and in building institutions to support growth is the creation and preservation of 'dynamism.' In this regard, among countries experiencing growth spurts, it might be interesting to find out what the sectoral concentration profiles look like for the countries that were able to make it to the second stage versus those that did not, particularly a comparison of measures such as exit rates. More broadly, however, the question is whether there are deeper connections between the U-shaped concentration curve that appears to provide a very robust characterization of the growth profile of developing countries, and the first and second stages of growth identified in the book.

And this brings me to my last point. Whether or not there's anything to the conjecture above about stagnation due to the inability to clear out unproductive elements in the economy, a bigger message is that we need to learn more about the connections between the first and second stages of growth, i.e. about the transition itself. For example, what if removing the one or two most important impediments to jump-starting economic growth in the short-run is not the best means of getting to the second stage, or leads to a dead end where you cannot get to the second stage at all? Maybe some other development strategy involving the second and third most important barriers, say, won't give quite as much boost initially, but gives the country a much better chance of surviving the transition and sustaining growth over the longer term. The example is simplistic, but the point is that this is a single, interconnected problem, not two separate problems, and the first stage must be devoted to bringing about a successful transition to the second stage. The book does a great job of listing the guiding principles for each stage, and of describing how to design institutions to sustain growth, but I would like to see the connections between the two stages, particularly how to set conditions in the first stage so as to make the second stage more likely, explored in more depth. As noted above, the kick-start phase seems relatively easy to bring about and there are scores of instances of this happening, but getting to the second stage is much more difficult and perhaps there is more that can be done to enable the transition to take place. In any case, since so many countries fail after growth is initiated, the transition is something we need to learn more about and this book provides a solid foundation from which to explore this issue further.

Six

Adam Przeworski – One Economics?

The main point of Rodrik's book is that economics leaves a lot of slack for policy prescriptions. As I see it, this may be true for two distinct reasons. One is that economic knowledge is not sufficiently robust in general to indicate appropriate policies. Another is that it is inherently incomplete, specifically, that it cannot and does not consider all the factors that may matter in the particular situations to which policies are applied. Rodrik emphasizes the second reason but first I want to comment on the title.

To put it in a nutshell, is it "One Economics, Many Recipes" or "Many Economics, ..."? In what sense do we have "one economics"? Economics is a science that derives conclusions about states of collectivities ("the economy" but also "the polity," since the same methods are now applied to politics) from assumptions about preferences of individuals and the constraints they face. What unifies economics are the methods for making such inferences. The assumptions vary: they are at best disciplined by "stylized facts" and often at variance with more direct, psychological, evidence. Moreover, these assumptions often reflect ideological priors. One example that jumps to my mind is an article, published in a leading journal of political economy, that went like this: assume that tax revenues do not finance inputs into production, assume that they do not subsidize consumption, write a growth model, and – surprise – taxes are bad for growth! But almost the same year, another article, published in an equally reputable journal, assumed that public inputs are complementary to privateones and that tax revenues are used to finance public productive services, only to find that growth is maximized at a positive, indeed sizeable, tax rate.

One may think that even if the assumptions are to some extent arbitrary, they are at least disciplined by empirical tests of the conclusions. Assumptions are "as if," but conclusions must defend themselves against facts. But econometrics are a tolerant disciplinarian. As a frequent practitioner I am not cynical: some results withstand all specifications and all estimators, some results will not appear however one massages the data. But many are simply not robust. Econometric models are also based on assumptions and, indeed, these assumptions are so demanding that at least some of them are, perhaps always, violated. In the presence of endogeneity, errors of measurement, and omitted variables, all estimators are biased. And since different estimators correct for different biases, they often generate disparate results. Moreover, I suspect again that ideology has a way of sneaking even into econometrics. When in 1993 Limongi and I reviewed studies of the effect of political regimes on economic growth, we found that the results perfectly fitted the ideological climate of the period when they were published: before 1982 none found that democracies promoted growth, after 1982 none found that dictatorships did.

All this leads me to think that economics is one only with regard to the criteria of inference and evidence that are required to publish in the journals of the discipline. But with different assumptions and divergent statistical results, the resulting body of knowledge would not be unequivocal even if it were generally applicable to all possible contexts. The neo-liberal tenet that dismantling the state is sufficient for "the market" to usher a country on the road to development had little justification in the body of economic knowledge: after all, the theory says that economies stagnate if markets are perfectly, as distinctly from monopolistically, competitive. Yet this was the advice offered around the world by eminent economists.

Rodrik does not get entangled in these issues of philosophy of science because his point of departure is appropriately practical. His emphasis is that even if economics were one, the concrete situation of particular countries always includes factors that are not considered in general theories or that coexist in interactive combinations not entertained by such theories. And ignoring them may cause havoc. What we need, according to Rodrik, are theoretically and statistically informed analyses of particular situations. Such analyses must be consistent with what we believe theoretically and what we think to be true empirically, but they also must pay attention to facts specific to each context. And since different countries face different problems, these call for different remedies: his axiom is that "appropriate growth policies are almost always context specific" (page 4).

This is a salutary approach, appropriately cautious about general theories, appropriately wary about treating them as ready-made recipes, and importantly calling for considering the particular facts at hand. As I was reading the book, I remembered a public debate in 1990 about economic reforms in Poland with a prominent economist who had been an advisor to a Latin American country. At one time during the discussion I asked him what proportion of the Polish labor force was employed in agriculture. After some squirming, he gave the number of the country from which he had just returned, which was 40 percent of the true Polish number. No wonder Poland experienced unemployment rates nearing 20 percent during the following fifteen years. Hence, I am taken by Rodrik's approach to policy design: it is practical, responsible, and modest. Moreover, I found some of his analyses, particularly of industrial policy, exceptionally illuminating.

Yet studying specific situations is a craft, not a science. I am convinced by the spirit of "growth diagnostics" that seeks to identify the constraints to development that are most binding in a particular economic and political context. I also see the theoretical justification of the Hausmann-Rodrik-Velasco method for identifying such most binding constraints. Yet I am not sure that this approach leads to unique conclusions. Indeed, all the applications of the method I have seen rely on intuitive understandings. I can easily imagine someone applying the same method to El Salvador, Brazil and the Dominican Republic and arriving at conclusions different from those of Rodrik. By construction, when we treat situations as unique, at one point we have to stop relying on general knowledge, and then what else can guide us than intuition and perhaps caution?

The chapter on institutions diverges in tone from the pragmatism that permeates the rest of the book. We now get a general recipe: "participatory politics." Rodrik offers several regression analyses intended to support this recipe, but within the confines of this book he obviously cannot consider the endogeneity of institutions and other econometric issues. The impact of institutions on development is the new rage and even a narrower topic of the impact of democracy on growth has recently been subject to meta-analysis based on over 400 statistical studies. And is the observation that "Mauritius found its own way to economic development because it created social and political institutions that encouraged participation, negotiation, and compromise" (page 168) based on growth diagnostics?

In spite of my reaction against this chapter, it is still an advance over the view in which the political and economic future of a country was sealed by the first mosquito that bit a European settler when he stepped on its shore. I wish, however, that Rodrik had clarified the relation between two points he makes, namely, that a successful market economy can be sustained by a variety of institutional arrangements and that "institutions matter," even more narrowly, that "participatory institutions" are the engine of growth. If "there is no single mapping between the market and the set of nonmarket institutions required to sustain it" (page 162), then identifying the institutions that are compatible with sustained growth calls for a different methodology than Rodrik, and everyone else, uses. Yet the first point — that development can be sustained under different institutional frameworks as long as they manage conflicts, coordinate, and regulate — is tantalizing and calls for systematic attention. I am less taken by secure property rights, because property rights can be excessively as well as insufficiently protected, but this is a digression.

Altogether, this is a book that convinces by its intelligence and evokes trust by its modesty and its sense of responsibility. In spite of my specific doubts and disagreements, I believe that Rodrik has opened a new vista for policy analysis. After reading it, one can no longer believe in mantras, whether they are agreed upon in Washington or elsewhere. We have to pay attention to real problems that people face in particular contexts and seek to relieve their particular deprivations in ways that are suited to these contexts.

Seven

Daniel W. Drezner – One Book, Many Reactions

One Economics, Many Recipes elicited multiple reactions from this reader. As someone who's had to review development books for a public audience over the past few years, I found Rodrik's book to be well worth the read. As a political scientist, there were times when my wife asked me, "why are you yelling at the book?"

The first part of the book argues that the institutions that foster economic growth are highly sensitive to local conditions. Because local conditions vary so widely, it is logical that the policies that promote growth should also vary widely from country to country, while still adhering to a few key macro-conditions. In other words, there is such a thing as policy substitutability. While neoclassical economists are by and large correct about the necessary conditions for growth, the policy means through which those conditions can be met are much more heterogeneous than the augmented Washington Consensus suggests.

This emphasis on pragmatism and experimentation sounds perfectly sensible, and yet the faddishness that governs development and growth economics gives me pause. Rodrik is hardly the first economist in recent years to suggest a diagnostic approach to policy prescriptions. Even though they disagree on almost everything else, both Jeffrey Sachs and William Easterly stress the power of diagnostics and local "ownership" of development policy in their prescriptions on how to promote growth. I smell a fad here. The question is whether this fad is rooted in an improved understanding of the political economy of development, or is simply another new trend to replace passé but goodies like Harrod-Domar or the big push.

As a political scientist, *One Economics, Many Recipes* proved to be a bit more frustrating. It's far from clear whether the political organizations that Rodrik is counting on are up to the task. The book asserts that there are "pockets of bureaucratic competence in all states." Conflict-ridden states excepted (and this is an important exception in a book about helping out the bottom billion/a), this is undoubtedly true. Scholars of political organizations, however, might suggest that these areas of competence exist precisely because their existing missions are well-defined and clearly constrained. They might not want to branch out. Indeed, studies of successful agencies show that they will likely refuse new, high-profile tasks unless they are confident that their new tasks coincide with their current capabilities and practices. The possibility of failure, and a tarnishing of reputation, is the source of this risk aversion. Less successful agencies, in contrast, are more willing to take on riskier initiatives, as a way to grab power and enhance their reputation for competence.

The discussion of globalization and global governance also contained a few contradictions. The first is whether economic globalization severely restricts the policymaking autonomy of the nation-state. In chapter four, Rodrik assures us that this is not the case. Indeed, he assures us that states retain a wide array of possible "industrial policies" that could be pursued by governments. This includes signing preferential trade agreements. However, by the time we get to chapters seven and eight, Rodrik thinks the constraints of globalization are much stronger. In the extreme, he posits a governance "trilemma," in which it is impossible to reconcile global economic integration, mass politics, and national governance.[1] Which is it?

As someone who just wrote a book on international regulatory regimes, my answer is "neither." Globalization has not really transformed international relations. Governments that make the rules for large internal markets (the United States and European Union) retain the ability to determine the course of global regulatory and technical standards. Large markets have a gravitational effect on smaller actors. Their market power — and implicit coercive power — shape the preferences of smaller states and private producers. Simply put, when it comes to the setting of standards, great powers are price makers and not price takers. At the current moment, when the United States and European Union agree on a common set of standards, there will be effective coordination; if not, there will be no effective coordination. The important thing, however, is that it is the large market governments who retain the largest possible level of policy autonomy.

This leads us to the second contradiction. *One Economics, Many Recipes* devotes a great deal of effort to recognizing that for development policies to work, they need to be incentive compatible. China's agricultural reforms — cited numerous times throughout the text — were ingenious because they created a "two-track" system in which the winners did not affect the pre-existing set of politically entrenched actors. Rodrik takes great pains to try to devise metaprocesses through which governments can experiment with growth-promoting policies without falling into Olsonian

[1] Furthermore, this trilemma seems falsely posed. Precisely because governance takes place at the national level, and because the governance of the large market states operate by mass politics, there won't be a movement towards Rodrik's "complete economic integration" without a lot more consensus than exists today.

or Stiglerian traps. Whether sunset policies or deliberative councils would actually work is open to question. At domestic level, however, at least Rodrik gets that the incentive problems need to be factored into the equation — a vast improvement over the pie-in-the-sky proposals of Sachs or Stiglitz.

When we get to the global governance level, however, there is no discussion of incentive compatibility. The status quo allows the United States and European Union to dictate a wide array of regulatory strictures on smaller governments through a welter of multilateral and bilateral arrangements. Over the next decade or so, countries like China and India will be able to do the same. Crudely put, what's in Rodrik's proposals for these countries? Why would they voluntarily relinquish their power and influence for a nebulous form of "global federalism"?[2]

[2] On p. 208, Rodrik posit what he believes to be a Pareto-improving decision rule in which a "simple majority rule" decides a common global standard. A simple majority of what? States? Populations? Output?

Eight

Jack Knight – Experimentalism and Institutional Choice

In his new book Dani Rodrik argues that the primary question facing both scholars and policy makers in the area of economic development should be "how should the institutions of economic globalization be designed to provide maximal support for national development goals?" In the course of answering this question in a challenging and highly engaging way, he continually pushes the idea that "when it comes to industrial policy, specifying the process is more important than specifying the outcome." Quite appropriately he acknowledges that despite all of our efforts there is still a great deal that we do not know about the relationship between political and economic institutions on the one hand and economic growth on the other. And thus Rodrik recommends that we employ processes of experimentation as a way of developing a better understanding of which institutions might best facilitate growth in different contexts. In this regard he suggests that democracy might serve as a metainstitution for structuring this type of experimentation.

As a pragmatist I heartily endorse the focus on experimentation. And I share the belief that democracy can play a central role in facilitating institutional experimentation. Nonetheless, I think that the relationships between institutional choice and experimentalism and between experimentalism and democracy are more complicated than Rodrik's optimistic account might suggest. At the very least I want to suggest that the institutional implications of these processes warrant more extended consideration than Rodrik is able to give them in this very interesting work.

In some joint work with Jim Johnson I have pursued an analysis of the relationships among institutional choice, experimentalism and democracy. On our account, politics in most modern societies largely consists in contests over the contours and distributive implications of shared institutional arrangements. In any country there exists a plurality or range of feasible institutional forms. In addition to markets and democratic decision-making procedures this includes, but is not limited to, bureaucracy, adjudication through courts, private associations, economic hierarchies and social norms. Once we recognize this plurality, the task of choosing which kind of institution should best coordinate our social interactions in any particular setting appears quite daunting. This in turn raises the difficult task of discerning how, in such circumstances, any heterogenous constituency of actors, with their diverse and often conflicting interests, values and commitments, might determine which array of institutional forms to use to coordinate ongoing interactions across various social domains.

This task turns out to be a very complex one. As standard textbook accounts make clear in the case of markets, any available institutional form will operate effectively and thus generate normatively attractive outcomes only under particular (and in principle specifiable) conditions. In his book Rodrik puts great emphasis on the claim that the effectiveness of any set of economic institutions is context specific. It is important to understand what all is entailed by this claim. Most obviously, it implies that institutional effectiveness is a function of the social, economic and cultural factors that characterize a particular country. However, it also implies that institutional effectiveness depends on the presence of certain preconditions, such as, in the case of markets, the structural and participatory conditions necessary for effective market competition. So, in any particular context the task of institutional choice involves both the selection of an institutional type and a commitment to foster and maintain the conditions necessary for effective institutional performance.

Here it is important to emphasize that this does not merely characterize the choice of economic institutions. The effectiveness of any type of institution will be a function in large part of the conditions in which it operates. And to further complicate the task there has been much less systematic research about the effective preconditions of institutions like democratic decision-making and courts than about markets. Nonetheless, the task of institutionalizing democratic and judicial arrangements require that we make a commitment to establishing and maintaining our best understanding of the conditions that make democratic governance and courts effective in achieving the goals we impute to them.

This is where experimentalism enters the picture. We engage in institutional experimentation in order to generate knowledge about the effectiveness of institutions in various social contexts. Theoretical analysis can make important contributions to our understanding of issues of institutional performance but, as Rodrik forcefully argues, it alone cannot provide definitive answers to questions of actual effect. Such answers can only come from the cumulative experience of using the various institutions at our disposal. In the end we trust or value experimental outcomes insofar as they emerge under proper conditions and we expend much effort in monitoring those conditions.

The importance of "proper conditions" highlights the fact that experimentalism is itself an institutional choice. The type of experimentalism that Rodrik advocates will only be effective if the conditions are in place to let the process work as we intend it. To foster and maintain these experimental conditions we require some kind of institutional framework of monitoring and assessment. Here the logic of institutional experimentation starts to resemble an infinite regress of institutions and conditions and metainstitutions and metaconditions, etc.

But this is where the central importance of democratic governance for the type of experimentalism that Rodrik envisions becomes evident. We need institutions of democratic decision-making to be able to stop the dynamic of moving back from coordinating institutions and effective coordinating conditions to the monitoring institutions and effective monitoring conditions that allow these institutions and conditions to work, to the meta-monitoring institutions and meta-conditions, and so on ….

Any pragmatic account (such as Rodrik's account) that wants to avoid this infinite regress has to be grounded in some institutional arrangement that can satisfy the burden of self-regulation. Institutions of democratic decision-making are the only arrangement that offer such a possibility through the logic of their own operation. They are the one type of institution that can serve as the framework for institutional experimentation (including the tasks of monitoring conditions and assessing outcomes) and, crucially, monitor whether the conditions of their own effective operation actually obtain. This follows from an important characteristic shared by democracy and the logic of experimentalism: reflexivity. Reflexivity is a crucial feature of experimental procedures. And, as I have argued together with Jim, it is a basic characteristic of effective democratic governance.

With limited space here I cannot set out the full details of our argument, but its thrust is as follows. The reflexivity of democratic arrangements derives from the fact that political argument — again, under the appropriate conditions of freedom and equality — requires relevant parties to assert, defend and revise their own views and to entertain, challenge or accept those of others. It derives, in other words, from ongoing disagreement and conflict.

In this respect democratic institutions clearly differ from market interactions where, in the case of disagreement parties simply "exit" to seek more favorable terms of exchange elsewhere while in cases of agreement they simply trade without concern for the efficiency or distributive features of aggregate outcomes. Market interaction is decentralized in the sense that those who are party to any particular interaction are charged neither with reviewing or monitoring in an ongoing way the conditions under which the institution of exchange itself operates nor with assessing the consequences of how broader institutional arrangements operate.

It is just here that democratic institutions enable a level of reflexivity unavailable in other institutional forms. For political argument not only allows members of a democratic polity to collectively revisit past substantive decisions, it also allows them to collectively reconsider and revise the terms of their ongoing interactions. Moreover, because democratic arrangements structure political argument so as to allow the emergence of new ideas, perspectives and interests and, thereby, new constituencies and oppositions, this reflexivity has an endogenous, dynamic dimension. In practical terms it is less useful to characterize political debate as inducing agreement than to see it as structuring disagreement. The grounds for the claim that democracy is reflexive, then, emerge when we see how, when it is successful, it structures disagreement and, thereby, potentially fosters still further disagreement.

This, of course, does not guarantee that democratic governance will function to foster and maintain experimentalism as well as to stabilize the dynamic of effective institutional choice. The claim that I am making is, rather, a possibility argument. What I mean to emphasize here is that democracy, under the appropriate conditions, offers a better opportunity of achieving these necessary goals than do other available alternatives.

The fundamental importance of this is brought home when we acknowledge that institutional choice will commonly involve disagreement and conflict. Some sources of conflict may involve differences over interests and values, differences in the distributional consequences of institutional choice. Other sources may merely involve differences of opinion about the best way to proceed in the face of uncertainty about the present and the future. When such disagreement inevitably arises, the success of any institutional framework will depend on our ability to manage conflict and facilitate coordination wherever possible. Thought about in this way, a commitment to experimentalism is, like a commitment to democracy, really another instance of a more general commitment to ways of coordinating our various forms of disagreement.

One Economics, Many Recipes creatively and persuasively sets out a case for the benefits of this kind of institutional experimentalism for economic development and growth. As Rodrik continues to pursue these questions, I would offer two friendly suggestions. First, we need to give greater attention to what he categorizes as "the institutions of conflict management." Near the top of this agenda should be questions about the incentive effects of various institutional arrangements. For example, what incentives are necessary to get people to participate in such an experimental process? And what incentives are necessary to encourage them to accept both the costs and the resulting outcomes of the process? Second, Rodrik's first best proposal envisions experimentalism at the global level. If this is desirable, then we need to think hard about how we will foster and maintain conditions of effective experimentation in the international context. This strikes me as a much harder question than anyone presently writing on these issues seems to think.

Nine

Daniel Davies – The Undercover Apostate

Rather as Galileo and Newton used to make sure to profess allegiance to the doctrines of the Holy Church, *One Economics, Many Recipes* asserts firmly in its introduction that the book is firmly in the neoclassical tradition and that although substantial use is made of case studies, the author is a believer in the catchechism of econometrics — the validity of cross-sectional regressions as a means of extracting underlying structural facts. In actual fact, however, the first cross-sectional regression does not appear until page 170, and when it does it really does throw into sharp relief the weakness of cross sectional regressions relative to case studies (it's a regression which uses one of those Freedom House indices as if it were an unproblematic proxy for "democratic institutions")

Reading the book, I increasingly grew to believe that it's an intentionally subversive piece of work. Rodrik is correct in the introduction to say that he uses orthdox neoclassical approaches side by side with institutional methods, but the manner in which he does seems to be almost designed to show the neoclassical approach in a bad light. As the book progresses, the case studies and verbal arguments are so interesting and insightful, and the neoclassical models and regression results so thin and unproductive by comparison, it seemed to me that he was doing it on purpose, to show what dare not be said — that well-chosen and thoroughly researched case studies are almost always the best way to analyse something and that as a means of making meaningful cross-country comparisons, regression analysis is almost always dreadful.

Let's get specific here, because I think I disagree with Dani on what it means to be "neoclassical." The passage of interest is in "A Credo of Sorts", from the introduction.

> First, this book is strictly grounded in neoclassical economic analysis. At the core of neoclassical economics lies the following methodological predisposition; social phenomena can best be understood by consider-

> ing them to be an aggregation of purposeful behaviour by individuals — in their roles as consumer, producer, investor, politician and so on — interacting with each other and acting under the constraints that their environment imposes.

This is basically a statement of methodological individualism and fine as far as it goes. But if this was *all* there was in the way of a membership criterion for the class "neoclassical economic analysis," then I think that JK Galbraith would also be "strictly grounded" in it too — particularly as it's clear from the passage here and the context that "individuals" include people like corporate managers, government officials and even heads of state, who all of them have substantial ability to impose their will on the world as well as "acting under the constraints that their environment imposes". I am tempted to say that this in itself pushes Dani toward the border with heterodoxy — a lot of neoclassical analysis makes an assumption of methodological atomism which rules out this sort of agent. But like the small-country assumption in open economy macro, methodological atomism looks like the kind of thing that genuinely can be relaxed while staying in the overall paradigm, so I don't think that this can be grounds for quibbling with the book's neoclassical orthodoxy.

On the other hand, JK Galbraith *isn't* strictly grounded in neoclassical analysis, so if Dani's book is, then what's the difference between them? Basically, as far as I can see, there is another methodological assumption which is constitutive of neoclassical analysis; that the interactions between individuals and their environment are usefully describable by certain kinds of mathematical function.[1] What kinds? Well, any rigorous description will certainly have counterexamples, and I take the David Lewis approach to counterexamples, so I'll just say that the mathematical membership-criterion for neoclassical economics is defined by a Wittgensteinian family resemblance to Cobb-Douglas production functions.

And in this sense, the book certainly is strictly grounded in neoclassical analysis. But it achieves this grounding at the cost of introducing a methodological contradiction. If you read it straight through you don't notice this much, but flip back and forth and it almost looks like some chapters of the book are scolding others.

So in chapter 2, the central problem of institutional reform is set up in good neoclassical style as a second-best problem. The constraints on institutional form are expressed as the Lagrange multipliers of an optimisation problem, and the economist community is encouraged to think in terms of local shadow prices (rather than grand overarching schemes) in making its policy recommendations. Hurray for this, by the way.

[1] Perhaps this should just be "by mathematical functions," but I think it very much stretches conventional usage to consider Paul Davidson or Barkley Rosser as part of neoclassical economics. I think I'll return to this angle later.

There's no *real* contradiction here — Dani says the same thing again a couple of times in the more institutional chapters. But there is a *methodological* contradiction, because in order to set up the model in Chapter 2, the author has to help himself to a set of parameters tau which index the size of the distortions in economic activity and a set of parameters mu which represent the marginal valuations. Mu is perhaps a debate for another time and place, because the existence of utility functions is a background controversy for the whole of economics. But what about tau, the handy number which indexes the size of the wedge in every industry between social and private value as a result of institutional distortions?

Does any such function exist? Highly doubtful. If it did, would it be the right kind of function to ensure the existence of the relevant Lagrange multipliers? Even more doubtful. Chapter 6 agrees with me, discussing at length the complexity, irreducibility and geographical and historical specificity of institutions. Basically, national political, economic, legal and tacit institutions are the quintessential self-organising process. Most of them are how they are "just because". There isn't any index of institutional constraints on the realisation of investment returns, any more than the Freedom House numbers really index the degree of deomcracy in a country.

Now it is possible (as the book shows, by doing it) to say sensible things about institutions, and to make sensible predictions about what might be the effect of your actions upon them. But these sensible things and sensible predictions just don't have mathematical descriptions which are in the class of functions that are "neoclassical economics." Encouraging economists to think about institutional constraints as "shadow prices" is pretty sensible, even though there are already problems with that, since it is not always the case that different institutional constraints can be decoupled in the straightforward sort of way in which one would need in order to think of them as a price.[2]

But motivating them to think of institutional reform in this way by setting it up in terms of Lagrange multipliers is a bit funny, in my view. As far as I can see, the correct conclusion is the institutional one, Chapter 6 represents Dani's actual argument, and the main way in which neoclassical analysis is used in the book is as a means of

[2] What do I mean here? Well, let's think about auto engineering. What's the tradeoff between fuel efficiency and speed? Pretty straightforward — not completely so but you could probably draw up a reasonable schedule which would have enough shape to it to give some rough idea of the tradeoff between fuel consumption and speed. Now a more difficult question. What's the tradeoff between fuel efficiency and reliability? Not obvious at all. Usually a more efficient engine is also a more reliable one (just as a more stable computer operating system is also a more secure one). Does this tradeoff even exist? Is there a meaningful fuel consumption/reliability tradeoff and should you treat your estimate of the "price" of interval between breakdowns in mpg as representing anything meaningful at all? In my view, no. Lots of institutional tradeoffs are more like this (think about democracy and security) and therefore the idea of treating institutional reform as a shadow price might be a bit more difficult than one thinks.

persuading neoclassical economists.[3]

So in other words, I think that "One Economics, Many Recipes," is a rather more subversive book than it seems, and this is all to the good. The message that I really took away from it is that *there is no substitute for detailed factual knowledge*, which seems to me to be exactly right. Coming from the business school side of the street, it seems pretty obvious to me that this is the way in which economics (and not just development economics) ought to be going.

[3] "Economics is very useful as a source of employment for economist"; JK Galbraith

Ten

Dani Rodrik – Response

I owe Henry Farrell thanks for managing to get me such a thoughtful set of reactions from such a distinguished group of commentators. It is gratifying that the book's main themes appear to have resonated with these readers-even though of course there are many areas of gentle dissent and some real disagreements. I am struck as well by the richness of the diverse elaborations my commentators offer, suggesting that my very practical agenda may have come into contact with strands of intellectual inquiry of which I was perhaps only vaguely aware.

John Quiggin inverts my question to ask why so many growth recipes fail. As he notes, economists tend to see in successful economies those features that they want to see, while ignoring the rest. So in South Korea they saw outward orientation, but ignored the role that industrial policy played. Today in China they see the move toward market liberalization, but ignore all the institutional heterodoxy that allowed China to get there. This kind of myopia is an essential ingredient of the fads and fashions to which development "big think" is given.

David Warsh places my work against the backdrop of evolving intellectual traditions within mainstream economics, a story which he has traced out eloquently in his *Knowledge and the Wealth of Nations: A Story of Economic Discovery* and which he summarizes in his comment here. He is correct to emphasize that I come from a development tradition (I read Hirschman and Lewis before I read Samuelson and Solow), but that I recognized that one "couldn't hang out a shingle except as a full-fledged practicing member of the community of mainstream economics." He is also correct to say that I view my task as one of "rebuild[ing] development economics on the firmer foundation of the new, more cosmopolitan economics — all the various "new" economics that have emerged since the 1970s, when game theory and decentralized stochastic general equilibrium models became part of the normal graduate student toolkit." I am also gratified that he likes the Growth Diagnostic framework enough to make it required reading (and mulling over) by the World Bank and the UN.

Mark Thoma makes a number of interesting suggestions, in particular on what it takes to transform growth accelerations into sustained growth. He posits that maybe the transition has to do with a"process of clearing out unproductive, unprofitable firms … [as] an essential part of getting to the second stage and that this process must begin fairly early in the development process …" Perhaps. One can make his point more broadly not just about firms but all pre-existing institutions. At some point, inherited institutions can become dysfunctional-even if they served to ignite growth (think of China's property regime or Germany's welfare state). Figuring when you want to get out, while things are still going relatively well, is a complicated dynamic programming problem.

Adam Przeworski pays me a great compliment when he writes "this is a book that convinces by its intelligence and evokes trust by its modesty and its sense of responsibility." He is correct in emphasizing that there is a single economics "only with regard to the criteria of inference and evidence that are required to publish in the journals of the discipline." Indeed, the uniqueness that I claim is one having to do with methodology. He is also right in saying that "studying specific situations is a craft, not a science." But what I tried to suggest in the book and show through examples is that this unique methodology does help us navigate the complexity of specific local situations.

Przeworksi writes:

> I wish, however, that Rodrik had clarified the relation between two points he makes, namely, that a successful market economy can be sustained by a variety of institutional arrangements and that 'institutions matter,' even more narrowly, that 'participatory institutions' are the engine of growth. If 'there is no single mapping between the market and the set of nonmarket institutions required to sustain it' (page 162), then identifying the institutions that are compatible with sustained growth calls for a different methodology than Rodrik, and everyone else, uses.

He is quite correct here too. I think cross-national empirical analysis has so far gotten away because it has not focused much on the causal impact of specific institutional designs. Asking whether investors feel secure from expropriation is very different from asking whether laws takes a particular form. Asking whether civil liberties are protected is different from asking what specific institutional arrangements achieve that end.

Henry Farrell starts out by contrasting my work to Roberto Unger's recent book, also published by Princeton University Press. As he notes, our rhetoric may seem far apart, but we are a lot closer than it seems. Not a big surprise here. He and I have been discussing these issues intensely over the last few years, have taught together, and I cite him as one of my two major influences (the other is Ricardo Hausmann). It is quite possible that Roberto may have been, just slightly, influenced by me as well. In any case, I have been making the case for neoclassical economics to Unger over many years, although it would be foolish for me to claim success.

Farrell also says that I am less of a neoclassical than I presuppose, mentioning the parts on industrial policy and on the primacy of democracy. I suppose he may be right. I do make the case in the discussion of industrial policy that the standard principal-agent way of thinking on this issue is not very helpful (a point that I borrow from Chuck Sabel). And in my discussion on democracy, I am taking a normative stand, which does require me to leave my neoclassical baggage at home (at least if I am honest). And no, being taken as a political theorist does not offend me at all, although it does make me feel a bit like the bourgeois gentilhomme.

I am happy that Daniel Drezner liked parts of the book. As for the other parts, I wonder if his complaints are not the result of his having read them a bit too quickly. One complaint is that"It's far from clear whether the political organizations that Rodrik is counting on are up to the task." This is odd in light of the amount of attention I devote to how the requisite organizational arrangements can be designed (this is the non-neoclassical parts of the book that Henry Farrell talks about). It is also odd in light of a key argument in the book, namely that the diagnostic framework I advocate economizes on political and institutional resources, relative to the Washington Consensus and other approaches.

Drezner also says that I am inconsistent in my discussion of the constraints that globalization imposes on national policy. Again, Drezner does not seem to have read the book carefully, as he conflates a discussion of the current situation (in chapter 4), with a discussion of the direction in which we are headed (chapter 7), and both of these with a description of a theoretical ideal type (chapter 8). Finally, Drezner chides me for not asking whether the global economic arrangements I advocate are incentive compatible. But the point I am making in the book is precisely that such a system is in the interest of both advanced countries and developing nations-because it is the only one that can sustain the type of globalization that actually benefits them! Something that advances your interest is incentive compatible, no?

Jack Knight's fascinating discussion clarifies (and complicates) my scratching-of-the-surface on experimentalism. In the process, he greatly strengthens (I think) my argument. I learned much from his sophisticated discussion of the relationship among pragmatism, democracy, and institutional choice. A key point is that we need to provide a stronger foundation to the primacy of democracy as a meta-institution, something which the line of thinking that Knight advocates is able to do.

Daniel Davies makes the interesting point that my book is purposefully subversive: I intend to show what a disaster neoclassical economics really is by contrasting it to non-neoclassical modes of reasoning, and letting the readers make up their own minds. While I am open to this idea, I agree only partially with the argument that he makes. I think the problem resides with Davies' definition of neoclassical economics, which crudely put is: methodological individualism + math. I don't see math really as being key here. Some brands of Marxian economics are much more mathematical than anything I do.

Also Davies has issues about math, which I did not quite follow. For example, I do not see any inconsistency between discussing a wedge between social and marginal valuations ("tau") as a conceptual construct and granting the empirical difficulty of identifying it in practice. It is still the case that the concept clarifies what we should do in practice (and indeed the "lambdas" if not the "taus" are what the growth diagnosis tries to uncover). Davies is right that there are some tensions in my work between the neoclassical pretensions and the occasional non-neoclassical colorations (as Farrell also notes). Where that is coming from is well explained in Warsh's contribution.

OK I have to stop now before I split too many hairs and abuse the patience of the gentle people who have done me the favor of paying close attention to what I have written. I am delighted with the warm reception the book has received. I am also delighted that it has served as a springboard for further thoughts in so many different directions. With these kinds of reactions, it is hard not to feel happy.