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What Professor Rodrik Means by Policy Reform: Appraising a Post-Washington Paradigm

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Abstract

This article reviews Professor Dani Rodrik's work on growth and development. The review first provides an outline of Rodrik's critique of the Washington Consensus and his alternative 'post-Washington' paradigm for formulating and implementing growth strategies. The remainder of the paper then critically assesses some key elements of this alternative vision for development. In particular, the critical analysis focuses on Rodrik's institutionalist revision of the existing growth evidence, his proposed changes to the way in which economists formulate policies in a post-Washington era, and his call for greater democratization as a universal institutional reform.

Keywords: Washington Consensus, growth strategies, revisionism, institutionalism.

I. Introduction

In 1990, John Williamson attempted to identify *What Washington Means by Policy Reform*. This consisted of a ten-point liberalizing reform agenda which, at that time, policymakers in the US government, the IMF and The World Bank all agreed upon.[1] But whilst this 'Washington Consensus' arguably was the most influential development paradigm of the 1980s and most of the 1990s, it is a reform agenda which has increasingly come under attack from a broad spectrum of eminent development economists whose interpretation of development theory and experience differs significantly to Washington's policymakers.[2] Arguably at the forefront of post-Washington proponents is Professor Dani Rodrik of Harvard University. Professor Rodrik has published a large body of work for two decades now which directly and indirectly criticizes the development strategy of the Washington institutions. He is also a widely demanded public speaker on this topic,[3] and his good name is liberally bandied about wherever development economists choose to gather in large numbers. But the surest sign that his efforts to significantly alter Washington's approach to reform have finally met with some success is the publication of a recent World Bank report (The-World-Bank, 2005) - *Economic Growth in the 1990s: Learning from a Decade of Reform* - which provides a fresh and surprisingly critical appraisal of the so called Washington Consensus. Indeed, Rodrik has recently provided a very favorable review of this report (Rodrik, 2006), and the report itself has a bibliography which includes no less than twenty of Professor Rodrik's works, far more than any other author referenced therein.

And yet, despite the increasing notoriety of the work of Rodrik and his collaborators – including the various authors of *Learning from a Decade of Reform* - and some clear enunciation of precisely what it is this emerging school of thought rejects, it seems rather more difficult for the occasional reader to definitively say what it is that Professor Rodrik and his collaborators definitively embrace. Is Rodrik a free or strategic trader? Is he a supporter of government-led industrialization? Is he a shock therapist or a gradualist? Is he an

[1] The specific ten points of the Washington Consensus are discussed in more detail in Section III (see Table 1).
[2] In September 2004, 16 well-known economists - Olivier Blanchard, Guillermo Calvo, Daniel Cohen, Stanley Fischer, Jeffrey Frankel, Jordi Galí, Ricardo Hausmann, Paul Krugman, Deepak Nayyar, José-Antonio Ocampo, Dani Rodrik, Jeffrey Sachs, Joseph Stiglitz, Andrés Velasco, Jaime Ventura, and John Williamson - gathered in Barcelona, Spain, and, as a result of their meetings and deliberations, issued a document containing a new post-Washington consensus on growth and development, termed the Barcelona Consensus.
[3] Professor Rodrik has been an invited speaker at a range of prestigious events, including the WIDER Annual Lecture (November 2004), the Alfred Marshall Lecture of the European Economic Association (August 1996), and the Raul Prebisch Lecture of UNCTAD (October 1997).

institutionalist, and if he is, what kind of institutionalist? Considering how widely cited Professor Rodrik and his colleagues are, it is unusually difficult to associate his work with clear-cut answers to these questions.

Thus the first objective of this review is to provide an overview of Rodrik's criticisms of the Washington Consensus, and a synthesized outline of his alternative 'post-Washington' development paradigm. Section II begins by reviewing the background to Rodrik's work, namely the rise of the Washington Consensus and its key policy elements, as synthesized by Williamson (1990). However, Section II also contrasts Williamson's *What Washington Means by Policy Reform* with the very different set of prescriptions proposed by Robert Wade (1990) in *Governing the Market*. I will argue that these two references are invaluable for understanding Professor's Rodrik's more recent works, the key arguments of which are outlined in Section III. That outline also shows that Professor Rodrik does indeed address the types of questions raised in the preceding paragraph, although his answers are rarely of the definitive 'yes or no' variety. Instead, like many post-Washington development economists, Rodrik seems to suggest that an unbiased reading of development experience to date elicits an interpretation that is far more nuanced than paradigms based on any one 'big idea' (Lindauer & Pritchett, 2002) or development 'panacea' (Easterly, 2002). Indeed, Professor Rodrik's greatest contribution to date is not one grand theory of *why* countries develop. It is instead a revision to the way in which economists and policymakers might interpret the development experiences of the recent past, as well as a series of proposals for such experts might better formulate development strategies in a 'post-Washington' world.

The second objective of this paper is to critically assess Professor Rodrik's 'revisionism'. Section IV addresses this objective by primarily focusing on those elements of his vision which I see as the most contentious. For example, Section IV will mostly overlook the issues of whether the Washington Consensus has really failed, and focus instead on Rodrik's institutionalist interpretation of development history, his binding constraints growth diagnostic approach to growth strategizing, and his advocacy of democracy as a growth-enhancing 'meta-institution'. Along the way I will also contrast Rodrik's revisionism with other heterodox economists, especially the aforementioned Robert Wade, as well as earlier institutionalists such as Irma Adelman and Gunnar Myrdal. Finally, Section V will provide a few concluding remarks as well as some overall assessment of Professor Rodrik's contribution to development economics in a post-Washington era.

II. Background: What Washington Meant by Policy Reform.

Since of much of Rodrik's work on development strategies is based on a critique of the Washington Consensus, it is important to reiterate as precisely as possible what that Consensus entailed. Williamson's (1990) original use of the term attempted to outline a 10-point reform agenda upon which key US government and Bretton Woods policymakers broadly agreed. These ten policy instruments are listed in column 2 of Table 1, but they essentially comprise an across-the-board liberalization objective. Additional characteristics of Washington's reform agenda, which more directly relate to governance issues, are listed in Table 2. A salient point of the Washington reform agenda described in both Tables 1 and 2 is that this reform strategy was advocated for a large number of countries, first in Latin America in the 1980s, but then increasingly in Africa and Asia. And thanks to the technical assistance programs and the conditionalities embodied in their structural adjustment packages of the IMF and The World Bank, many developing countries closely followed these prescriptions. So the widespread adoption of these reforms implies that the Washington 'experiment' was not short of subjects.[4]

By a happy coincidence, in the same year that Williamson outlined Washington Consensus, Professor Robert Wade – now of the London School of Economics but previously with The World Bank - published his widely read *Governing the Market*, a heterodox revision of the contribution of East Asian governments to the remarkable economic success of that region (especially Taiwan, China). In Chapter 12 of that text Wade outlines his own 10-point reform agenda based on his revision of the East Asian evidence. This agenda is relevant to the present discussion for two reasons.

First, Wade's revionist agenda provides a strong contrast to the Washington agenda. A perusal of the final columns of Tables 1 and 2 below indicates that Wade's analysis of East Asia's success led him to propose a growth strategy which includes significant government leadership in guiding the economy through the complex process of structural transformation. Hence he proposes strategic trade and investment policies to shift production into sectors of future importance for the national economy. A second and somewhat overlooked feature of Wade's prescriptions relate to political economy and

[4] One caveat here, discussed further below, is that many developing countries at least partially resisted some of the Washington Consensus reform efforts (especially in Africa).

governance (Table 2). Wade heavily stressed the conditions under which bureaucratic and political decision-making could be improved. In particular, Wade warned of the dangers of 'shock therapy' and across-the-board reforms (just prior to their disastrous use in Eastern Europe), as well as the risks associated with premature democratization. Wade is also skeptical that there is any good evidence that provides general support for decentralization.[5] He instead emphasized the importance of growth coalitions between government elites and industrial groups, and the need to create an elite bureaucratic cadre for the purpose of fostering industrial transformation. The existence of a 'growth coalition' and an elite bureaucracy would also justify less dependence on discretionary rules, which the Washington policymakers regularly favor for the presumed robustness to government corruption and incompetence.

Wade's heterodox vision is relevant for another reason, for it bears strong similarities to much of Rodrik's work on the benefits of trade liberalization, the importance of strategic trade and industrialization policies, and the non-equivalency of neoclassical theory and practice, as I explore below. Rodrik, in fact, might also be classified as an East Asian revisionist (Rodrik, 1986, 1987, 1988, 1989, 1994, 1995a, 1995b, 1995c, 1996a, 1997a, 1997b), although much of his recent work takes a more global perspective. So I will ask readers to bear Wade's growth strategy in mind as explore the more nuanced approach which Professor Rodrik has proposed.

[5] Professor Wade's views on decentralization are not directly based on statements in *Governing the Market,* but are instead based on personal communications with the author.

Table 1. Contrasting two existing reform paradigms: Williamson's (1990) Washington Consensus vs. Wade's revisionist prescriptions.

Policy domain	**The Original Washington Consensus**[a]	**Wade's (1990) Revisionist position**
Size of government	1. Fiscal discipline.	Cautious agreement.
	2. Reorder public expenditure priorities (i.e. from production subsidies to social sectors & infrastructure).	Disagreement: At the margin sectoral expenditure on agriculture and industry, to change production structure, may be just as important as social expenditure.
	3. Privatization.	Disagreement: Public ownership in upstream industries may be sensible.
	4. Tax reform (broaden base, moderate marginal tax rates).	Partial agreement only: Tax base can include substantial customs revenue.
Finance & Trade	5. Financial liberalization (formerly "liberalization of interest rates").	Disagreement: Governments should own some banks, provide selective credit for strategic industries, impose substantial capital account controls, and only liberalize financial sector late in reform process.
	6. Competitive exchange rates.	Partial agreement only: There may be a case for an undervalued exchange rate as a spur to industrialization (see above and below).[b]
	7. Trade liberalization.	Disagreement: Liberalize strategically, use protection to support infant industry (but withdraw from non-infants and perpetual infants), liberalize imports for exporters, quantitative restrictions can be useful.
	8. FDI liberalization.	Disagreement: In the general case FDI flows should be conditional upon domestic content requirements, export requirements, foreign exchange restrictions.
Competition	9. Deregulation (entry and exit barriers).	Disagreement: Limit competition if there are economies of scale and coordination failures.
Property rights	10. Grant private property rights when costs acceptable.	Partial agreement. There can be functional alternatives to property rights and their enforcement as defined in the West.

Notes: [a] The 'Original Washington Consensus' refers to Williamson's (1990) original ten-point agenda, not to the subsequent augmentation of this list which Rodrik's has termed The Washington Consensus Mark II. [b] Wade is also skeptical that it is feasible to define competitive exchange rates via theoretical means, and skeptical that free markets will achieve competitive rates.

Table 2. Other Washington Consensus reform strategies vs. Wade's revisionist prescriptions.

Policy domain	**Other Washington reform strategies**	**Wade's (1990) revisionist position**
Reform scope, speed.	Wide, quick.	Disagreement: Reforms should be sequential, gradual, and targeted at real constraints in a given time period, in line with limited public implementation capacity.
Political structure	Democratization.[a]	Disagreement: There is no good evidence in support of a general presumption in favor of democratization. But irrespective of the political system in place, policymakers should develop corporatist 'growth coalitions' between government elites and industrialists.
	Decentralization.	Disagreement: There is no good evidence in support of a general presumption in favor of decentralization. More emphasis should be placed on creating an elite bureaucratic cadre capable for the purpose of guiding industrial transformation.
Scope for policy discretion [b]	Limited: rules are favored over discretion (e.g. fiscal discipline, monetary policy, tariff policies).	Disagreement: There is no general principle or evidence favoring the limitation of discretion for policymakers in developing countries.

Notes: [a.] The World Bank and IMF do not have mandates to intervene in the political processes of their client countries, so this criterion only relates to US government institutions. [b.] Examples of Washington policymakers favoring discretion which are listed in Williamson (1990) include: fiscal deficits constrained by debt to GDP ratios, the removal of import licenses and highly differential tariff structures, a preference for floating exchange rates over discretionary systems, preferences for privatization over public management, and concerns that government regulation breeds corruption.

III. The Rodrik Critique.

What, then, is Rodrik's own reading of development experience to date, and what are the practical conclusions to be drawn from this interpretation?

III.1 The practical problem: the failure of the Washington Consensus.

The relatively uncontroversial part of the critique made by Rodrik and the Barcelona Consensus is that the Washington Consensus reforms have produced disappointing results on average. The apparent failure of state-centric approaches to development in the 1970s seemed to warrant enthusiasm for a much more liberal policymaking paradigm in the early 1980s. Thus The World Bank and the IMF began to make their concessional loans conditional upon the recipient government's enacting a wide range of liberalizing reforms in trade, government finances, monetary policy and interest rates, state owned enterprises, foreign investment, and regulatory policy. However, Rodrik sees this paradigm as having failed in two dimensions. First, when liberalization has been carried out more or less wholesale – as in Latin America – the results have been disappointing (Rodrik, 2001, 2005a). Growth has often accelerated very little or not at all, and the impact on the poor has been unfavorable or negligible. Second, liberalization reforms have often been rejected or heavily watered down in much of Africa and parts of Asia because the liberalization blueprint has proved to be unpopular with policymakers and/or voters (Rodrik, 1996b).

III.2 The methodological problem.

The failure of the Washington Consensus appears to be paradoxical given the large body of empirical evidence demonstrating the importance 'market friendly' policies. Surely countries pursuing inward-oriented import substitution industrialization (ISI) policies have grown more slowly relative to the outward-oriented export-promoting East Asian miracles (Balassa, 1971; Krueger, 1978, 1983; Krueger, Schiff, & Valdes, 1991; Little, Scitovsky, & Scott, 1970)? Surely LDCs have often engaged in bloated government expenditure practices which encouraged corruption, cronyism and inefficiency (Ades & di Tella, 1997; Krueger, 1974)? Surely the empirical macroeconomic literature – i.e. growth regressions - has amply demonstrated the superior performance of more liberally oriented economies (Burnside & Dollar, 2000; Dollar, 1992; Sachs & Warner, 1995)?

By and large Rodrik rejects these empirical conclusions. For example, whilst it is true that developing countries pursued ISI in the 1960s and experienced slower growth thereafter, the slowdown was in fact chiefly triggered by external shocks which had little to do with trade and industrialization policies (Rodrik, 2000a). The failure of these economies to buffer such shocks was more likely due to poor institutions, especially macroeconomic institutions, but also institutions related to conflict management. And whilst big governments have often failed to ignite growth, many large governments – often with strategic trade and industrialization policies - have also succeeded (China, Botswana, South Korea, Taiwan, Vietnam). And finally, the macroeconomic 'growth regressions' literature is inherently constrained in both its ability to measure policies and institutions in detail, and in its ability to deal with some fundamental endogeneity problems (Rodriguez & Rodrik, 2001; Rodrik, 2005b).

III.3 Explaining the failure of the neoclassical reform.

Whilst methodological problems explain part of the apparent paradox between the empirical support of the neoclassical paradigm in the academic literature and the observed failure of the Washington Consensus in the real world, this explanation would be incomplete without an alternative theoretical explanation for the disappointing performance of liberalization efforts in the last twenty years. Rodrik proposes three such hypotheses.

First, early Washington Consensus reform attempts were theoretically misguided in viewing bad policies as the root cause of failed development. In doing so they neglected the importance of reforming the institutions – key determinants of development in the long run - that both support and constrain the market. Rodrik also appears to agree with the widely held view that reform packages consistently failed to compensate the losers from reforms, and underemphasized the key role of financial regulation.

Second, whilst 'Second Generation' Washington reformers did at least try to incorporate more emphasis on institutionalism and poverty reduction, they still attempted to export a common policy and institutional blueprint to a diverse set of developing countries. In Rodrik's appraisal this failure to incorporate country-specific factors – geography, history, culture, and current policies and institutions – are likely to render the blueprint's policy prescriptions ineffective, and potentially even regressive. So while Rodrik agrees that these first order goals are important, he suggests that there is no unique mapping from policy targets to policy instruments (Rodrik, 2005a).

A third and final mistake of the second-generation Washington reformers, and reformers of other bents, is that they have become increasingly prone to over-ambition. The Washington Consensus Mark I may have had some omissions, but its reform agenda was parsimonious relative to its Mark II descendent, which now includes a 'laundry list' of other reforms and a mandate to achieve a whole host of Millennium Development Goals. Whilst carrying out one reform but ignoring conditional reforms can be dangerous (as per the theory of Second Best), carrying out all reforms simultaneously is institutionally infeasible in underdeveloped countries.

III.4 Towards a new development paradigm.

If the Washington Consensus paradigm is flawed, what sort of paradigm should replace it? Rodrik's revisionist paradigm relies on several core tenets about the workings of markets and governments, some of which apply generally to all economies, and some to developing countries in particular.

III.4.1. The role of markets.

Rodrik holds that agents are individually rational inasmuch as they respond to economic incentives, and that market-based economic systems can lead to sustained growth in the absence of significant market failures. However, in addition to the standard failings of markets (public goods, externalities, monopolies) market-based economies face other problems which are accentuated in developing countries. Like development pioneers such as Hirschman (1958) and Rosenstein-Rodan (1943), Rodrik emphasizes coordination failures, economies of scale and various spillover effects (Rodrik, 2005a), as well as imperfect knowledge of what it is a country is good at producing (Hausman & Rodrik, 2003). Markets can also generate highly unequal outcomes, thereby promoting conflict which the market cannot automatically correct (Alesina & Rodrik, 1994; Rodrik, 2005a). And markets are subjected to severe exogenous shocks which can significantly impede longer run development unless their influence is appropriately checked by good institutions (Rodrik, 1999). So although markets are potentially a fecund source of growth and development, "markets need not be self-creating, self-regulating, self-stabilizing, and self-legitimizing" (Rodrik, 2005a).

III.4.2. The role of governments.

These market failures nominally justify a role for governments in creating policies and institutions that support, restrain, and in some cases replace, private markets. So in the long run governments ultimately need to provide institutions that achieve the following intermediate policy targets (Rodrik, 2005a):

- A rule of law which grants adequate and secure control of property;
- Appropriate restraint of market excesses (pollution, monopoly power, financial imprudence, inequality);
- Sound macroeconomic management (e.g. stable prices, adequate access to credit, opportunities for trade, business cycle smoothing);
- Social insurance (e.g. poverty alleviation); and
- Management of conflict (e.g. appropriate distributional policies, adequate justice system).

However, such a list of intermediate targets cannot simply be pegged on to the already extensive list of Washington Consensus policy instruments outlined by Williamson (1990). For one thing, the policies and institutions which can achieve these outcomes are environment-specific, so the effective adaptation of existing institutional paradigms requires country-specific knowledge and a flexible and open-minded consideration of the types of public policies which might achieve these institutional outcomes. There are two important implications of this point. First, most of these 'new' policy and institutional arrangements will not be found in economics textbooks; rather they are pragmatic 'on the ground', 'home-grown' solutions to idiosyncratic problems (Rodrik, 2005a, 2006). Second, the political structures that are typically best suited to imbuing policy design with the necessary degree of country-specific knowledge and political support are democratic and participatory (Rodrik, 2000b). Democracy, in fact, might be thought of as a meta-institution which improves policy and institutional design in a number of different dimensions.

III.V.3 Towards a new growth strategy.

Governments need not only be more flexible, experimental and democratic, they must also be more parsimonious in their reform efforts. For whilst a broad array of institutional functions must be present if an economy is to achieve development in the long run, different functions are important at different stages in the growth process. For example, the institutions and policies that enable growth are not necessarily those that are important for sustaining growth (Rodrik, 2006). A consequence of this is that governments do not need to carry out a full 'laundry list' of reforms to initiate growth, or for that matter to sustain it. Because whilst a laundry list reform agenda might be optimal as per the Theory of Second Best – which states that the removal of any one distortion in the presence of other market or government distortions is not an action guaranteed to improve welfare – that laundry list is both technically and politically infeasible in practice. Instead, policymakers would do well to identify the largest direct constraint to growth as the policy target of immediate interest, and concentrate public resources into removing that constraint before addressing all others (Hausman, Rodrik, & Velasco, 2006). I hereafter term this approach the 'binding constraints growth diagnostic'.[6]

And finally, one last point of note should be mentioned. Whilst Rodrik supports parsimony and flexibility, he does necessarily vindicate gradualism, at least in the sense that any given distortion should only be removed slowly. His primary concern over that sort of gradualism is that it sends uncertain signals to private investors, and may therefore undermine government credibility and inhibit responses from the private sector. Thus, if anything, Rodrik favors 'radical reform in a narrow range of policies' (Rodrik, 1990).

IV. Discussion

Hopefully the above synthesis of Rodrik's writings has done appropriate justice to the intended meaning of the cited works. Let us now proceed to critically consider some of the key elements of Rodrik's post-Washington paradigm.

IV.1 The failures of the Washington Consensus.

If one were to begin at the beginning, the first aspect of Rodrik's critique that should be considered is to what degree the Washington Consensus has really failed as an operational growth strategy. Were one to play the devil's advocate, so to speak, it would be important to point out the following:

a. Many alleged proponents of the Washington Consensus have argued that there is not one strict Washington 'growth blueprint' and that in practice the Washington institutions have engaged in sufficiently flexible policymaking, constrained as they are by the need for a certain degree of internal policy coherence. Nevertheless, most independent observers feel

[6] By 'direct' obstacle, Hausman, Rodrik and Velasco (2006) refer to distortions that have the largest direct effects on growth, where indirect effects refer to the effects hat removing this distortion will have on other distortions in the economy, which may be positive or negative. Thus there is no guarantee that their strategy will be welfare improving, but removing large direct distortions seems the safest way to proceed in the absence of perfect knowledge the strength and direction of indirect effects, and in the presence of limited technical and political capability to address all constraints simultaneously.

that these institutions have only displayed flexibility at the margin.[7] For example, the conditions attached to structural adjustment loans (SALs) are fairly explicit and show substantial similarities across countries in terms of their basic elements.

b. The task the Washington institutions set themselves – that of reforming policies in countries with long histories of poor policies, weak institutions and considerable political instability – was not an easy one, and universally successful reform outcomes were (and still are) an unrealistic aspiration.

c. Not all domestic political opposition to Washington-based reforms has been legitimate (i.e. based on valid economic or social concerns).

d. Some of Washington's "students" have performed quite well, especially in recent years (Ghana, Uganda, Mozambique, Tanzania).

e. Following from d., it is important to remember that the returns to economic reforms are sometimes difficult to measure, since reform outcomes are affected by exogenous events (e.g. terms of trade movements, political changes) and may only be positive in the longer term.

f. While the Washington Consensus as a whole might have failed, this does not mean that there is not widespread agreement on the importance of many of the elements of Williamson's 10-point reform agenda (especially avoiding significant macroeconomic distortions).

Though it is important to bear these kinds of arguments in mind, it is well beyond the scope of this review to fully appraise to what extent the Washington Consensus has failed; that primarily empirical task has been carried out elsewhere (Easterly, 2005; The-World-Bank, 2005), although there are still significant methodological challenges involved, as Rodrik himself would have to admit (Rodrik, 2005b). At any rate even a fairly conservative assessment would necessarily conclude that the Washington Consensus has failed to live up to its own expectations, even if there is some doubt as to the degree of failure, and what

[7] For example, one World Bank economist objected to the notion that The World Bank always calls reduces government expenditure, citing instances where they have called for greater tax revenue to achieve reductions in the fiscal deficit. Of course, the objective of achieving reductions in the fiscal deficit is still the same, and there is considerable evidence that important government expenditures have decreased under Washington conditionalities (Fan & Rao, 2004; Lipton & Ahmed, 1997).

might have happened had other strategies been pursued. So for the remainder of this discussion I will take this alleged failure for granted.

IV.2 Rodrik's revisionism.

Let us therefore turn our attention to Rodrik's revision of growth experience in general, including East Asian experience. 'Revisionism' aptly refers to the act of demonstrating that existing research has misinterpreted or ignored importance evidence. Thus earlier East Asian Revisionists, such as Wade (1990) and Amsden (1989), went to great lengths to show that the neoclassical interpretation of the source of East Asia's growth success – in a word, the market – was erroneous. In fact, say these Revisionists, most relevant East Asian economies had high degrees of government intervention, including (but not limited to) a significant number of state owned enterprises in key sectors, surprisingly large but also quite strategic trade barriers, and highly regulated and often publicly owned financial sectors. Rodrik also emphasizes these kinds of diverse government interventions, and so he very much follows in the footsteps of Amsden and Wade in this regard.[8]

Methodological critiques are also a common component of revisionism. However, one partial difference between Rodrik and the East Asian revisionism of the late 1980s is that much of the subsequent literature in the 1990s has moved away from East Asian analysis per se to a greater use of cross-country comparisons, often via the near-ubiquitous growth regression. So Rodrik's critique of cross-country econometric analysis is arguably a very important component of his revisionism (Rodriguez & Rodrik, 2001), even though it is, in part, an extension of Wade's own critique of the cross-country evidence a decade earlier (see Chapter 1 of Wade (1990), pages 15-22). These kinds of criticisms of the cross-country evidence also appear to have gained in influence. A recent 29-person panel of experts reviewing World Bank research from 1998 to 2005, for example, came down heavily indeed upon the Bank's over-use of cross-country econometrics (Banerjee, Deaton, Lustig, Rogoff, & Hsu, 2006). Perhaps the implication of these methodological critiques is that the development profession as a whole is now much more skeptical of the international 'evidence' on the virtues of neoliberal policies.[9]

[8] Although several of Rodrik's earliest works already dealt with precisely these issues at around the same time as Amsden and Wade were writing (Rodrik, 1986, 1987, 1988, 1989, 1994).

[9] Of course, the term "as a whole" is important here: perceptions in The World Bank could be quite different to impressions in independent academia.

But on the other hand, what are the alternatives to cross-country growth regressions? Rodrik has pursed several alternative approaches. First, he has used cross-country regressions, but examined the data in different ways, focusing on event analysis such as oil shocks (Rodrik, 1999) and growth accelerations (Hausman, Pritchett, & Rodrik, 2005). These studies are new and insightful ways at re-examining cross-country growth patterns, but I must admit to being skeptical as to whether they provide any fundamental advantages relative to conventional growth regressions. Cross-country event analysis still tends to rely on highly aggregated measurement units and restrictive parameter homogeneity assumptions, and event analysis is unlikely to overcomes basic endogeneity challenges of the kind identified by Rodrik himself (Rodrik, 2005b).[10]

A second approach which Rodrik adopts is the more informal analysis of stylized facts, a type of analysis best embodied in *Growth Strategies* (Rodrik, 2005a). That particular essay is a remarkably good non-econometric analysis of growth history, and a real 'must-read' for any student of development. The uniqueness of Rodrik's analysis in *Growth Strategies* is that instead of looking for common 'best practice' policies across countries – as one would implicitly do in any linear growth regression, as well in case studies of East Asian 'miracles' – Rodrik emphasizes the *different* policies which fast growing countries have pursued. From these differences and, more specifically, these departures from the Washington Consensus strategy, Rodrik concludes that the most important stylized facts of development experience to date are that one size most certainly does not fit all, that development is largely a homegrown affair, and that neoclassical theory is consistent with a diverse range of government interventions. Of course, these conclusions are not entirely unique to Rodrik. Adelman and Morris (Adelman & Morris, 1967, 1988; 1997) heavily emphasized 'alternative development paths' from the 1960s onward, albeit based on cross-country econometric techniques. Robert Wade makes some very similar conclusions on the non-equivalency of neoclassical theory and neoclassical policies in *Governing the Market*, as does Stiglitz (1994) in

[10] With regard to endogeneity issues, the factors that *statistically* explain a growth acceleration are just as likely to be endogenous as those used to *statistically* account for average growth rates. In the case of policy responses to common shocks – such as oil shocks (Rodrik, 1999) – the problem may be less severe in that one can ideally gauge diverse policy responses to a common event. However: one would still need to control for an array of factors in order to estimate the true effect of any individual factor; common shocks are few and far between, so event analysis provides limited opportunities for empirical analysis; what are presumed to be common shocks are unlikely to really be common (e.g. oil shocks vary according to dependence on oil).

Whither Socialism.[11] And the profession as a whole has increasingly gravitated towards 'helping developing countries help themselves' and greater 'ownership' of reform strategies. But nevertheless, Rodrik's inductive analysis in *Growth Strategies* is exceptional insofar as it unifies all these arguments into a clear and coherent revision of recent development experience.

Yet the inductive approach used in *Growth Strategies* is not an analysis free of methodological pitfalls. Consider, for example, the following extract from that paper:

> Function does not map uniquely into form. It would be hard to explain otherwise how social systems that are so different in their institutional details as those of the United States, Japan, and Europe have managed to generate roughly similar levels of wealth for their citizens. All these societies protect property rights, regulate product, labor, and financial markets, have sound money, and provide for social insurance. But the rules of the game that prevail in the American style of capitalism are very different from those in the Japanese style of capitalism. Both differ from the European style.

But from this can we emphatically conclude that these specific institutional forms were really important for growth? If X different countries achieved the same institutional outcomes with X different types of specific institutional forms, then might we not also conclude that the specific institutional forms which these countries adopted were irrelevant? Is it possible that Japan would have grown just as quickly with an American-style labor market? The answer to all these questions is "Yes", for it is at least possible that the specific institutional forms adopted by different successful countries were largely irrelevant.[12] Perhaps it is not <u>diverse</u> institutions that matter but other <u>common</u> factors – geography, hard work, luck. Or

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[11] Consider, for example, the following quote from Wade (1990, p. 349):

> 'A distinction has to be made between what is consistent with neoclassical theory and what is consistent with neoclassical prescription. There is room within the confines of neoclassical theory for practically any mix of markets and interventions.'

Rodrik has emphasized the same conclusion is many different papers and presentations:

> 'There is no unique mapping between these objectives and specific policy proposals, which is the next point on which there is by now a fair amount of consensus. . . General principles of good economic policy do not map directly and uniquely into specific policy agendas.' *Rethinking Growth Policies* (Rodrik, 2004)

or:

> 'All of the above institutional anomalies are compatible with, and can be understood in terms of, neoclassical economic reasoning ("good economics"). Neoclassical economic analysis does not determine the form that institutional arrangements should or do take.' *Growth Strategies* (Rodrik, 2005a).

[12] The same type of critique could be applied to many other inductively derived conclusions in this literature. Countries engaged in diverse industrial policies, some being associated with industrial success (East Asia) and others with industrial failure (e.g. Latin America). The literature has looked for differences in the industrial strategies of these countries – such as discretion- versus rules-based regimes, or whether the carrot was accompanied by the stick – but without much in the way of definitive conclusions. Compare Rodrik (Rodrik, 1995b) to the World Bank (The-World-Bank, 1993) for example.

it may be that specific institutional forms must be diverse, but only in a systematic way, perhaps according to stages of development. Wade (1990), Adelman and Morris (1967), and Myrdal (1968), for example, all argue – with slight differences in terminology and other specifics – for 'hard states' at early stages of development (i.e. post-War Japan, South Korea or Taiwan) followed by more participatory regimes later on (the U.S., present day Japan, South Korea and Taiwan). Indeed, the historical evidence for this evolution from hard states to participatory regimes is very strong.

This is not to say that Rodrik's inductive conclusions in *Growth Strategies* and other papers are wrong, merely that they are vulnerable to the functionalist fallacy that because countries adopt different institutional forms, these different forms necessarily serve important functions. This means that the evidence which Rodrik cites – especially on institutional diversity - is open to more interpretations than his alone.

There are at least two other related problems in papers such as *Growth Strategies*. First, this kind of inductive institutionalism, though insightful, does not yield much in the way of testable hypotheses. For one thing, institutions are difficult to identify and measure (again, this is not to say that we should ignore them or not try to measure or identify them). But more importantly, it is a bold departure from conventional research to conclude that successful countries have so little in common with one another that researchers can draw no concrete generalizations as to what policies might promote faster and more pro-poor growth.[13] In contrast, earlier institutionalists – the Wades, Adelmans and Myrdals – argued that the optimality of such alternative development paths was relatively systematic, and therefore discernible in the international evidence (as with the above example of the evolution from hard states to more participatory regimes). Rodrik offers very little in the way of this kind of second-order generalization, which means that his conclusions lack testability, let alone the resonant directness of earlier theories of growth, including the Washington Consensus. For example, even though Rodrik supports the idea of bringing industrialization policies back to the policy table (Hausman & Rodrik, 2003; Rodrik, forthcoming), he cannot

[13] Certainly existing research on growth has highlighted a number of common determinants. Consider, for example, the broad number of common policy and non-policy determinants explaining East Asian growth (The-World-Bank, 1993). And while Rodrik may be rightly skeptical of large parts of the cross-country empirics literature, this literature has at least provided statistically powerful explanations of the variation in cross-country growth experiences (Temple, 1999).

unequivocally support industrial policies (as per Wade)[14] because the optimality of individual policies is context specific (Rodrik, 2005a).

It should again be stressed that we need not necessarily conclude that Rodrik's interpretation of institutional diversity is wrong, merely that there are other interpretations of the facts which he draws upon, and that his own interpretation is not easily amenable to standard empirical tests. No less importantly, the logical impossibility of simultaneously emphasizing institutional diversities and advocating concrete policy prescriptions forces Rodrik into making prescriptions that are highly general, extremely nuanced and rather guarded. This means that Rodrik's message lacks the popular (populist?) appeal of the more concrete statements put forth by earlier institutionalists as well as Rodrik's contemporaries (such as Jeffrey Sachs (2005a) or Nicholas Stern (2001)).

IV.3 Binding constraints growth diagnostics.

In the face of context-specific policy optimality, Rodrik and his coauthors (Birdsall, Rodrik, & Subramanian, 2005; Hausman et al., 2006) propose that economists and other technocrats should adopt a diagnostic approach to policymaking. As I noted in the previous section, the main novelty of their approach is that they favor the <u>sequential</u> identification and removal of the largest <u>direct</u> obstacles to growth, rather than hopelessly optimistic 'laundry list' reform agendas (e.g. Millennium Development Goals), Second Best 'tailoring', or the importation of 'one size fits all' blueprints and erroneous 'best practices' (e.g. Washington Consensus).

There are two relevant issues concerning growth diagnostics. First, Rodrik and colleagues are certainly not alone in advocating a more humble, more pragmatic and less ambitious diagnostic approach to policymaking. Humility, pragmatism and interdisciplinary diagnostism are all features of growth strategies advocated by earlier institutionalists (Adelman & Morris, 1967; Myrdal, 1968),[15] and more recently by Professor Jeffrey Sachs (2005a). The bindings constraints diagnostic approach also shares some small affinity with the multiple gap class of models proposed by Chenery and Strout (1966). The principle is much the same, but what most economists have learned – or re-learned – since Chenery and

[14] In the preface to a recent second edition of *Governing the Market* (Wade, 2004), Wade is arguably more cautious about feasibility of strategic trade and industrial policies in small countries with limited potential for government leadership and strong interlinkages between sectors in the domestic economy. But although this caveat qualifies his earlier proposals somewhat, it is a caveat which is still consistent with the idea that 'good policies' can be identified and prescribed in a relatively systematic fashion.

[15] In the case of Myrdal (1968), see Appendix II, much of which was authored by Paul Streeten.

Strout's seminal work, is that the obstacles to growth are far more numerous and non-economic in nature than just investment, foreign exchange gaps or fiscal gaps. So an integral feature of any diagnostic approach in the modern era must incorporate a fairly broad array of growth determinants, including informal institutions, which often seem to be consciously or unconsciously neglected in Rodrik's work.[16]

In that vein the more relevant approach is the 'growth diagnosticism' of Professor Jeffrey Sachs. Indeed Sachs (2005a) arguably provides a more complete list of potential obstacles to growth than Rodrik and colleagues. The main point of difference, however, is the scope of reform. Sachs agrees with the humility that comes with an inductive interdisciplinary analysis of development problems (i.e. geography, disease burdens), but, by my reading at least, Sachs disagrees with the notion of a modest sequential reform strategy a la Rodrik.[17] Indeed, Sachs sees modest 'piecemeal' growth strategies as wasteful and unnecessary, and is the leading advocate of a 'laundry list' of Millennium Development Goals (MDGs). Who is right, Sachs or Rodrik?

On the one hand there is probably agreement among many economists with Rodrik's assessment of the failure of laundry list Washington Consensus programs in countries severely constrained by financial capital, technical capital and political capital. The literatures on the effectiveness of policy reform and foreign aid have made ample note of small returns to 'big pushes', and there is a widespread feeling in the profession that aid projects and technical assistance must be able to extract more bang for any given buck. But whilst laundry list reform strategies and large scale aid flows have often disappointed in the past, economic theory and development experience only provide limited support for Rodrik's targeted reform strategy.

In terms of economic theory, Rodrik and collaborators justify a binding constraints growth diagnostic with the rather loose idea that the direct benefits of removing a large

[16] For example, in *Growth Strategies* Rodrik mentions informal institutions but then intentionally ignores them. I am not sure why. As Rodrik would doubtlessly acknowledge, informal institutions are extremely important in development – indeed, they are usually considered more important at earlier stages of development – and integral to the kinds of institutional principles, such as conflict management, that Rodrik identifies (see Section II).

[17] In a response to Easterly's critique of the Millennium Development Goals, Sachs writes:

> Easterly's call to do things piecemeal is vacuous. Shall we do vaccinations this decade, AIDS control the next, malaria control in the 2020s, clean drinking water in the 2030s, and food production in the 2040s? There is no reason why we can't help poor countries to invest in clinics, schools, roads and improved farms during the next 20 years. - (Sachs, 2005b)

distortion are likely to outweigh any indirect 'interaction' effects between distortions (as per the Theory of Second Best). For this outcome to hold Rodrik et al. must assume: (a) that direct effects of removing a distortion are measurable and predictable; (b) that interaction effects between distortions are not measurable or predictable but are nevertheless small relative to the direct effects of removing distortions; and (c) that the size of any deleterious interaction effects between distortions are not positively correlated with the size of the direct effects of removing distortions (i.e. that if we remove the biggest direct distortions we will not be systematically increasing the size of any deleterious indirect effects between distortions). Perhaps we can accept these assumptions, but this is certainly a murky area in which formal theory offers very limited guidance.[18]

Moreover, historical evidence presents us with two stylized facts which seem inconsistent with Rodrik's binding constraints diagnostic approach. First, the most successful developing countries – South Korea, Taiwan, Malaysia, Singapore – may have not have liberalized their economies all at once (a point made by Rodrik, Wade and others), but they did achieve a wide range of socio-economic outcomes in a fairly simultaneous fashion (Headey, 2006; The-World-Bank, 1993). In other words, it was as if they succeeded in achieving MDG-type targets. For these countries excelled not only in increasing incomes, but also in contemporaneously increasing education, health, and infrastructure, and in managing the macroeconomy. So one reasonable interpretation of their growth paths is that they represent big socioeconomic pushes, rather than more targeted pushes aimed at relieving the most binding constraints first.[19] Of course, one can argue for different strokes for different folks – Africa today is not East Asia of yesteryear – but Rodrik and colleagues need to confront these kinds of stylized fact and not simply rest their arguments on a highly abstract theoretical model.

A second empirical fact which their work does not confront is the more challenging African context. Rodrik et al.'s empirical work in *Getting the Diagnosis Right* is unconvincing in the context of extreme underdevelopment because the economies which they apply their

[18] It also seems rather odd to refer to static general equilibrium context – which the Theory of Second Best relates to – rather a growth context. Amsden (1989) and Wade (1990), for example, argue that government induced distortions played an important role in directing the private sector towards sectors of future importance to the national economy. In other words, these governments got the price wrong.

[19] Rodrik's own work is reasonably supportive of this statement. For example, high levels of inequality in Latin America adversely affected conflict management as well as policymaking in that region (Alesina & Rodrik, 1994), and the difficulties experienced by developing countries in the face of the international oil shocks of the 1970s essentially depended upon their institutions of conflict management (Rodrik, 1999).

growth diagnostics to are middle income Latin American economies. Engaging in diagnostic exercises for El Salvador, the Dominican Republic and Brazil, as Rodrik and colleagues do, is all very well, but it at least seems possible that these economies were selected by the authors precisely because a single large obstacle to growth can indeed be quite easily identified for each of these economies. But I am rather more skeptical that Rodrik and colleagues could pick out a typical African country and - out of the numerous challenges facing such a country (lack of primary education, poor roads, desperately low life expectancy, HIV/AIDS, government corruption, over-regulated labor markets, and poor macroeconomic management) - persuasively demonstrate that any one of these problems will easily present itself as the largest direct obstacle to growth. The situation in many developing countries is rarely so black and white.

Moreover, the argument that all these socio-economic problems are deeply interconnected is intuitively attractive. Such a hypothesis challenges Rodrik's conclusion that removing distortions one-by-one will be more effective than multi-dimensional reform strategies. Consider some examples. Can we eliminate HIV without improving education or changing cultural attitudes? Bloom and Canning (2003) answer in the negative? Can we implement better public policies as a whole without curtailing government corruption? The World Bank position now seems to be "No, we cannot". Or consider again East Asia 'big socioeconomic push' in education, health, infrastructure, industry and agriculture: can we make up a plausible story to support the idea that development in all these socio-economic sectors reinforced developmental processes in other sectors. Of course we can. This kind of cumulative causality was one of Myrdal's (1968) chief conjectures, and The World Bank (1993) report on the East Asian Miracle quite explicitly points out the multidimensional nature of East Asia's success. So on these grounds one could reasonably conclude that, due to the great interconnectedness of the large number of problems facing a typical underdeveloped country (East Asia in 1950s, or Africa today), Rodrik's proposal of identifying and targeting one supposedly binding constraint may be both inherently difficult and ultimately futile.

Another concern in adopting a growth diagnostic approach is its almost nihilistic implications for existing knowledge about development. One extreme interpretation of Rodrik's critique is that there is no such thing as 'best practice' economics: we need growth diagnostics because what is best practice can only ever be context-specific. But that conclusion seems highly inconsistent with much of Rodrik's own research agenda,[20] and with development experience. For whilst Rodrik points to the failures of importing policy blueprints from outside – especially from Washington – he tends overlook those examples in which policies *did* appear to travel well. This is ironic in some sense, for other East Asian Revisionists were the first to illustrate the great extent to which South Korea and Taiwan purposively copied Japan's industrialization strategies, and how the rest of South East Asia has thereafter learned from South Korean and Taiwanese experience (The-World-Bank, 1993). Indeed, one could quite plausibly argue that what is still needed today is greater and more effective information sharing, and more adaptation of best practice strategies, not less. For example, how many non-Asian countries have really tried to adopt and adapt the kind of East Asian growth strategy outlined in Wade's 10-point agenda above (Tables 1 and 2)? Few, if any. Indeed, one interpretation of the experience of recent decades is not that non-Asian countries have erroneously tried to adopt 'best practices', but that they have erroneously tried to adopt a neoclassical strategy ill-suited to early stages of development.[21]

In summary, there are some significant dangers in the binding constraint growth diagnostic advocated by Rodrik and his collaborators. First, such an approach may leave us vulnerable to under-ambition, as Sachs suggests. Second, the approach may not be effective: favorable reform outcomes in one area may regularly require reforms in other areas (i.e. more strategic sequencing) or simultaneous reform in a number of areas, as per the big socio-economic pushes witnessed in East Asia. And third, there is a danger that if Rodrik's analysis is taken to its extreme implications then poor countries may be overly reluctant to learn lessons from other countries that are (a) performing well because of identifiably effective government interventions; and (b) possessive of sufficiently similar economic characteristics to Country A, so that the interventions in question are potentially transferable

[20] Let me give some examples. Rodrik's *Getting Interventions Right: How South Korea and Taiwan Grew Rich* argues that these countries grew rich from a government-led domestic investment boom (rather than export booms) supported by high levels of initial equality and human capital. Rodrik's *Taking Trade Policy Seriously: Export Subsidization as a Case Study in Policy Effectiveness* is a more explicit practice of discerning best practice export subsidization policies. As one might expect of Rodrik, neither of these papers is over-ambitious in selling a 'best practice', but both papers serve to illustrate that one of the most important things economists do is share information on the policies that work, the policies that don't, and the conjectured reasons why.

[21] For example, Wade (1990, p. 371-372) heavily stressed the need to create an elite bureaucratic cadre at the top of the policymaking pyramid, as per Japan, Singapore, Taiwan and South Korea. And yet The World Bank and most poor countries have devoted almost all their attention to *de*centralizing political power. Is this the futile adoption of best practice policies?

after suitable adaptation.[22] There are probably too many uncertainties here to categorically reject a binding constraints growth diagnostic altogether.[23] But these considerations discussed above need to be more explicitly explored by Rodrik and his *Growth Diagnostics* co-authors before their approach can be accepted as a viable alternative to alternative practices and proposals.

IV.4 Democracy and development

Whilst Rodrik argues that there is very little evidence that any particular policy or institution will work in any given country – with the implication that making foreign aid flows conditional upon standard policy reforms, such as trade liberalization, are inappropriate - he seems to make the exception in the case of a 'meta-institution', democracy. Democratic institutions, he argues, lead to more 'predictable', less volatile and more equally distributed growth (Rodrik, 2000b). In positive terms, I find this argument the least well supported component of his paradigm. For whilst one can always proclaim the virtues of democracy on normative grounds, the notion that democracy increases or improves growth at early stages of development is a highly contestable claim.

The first hurdle for democracy advocates is precisely that region which Rodrik and many other economists have extracted so many development lessons from, East Asia. Rodrik is aware of this, of course - "In policy circles, the discussion on the relationship between political regime type and economic performance inevitably gravitates toward the experience of a handful of economies in East and Southeast Asia" (Rodrik, 2000b) – but he argues that "A systematic look at the evidence, however, yields a much more sanguine conclusion" as to the virtues of democracy.

This approach to interpreting the economic merits of democracy seems to me a significant methodological inconsistency. On the one hand Rodrik is happy to attach critical importance to the economic experience of "a handful of economies in East and Southeast Asia" in any number of his most prominent works (Rodrik, 1994, 1995a, 1997a, 2005a,

[22] Moreover, such adaptations are likely to be cost effective in that they presumably save on technical resources.

[23] Another issue I have not paid much explicit attention to is the level at which a binding constraint is defined. For example, one might conclude at a fairly aggregated level that human capital is a constraint. One might go further and conclude that health was a greater direct constraint than education, but that educational investments were important to improving health. So whilst one might conclude that there is a binding constraint at a highly aggregated level, removing this constraint may require multiple instruments. Nevertheless, I still find it difficult to reconcile the binding constraints approach with the truly broad 'big socioeconomic push' that East Asian countries embarked on.

forthcoming),[24] and equally happy to vilify cross-country econometric work in several other papers (Rodriguez & Rodrik, 2001; Rodrik, 2005b). But on the other hand Rodrik attaches little or no importance to the political experience of East Asia, and is instead content to rely on cross-country econometric work of questionable value.[25] Moreover, recent experience gives ample evidence of the troubles which premature democratization can create for both economic and political stability. Consider, for example, political instability in the Philippines post-Marcos, in Indonesia post-Suharto, in Thailand under Thaksin's brief reign, or Latin America's poor economic performance since a wave of democratization hit that continent in the late 1980s (Chile notwithstanding).[26]

This is not to say that democratization is not a worthy long term goal.[27] And even at lower levels of development there exist authoritarian regimes which are so fundamentally corrupt and 'kleptrocratic' that democracy – if it can be effectively instituted - is certainly a worthwhile 'second best' alternative to a benevolent dictatorship. One can likewise accept that decentralization – the other popular political and fiscal reform advocated for LDCs - is also an effective second best institution in countries where the central bureaucracy is at once

[24] Interestingly, Rodrik's assessment of South Korean and Taiwanese growth (Rodrik, 1995a) does identify the importance of the bureaucracies' independence from political interests and instability. However, Rodrik – to my knowledge at least - never explicitly uses these countries' experiences to draw the conclusion that their authoritarianism was favorable for their development.

[25] For example, in fleshing out his cross-country regressions, Rodrik concludes that "Long-standing democracies such as India, Costa Rica, Malta, and Mauritius have experienced significantly less volatility than countries like Syria, Chile, or Iran, even after controlling for country size and external shocks." But the growth regressions he uses would, in my view, do a poor job of explaining volatility, or the lack thereof, in these sorts of countries. Syria, Chile and Iran, for example, are all quite heavily dependent upon natural resource revenues, and Chile and Syria also have quite volatile weather systems which induce volatility in agricultural output especially. Syrian agriculture, for example, is highly dependent upon very unreliable rainfall, and is located in a relatively small geographical area, so it is therefore no surprise that its agricultural output is extremely volatile. It is difficult to argue that this volatility has much to do with institutions since irrigation potential is a geographically constrained factor. Moreover, whilst democratic India also has volatile weather systems, this largely exogenous source of volatility does not show up in aggregate data because the volatility varies from region to region (i.e. it is not highly covariate as some Indian states have predictable monsoons whilst others have very unpredictable monsoons). Nevertheless, output within (democratic) Indian states is highly volatile and this does show up in the state level data. Whilst Rodrik attempts to control for the fact that larger countries such as India will necessarily record lower volatility (through diversity of outputs and regions) by specifying population size as a control, this will only be a highly imperfect control (it does not factor in geographic diversity within countries, for example). In short, I remain unconvinced that a country like India records less institution-based output volatility than a country like Syria.

[26] On this point I find it a curiosity that Rodrik and his sympathizers in The World Bank (The-World-Bank, 2005) blame Latin America's weak performance in the 1980s and 1990s on The Washington Consensus reforms when these countries were also undergoing significant political change, namely democratization.

[27] If authoritarian regimes in East Asia were pro-growth but not pro-poor one might still reasonably defend democracy. But of course, this is not the case. East Asian growth was very pro-poor and included significant land reform (e.g. South Korea and Taiwan) and a Green Revolution that was more pro-poor than democratic India. Hence Suharto's famous phrase 'economic democracy'.

politically entrenched but economically ineffective.[28] But it is another thing entirely to argue for democratization – on a positive economic basis, at least - in poor countries which have at least obtained moderate economic momentum. Indeed the lesson extracted by Robert Wade (1990) – who is consistent in extracting both economic and political lessons from East Asia - is that LDCs must establish effective institutions of political authority before the political system is widely democratized.[29]

Ongoing research in economics and political science would do well to reassess this aspect of development in order to better understand why economic democracies can sometimes emerge under authoritarianism, why political democracies vary so much in their sustainability and effectiveness, and why the worldwide decentralization experiment has thus far achieved such mixed results (Anwar & Thomson, 2004). But until the economic and political science professions can come up with more definitive answers to these sorts of questions, unconditional arguments for democratization as a growth-enhancing institutional reform remain primarily ideological in nature.

V. Conclusions

Professor Rodrik and his collaborators present a persuasive critique of the most influential development paradigm of the last thirty years or so, the Washington Consensus. In its place they encourage policymakers to be more open-minded and experimental with the kinds of policies and institutions which successful growth strategies might include, but also more systematic and realistic in the design and implementation of those strategies. Rodrik also presents a forceful critique of the looking glass through which we view development, and shows how extracting the types of lessons policymakers really need requires a more nuanced interpretation of the facts that can be presented by either selective case studies or highly aggregated growth regressions.

[28] Other arguments for more participatory political systems – such as political and economic decentralization – which Rodrik presumably supports (as does The World Bank) also receive heavily qualified support from East Asian experience. As Wade and Rodrik himself documents, significant political power in South Korea and Taiwan belonged to just a handful of elites for most of these countries development histories. On the other hand, recent evidence suggests that one of the most politically decentralized countries in the world is authoritarian China (Binswanger, 2006). But effective decentralization is by no means easy, as recent research amply documents (Anwar & Thomson, 2004).

[29] Adelman and Morris (1967) long ago reached the same conclusion via cross-country evidence, and Huntingon (1968) provides an in-depth political treatise on the evolution towards democratic systems.

But whilst I am in agreement with the main thrust of Professor Rodrik's post-Washington paradigm – namely its caustic critique of neoclassical policymaking, its incorporation of a wider range of market failures (many of which are quite specific to LDCs), and its emphasis on the importance of institutions in a general way - I have also argued that there are still some serious deficiencies in this paradigm. Whilst it is true that institutional diversity is an important feature of successful developing countries, it is not obvious that the differences between 'miracles' are more important than their similarities. Whilst the hard work is indeed done at home, foreign blueprints can also be very useful. Whilst laundry list reform strategies may be overly ambitious, the 'binding constraints' growth diagnostic approach is arguably under-ambitious, and is not yet well supported by growth theory or empirics. And although democracy is a worthy long term economic goal with important non-economic virtues, the evidence that democracy is necessary for achieving pro-poor economic growth at early stages of development is questionable indeed.

These caveats aside, the ideas of Professor Rodrik and his collaborators have received deserved recognition from both the academic community at large and, eventually at least, from the international organizations for which his ideas are most relevant, especially The World Bank. For whilst Rodrik has abstained from turning the wheel full circle, he has nevertheless provided the profession with a long list of original, insightful and provocative reinterpretations of development history. Rodrik's particular brand of revisionism should prompt theorists and policymakers alike into seriously rethinking the content and formulation of development policies. And in a period in which the only consensus thus far is that the old Consensus was wrong (Stiglitz, 2004), Professor Rodrik's challenge to conventional wisdom may yet prove decisively influential in reshaping development strategies in a post-Washington era.

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