

## EVALUATING RECIPES FOR DEVELOPMENT SUCCESS\*

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The main purpose of most scholarly research, both theoretical and empirical, is to improve our understanding of the phenomena and processes being studied. The work may lead to useful prescriptions for policy, but that is usually the end-point of a long and winding path. Often, however, especially in matters of economic development and growth, the problems under investigation are large and urgent, so practitioners want immediate answers. Academic researchers are also tempted to offer suggestions for policy, however tentative or incomplete.

Work on institutions and growth is a case in point. When I present my own theoretical models to audiences, I am almost invariably asked to interpret the results for their policy implications. The listeners usually find my responses meager and unsatisfactory, and I am compelled to agree. Today I want to take this opportunity to apply the same, admittedly very stringent, test of policy relevance to all academic research in this area. I will interpret all findings of this body of work as if they were prescriptions for policy or institutional reform, and question the validity and practicality of these interpretations.

I am deliberately going to be provocative and critical, but will try to be evenhandedly so. I hope that my remarks will give everyone some incentive to think further and harder. I also hope to help scholarly researchers better see their own work in the context of the bigger picture, and help practitioners better appreciate the difficulties of drawing implications for action from an ongoing process of academic exchange.

My primary focus is on institutions of economic governance – protection of property rights, enforcement of voluntary contracts, and provision of public infrastructure and services that support private economic activity. This relates on one hand to the broader question of the emergence and reform of deeper institutions such as democracy, in so far as that influences the shape and function of the institutions of economic governance and in turn affects economic performance. It also relates on the other hand to some specific organizations that implement the rules of the game put in place by the institutional setting, and even to some specific policies in so far as they illustrate the

issues involved in thinking about institutional change. There is a spectrum from deep institutions to specific policies; my discussion is located somewhere in the middle of this spectrum, with focus blurring toward either end. One more restriction, dictated by the limitations of my own expertise, is that I focus on microeconomic issues and institutions, not those of macroeconomic policy.

Even within these confines, the literature is huge, and I cannot hope to include even a substantial fraction of it in the space and time available. However, those left out should be relieved, not upset: the policy recipes that emerge from this literature are almost invariably unsatisfactory, so being omitted from the list and the implied criticism, however friendly and constructive, may perhaps be regarded as a good thing.

Critical assessments of the contribution of academic research to development policy have a long tradition. Most recently, Easterly (2001, chapters 2-7) has produced such a comprehensive, compelling, and entertaining critique of theories of economic growth. He concludes that the prescriptions of these theories – policies to increase saving, investment, foreign aid, and education, to modernize technology, and to reduce population growth and debt – mostly failed because other policies of LDC governments often maintained or created perverse incentives. He concludes that the path to success must begin by setting up “quality institutions” (p. 252), but says little about how this can be done. I will try to extend the critique to the research on institutions, by subjecting this literature to a prospective test of usefulness; a retrospective test will have to wait for the outcomes of any attempts to use it.

Here are some examples of broad categories of ways in which the accumulated research on the role of institutions in development stops short of giving any useful or reliable policy prescriptions. (1) Some of the work finds that development success is contingent upon some historical or geographic preconditions that most countries do not meet. (2) On many crucial issues, from deep institutional divides of democracy versus authoritarianism, to general policy approaches such as comprehensive change versus gradualism and sequencing of reforms, all the way to very specific policies toward

population, inflation, regulation, and so on, the literature includes support for diametrically opposed prescriptions, each of which sounds quite plausible when presented on its own. Thus, unlike the theories of economic growth that serially offered definite answers (which did not work in practice), theories of institutions simultaneously offer different and conflicting answers. (3) The recipes for success are often motivated by ideological fashions or a herding instinct that follows the latest perceived success. (4) The recipes that seem most promising are very often at a level of generality that stops far short of practicability. Examples are advice to build institutions that complement existing ones, or to build participatory democracy; this leaves out all details of how to identify the complementarities, or how to convert an authoritarian regime into a participatory democracy.

My conclusion from this overview of the literature is that it has not elucidated the mysteries of successful development, and we cannot draw many reliable and practicable policy conclusions from it. But I do not want to be entirely negative. In the concluding section I develop a promising framework or methodology of research that combines general conceptual and empirical findings from academic research and the experience of practitioners, to help us narrow down or identify the causes of failures in the cases of individual countries. This can then serve as the first step toward remedying the failures or removing the obstacles.

### **1. Infeasibility – The “Irish” Recipes for Success**

Numerous researchers have found significant correlations across countries between various geological, ecological, geographic, and historical data and economic performance. Taken literally, these findings constitute a message of pessimistic determinism: if your country lacks the right prior or starting conditions, its economic future is bleak. Of course many of the correlations are not to be taken literally, but I will start by listing them all on an equal footing of literal interpretation, and then develop further arguments and different interpretations. To make them amusing and memorable, I call these findings “Irish recipes for success”, after the story of the Irishman who was asked for directions and replied “If I wanted to go there, I wouldn’t start here.”

Many geological and geographic characteristics have the greatest appearance of determinism. The extremely long run processes described by Diamond (1997) are of course the most extreme examples of such determinants of economic outcomes: an east-west rather than a north-south alignment, the kinds of animals and plants that were native to the region, and so on. We are also told that it helps to be far from the equator, in the temperate rather than the tropical belt (Gallup, Sachs, and Mellinger 1999, Hall and Jones 1999), unless your country is a small Pacific island, in which case distance from the equator is one of the factors conducive to a total collapse of your ecology, economy, civilization, and population (Diamond, 2004). Being far from the core areas of the world economy is a major piece of bad luck, and being landlocked makes it worse (Gallup, Sachs, and Mellinger 1999). You might have thought that your country was fortunate to be endowed with abundant natural resources, but the facts are just the opposite. Resource abundance worsens your growth prospects; Sachs and Warner (2001) summarize and extend these findings; see also Engerman and Sokoloff (1997). If your land is suitable for plantation agriculture, that is also bad for your economic performance (Engerman and Sokoloff (1997).

On the other side of the balance, if your region was sparsely populated and poor at the time of the spread of European colonialism in the 1500s, your country is more likely to be economically successful now (Acemoglu, Johnson, and Robinson 2002, Engerman and Sokoloff 2002). It was a special help to be colonized by the British, so a larger fraction of your population became fluent in English (Hall and Jones, 1999), and inherit British rather than French or other legal and administrative institutions, giving better protection to the rights of small shareholders and therefore allowing broader and more efficient capital markets (La Porta et al., 1998, 1999). If the local diseases were more likely to take heavy toll of the early colonizers, your economic performance today is less likely to be good (Acemoglu, Johnson, and Robinson 2001). And numerous economic and social scientists from Weber onward have appealed to higher authority, finding roots of success in Protestantism, Confucianism, and so on.

Interpreted literally as recipes or policy recommendations, these require a less-developed country to use plate tectonics to move itself to a more favorable location, or to turn the clock back and invite British colonizers, of course cleaning up the local disease environment and getting rid of mineral resources beforehand. As a practical matter, these findings are merely telling countries to accept their fate. Perhaps at best they could be reinterpreted as telling the fortunate countries to share their good luck by giving massive amounts of aid to the unfortunate.

Of course this is not the interpretation the researchers intend; they intend many of their history and even geography variables to have only indirect effects on economic outcomes via some other proximate determinant of success, or to be mere econometric instruments used for identifying the direction of causation. This is emphasized by the authors of these articles, and by others such as Rodrik (2004 a), who also makes an important cautionary remark: “Finding an appropriate econometric instrument is not the same as finding an adequate explanation.” The supposed proximate determinants of success are categorized as institutions (Acemoglu, Johnson, and Robinson 2001, 2002, Engerman and Sokoloff 1997, 2002), or social infrastructure (Hall and Jones 1999). Here are some examples of the suggested channels.

If a colony had rich mineral resources or climatic and soil conditions conducive to plantation agriculture, and a large or dense population that could be press-ganged into work in such activities, then the European colonizers established institutions such as slavery and inequality that facilitated extractive and plantation activities. Institutional change is a very slow process; old institutions persist or shape today’s institutions. But they are less suitable for modern economic conditions, where broader participation in the decision-making process, education, access to economic opportunities, and protection of everyone’s property rights are needed for success. Conversely, areas that were sparsely populated and poor in the 1500s were likely to lack such get-rich-quick opportunities. There the European colonizers set up institutions conducive to longer-term economic success. (See Engerman and Sokoloff 1997, 2002; Acemoglu, Johnson, and Robinson 2002. See also an excellent account of this research in Hoff 2003.)

If European colonizers were subject to local diseases and suffered high mortality in a colony, they were likely to establish extractive economies and institutions; if they had good prospects of long lives as producers, they were likely to settle, and establish economic activities and supporting institutions conducive to long-run economic success. (See Acemoglu, Johnson, and Robinson 2001; see also the critique by Albouy 2004, and reply by Acemoglu, Johnson, and Robinson 2005.)

More broadly, Hall and Jones (1999) regard “various correlates of the extent of Western European influence,” primarily distance from the equator and the prevalence of English, French, German, Portuguese, and Spanish as first languages in the countries, as instruments for “social infrastructure,” measured by an index of lack of corruption (government antidiversion policies) and openness to international trade.

The need for instruments arises because of potential reverse causation: good institutions are likely to be conducive to good economic performance, but citizens of economically successful countries may also demand and implement good institutions. However, Kaufmann and Kraay (2002) find a weak but negative reverse causation, suggesting the absence of a virtuous circle between better governance and better economic outcomes. Keefer (2004 b) also argues for a weak or even negative reverse causation, combining conceptual arguments and empirical evidence. But even negative reverse causation can create an econometric problem requiring instrumental variables.

Formally, let  $y_i$  denote the economic performance of country  $i$ , let  $x_i$  be a vector of measures of its institutions, and  $z_i$  a vector of other relevant variables. The hypothesis being tested is

$$(1) \quad y_i = a_1 + a_2 x_i + a_3 z_i + e_i$$

There is reverse causation from the  $y_i$  to the  $x_i$ , so one invokes instrumental variables that are assumed to affect the  $x_i$ , but have no direct effect on the  $y_i$ . Thus

$$(2) \quad x_i = b_1 + b_2 y_i + b_3 z_i + b_4 g_i + u_i$$

One then estimates (1) by two-stage least squares, estimating the  $x_i$  from the first-stage equation

$$(3) \quad x_i = c_1 + c_2 z_i + c_3 g_i + v_i$$

and using the estimated values in the regression for (1). This requires that the  $g_i$  do not appear directly in (1), and are uncorrelated with the error term  $e_i$  in that equation. One constructs various stories for why that should be so. For example, Hall and Jones (1999) say the following about their “European influence” instruments: “we must ask whether European influence was somehow more intensively targeted toward regions of the world that are more likely to have high output per worker today. In fact, this does not seem to be the case. On the one hand, Europeans did seek to conquer and exploit areas of the world that were rich in natural resources such as gold and silver or that could provide valuable trade in commodities such as sugar and molasses. There is no tendency for today for these areas to have high output per worker. On the other hand, European influence was much stronger in areas of the world that were sparsely settled at the beginning of the sixteenth century ... Presumably these regions were sparsely settled at that time because the land was not especially productive given the technologies of the fifteenth century. For these reasons, it seems reasonable to assume that our measures of the Western European influence are uncorrelated with [the error term].” This discussion also relates to the instruments and their discussion in the other work of this kind cited above.

Many of these stories do sound plausible. But how does one formally test them? In other words, how does one test the difference between the system (1)-(2) and an equation like

$$(4) \quad y_i = d_1 + d_2 z_i + d_3 g_i + w_i$$



that allows economic success to depend directly on the supposed instruments and not on the supposed proximate institutional determinants at all?

If there is just one instrument, for example colonial mortality in the primary specification of Acemoglu, Johnson, and Robinson (2001), then the system (1)-(2) is just-identified, and econometrically indistinguishable from (3)-(4), unless one is willing to make other assumptions about error variances or measurement errors; this is the procedure adopted by Hall and Jones (1999), Acemoglu, Johnson, and Robinson (2001), and Kaufman and Kraay (2002). This involves some other untested assumption. If there is more than one instrument ( $g_i$  is a vector), then one can perform overidentification tests, but these are weak and too likely to accept the null hypothesis that the  $g_i$  are excluded from (1). In Acemoglu, Johnson, and Robinson (2002), the geographic reversal of fortune instrument is tested in such a way, assuming that colonial mortality is a genuine valid instrument; but that is not problem-free as was just argued.

My view is that the idea that geographic and historical variables are merely instruments for institutional determinants of economic success is supported more by the intuitive appeal of the stories told than by the statistical significance of the tests performed. The value of rhetoric should not be ignored, but I wish the econometric evidence were stronger and more compelling. This is an area where more and sharper research is badly needed.

But let us proceed on the basis that we are convinced by these stories, and accept that institutions are important. What policy implications follow? The literature reviewed above gives us no guidance on this question; whether geography or history have a direct effect or an effect via institutions, a recommendation to change one's geography or history is useless. One must forget about history and geography and try to implement the relevant institutions directly. For advice on that, we must turn to some other research.

## **2. Contradictions – “Doctor Dolittle” Recipe Pairs**

There is no shortage of academic research that identifies institutions and policies to promote economic growth and development, and proffers advice on how to acquire good institutions. Indeed, “Institutions for High-Quality Growth: What They Are and How to Acquire Them” is the title of Rodrik’s (2000) paper. The trouble is that for every paper that endorses one kind of institution or policy, one finds another that makes precisely the opposite claim. Each is written by a prominent economist, and contains impressive arguments and evidence to support the recommendation being made.

I will now examine several dimensions of the contradictory conclusions of the literature on institutions and policies. They range from the largest choices of the deepest institutions that govern the society and the polity, to some minutiae of specific policies. Once again I have chosen a whimsical and memorable label for this literature. To remind us of the two-headed animal “pushme-pullyou” in the book and musical, I call these the “Doctor Dolittle” pairs of recipes for development.

#### *A. Democracy versus Authoritarianism*

At a broad and general level, cross-country regressions yield mixed results on the question of whether democratic or authoritarian governments are better for growth. For example, Barro (1999, p. 61) found a relatively poor fit and an inverse U-shaped relationship. He suggested that “more democracy raises growth when political freedoms are weak, but depresses growth when a moderate amount of freedom is already established.” Persson (2005), using cross-sectional as well as panel data, finds that the crude distinction between democratic and non-democratic forms of government is not enough; the precise form of democracy matters for policy design and economic outcomes. “Reforms of authoritarian regimes into parliamentary, proportional, and permanent democracies seem to foster the adoption of more growth-promoting structural policies, whereas reforms into presidential, majoritarian, and temporary democracy do not.” However, Keefer (2004 a), after surveying a wide-ranging literature on electoral rules and legislative organizations, concludes that they affect policies but are not a crucial determinant of success: “electoral rules ... almost surely do not explain why some

countries grow and others do not,” and “the mere fact that developing countries are more likely to have presidential forms of government is unlikely to be a key factor to explain slow development.”

What are the reasons or mechanisms that can explain the claimed superiority of democracy? Rodrik (2000) emphasizes the importance of local knowledge in the process of successful institution-building, and argues that participatory democracy is meta-institution that facilitates such use of local knowledge and thereby enables higher-quality growth. Besley, Persson, and Strum (2005) using data from the United States argue that political competition is a key to better economic policies and outcomes, and this also has intuitive appeal. Besley and Burgess (2002) using panel data from India find that an informed and active electorate leads to effective incentives for governments to respond to economic problems, and that mass media play an important part in this process. In other words, democracy succeeds by facilitating voice and participation. Bardhan (2005, ch. 1) also stresses the importance of democratic participation in a generalized interpretation of the “rule of law.” Islam (2003) emphasizes the role of information and transparency in improving the quality of governance institutions.

Some who favor democracy over authoritarianism in a comparison of ongoing systems may nevertheless be concerned about costs of disruption in a transition to democracy. For them, Rodrik and Wacziarg (2004) bring good news: using annual panel data for over 150 countries (with separate subsamples analyzed for some questions), they find that transitions from authoritarian to democratic regimes lead to striking and statistically significant improvements in GDP growth per capita; for example, the effect is as large as 2.8% per year for sub-Saharan Africa.<sup>1</sup> Growth also becomes less volatile after a transition to democracy. Declines in growth precede, not follow, such transitions.

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<sup>1</sup> Actually, Rodrik and Wacziarg find that either kind of transition is followed by better economic growth, but the effect is smaller for transitions from democracy to authoritarian rule than for transitions in the other direction. This suggests an alternative hypothesis that can explain the same facts. Suppose that purely exogenous random shocks are the main determinants of growth, but the public gives the rulers credit for good results and blame for bad results. Rulers are overthrown when the results are particularly bad. Then the average improvement in growth following a regime change is just a reversion to the mean, and the effect is stronger for transitions to democracy because authoritarian rulers can be overthrown only when the economy performs drastically badly whereas democratic governments may fall for smaller shortcomings.

These are just a few examples from a vast literature, and they add up to a message that is pleasing and even uplifting to many modern academics and policy practitioners alike – democracy is good not only for its moral and human appeal, but also for its economic performance. This would be a “warm glow recipe” or a “happy ending.”

But not so fast. There is an equally impressive emerging literature that makes a serious case for authoritarian governments and institutions when it comes to starting growth and development. Glaeser, La Porta, Lopez-de-Silanes and Shleifer (2004) argue that less-developed countries that achieve economic success do so by pursuing good policies, often under dictatorships, and only then do they democratize. While these conclusions are controversial, these authors’ criticisms of the measures of institutions used in the research that argues for the primacy of institutions in general, and of democracy in particular, are telling. Giavazzi and Tabellini (2004) find a positive feedback between economic and political reform, but they also find that the sequence of reforms matters, and countries that implement economic liberalization first and then democratize do much better in most dimensions than those that follow the opposite route.

This debate looks likely to go on for a long time, with contending arguments about basic concepts and theories as well as empirical methodologies, data, and anecdotes. Here I would like to say something brief about two questions.

First a conceptual matter: what feature or features of policies are important for good economic outcomes, regardless of what kind of government makes those policies? Here much of the literature does find one point of agreement: credibility of commitments is vital. Acemoglu (2003) argues that the fact that political contracts lack third-party enforcement makes it harder to make credible commitments, and this explains the absence of a Coase Theorem ensuring efficient outcomes in political bargaining. In his comprehensive overview of different dimensions of governance and their effects on economic development, Keefer (2004 a, b) also identifies the ability to make credible commitments as crucial. Bardhan (2005, ch. 4) offers a good discussion of the concept of

credibility, and of its lack as a barrier to reform. In their illuminating analytical narrative of Argentine politics, Spiller and Tommasi (2003) that the institutions and organizations of political bargaining in that country, and the volatility of its economic environment, combined to inhibit the capacity of its policymakers to “undertake efficient intertemporal political exchanges,” and this led to “low-quality policies” on many issues such as regulation, and control of inflation.

How do democracy and authoritarianism compare in this respect? Keefer (2004 a) stresses the role of checks and balances in achieving credible commitments, and these are more likely to be present and effective in democracies. However, one might argue that commitments coming from a dictator or authoritarian ruler should be more credible, if anything, so long as he is stable in his rule. And such a ruler does have incentives to pursue policies that enhance economic success. Even if his aims are predatory, if he is sufficiently stable in his position, he will best increase his own take if the pie is larger; this is Olson’s (1993) stationary bandit. Whereas, if he fears being overthrown, improving the people’s economic well-being may be his best way of postponing that fate.

China is usually cited as an example of an authoritarian regime where formal property rights are not formally protected, but are de facto quite secure to the point that the country can attract large investments from abroad. Different explanations of this can be found in the literature. Qian (2003) and Rodrik (2004 b) find it in the Chinese institutional innovation of township and village enterprises (TVEs). This “force[ed] entrepreneurs into partnership with their most likely expropriators, the local state authorities. ... Local governments were keen to ensure the prosperity of these enterprises as their equity stake generated revenues directly for them.” This puts the local authorities in a role similar to Olson’s stationary bandit. However, the strategy sounds very similar to the concept of “insider privatization” pursued in Russia, and supported by Shleifer and Treisman (2000, pp. 31-2), which did not work so well there. What was the difference?

McMillan (2003, pp. 98-100) argues that in the phase of China’s agricultural reform, “productivity gains were achieved without formal legal recognition of farmers’

ownership rights. ... Although the authorities are able to renege on contracts, they have refrained from doing so with sufficient predictability that the farmers are motivated to be productive.” He finds the probable reason in the “specifics of time and place. ... The communist government faced no challenge, [but] its legitimacy as the government, and its ability to preempt any future political opposition, rested on its delivering economic growth. High officials in Deng Xiaoping’s government understood enough about economics to recognize that growth requires markets and markets require assured property rights. The Communist Party had retained its highly disciplined organization and so was able to prevent self-seeking behavior by low-level officials.” This is the idea of a dictator operating under a constraint that captures his fear of losing power. Indeed, it appears that China retains several aspects of democracy at lower and middle levels of institutions and economic policymaking: there are some genuinely contested elections at these levels, press criticism of officials at this level is tolerated and perhaps even encouraged, and corruption is swiftly and severely punished when detected. Only at the top level is the Communist Party’s rule rigid and unchallengeable.<sup>2</sup>

Thus it may be possible to combine the two forms of government institutions for optimal economic performance. However, there is always the problem of how to ensure ex ante that your authoritarian ruler turns out to be a benevolent promoter of economic success and not a predatory despot. And if your country has the misfortune of getting a predatory despot, you face the bigger problem of how to get rid of him. Similarly, the voluminous literature has almost unanimously made a convincing case that credible protection of property rights is essential for investment and innovation and therefore an important necessary condition for development success. But this literature gives us no guidance on the operational question of what to do if one finds oneself in a policy role in

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<sup>2</sup> One other feature of economic reforms in China is often highlighted and praised (for example Qian 2003, Rodrik 2004 b): China preserved established entitlements in an inframarginal manner, while liberalizing activity at the margin. For example, the state purchase system and price subsidies for agriculture were retained, thereby protecting government revenue and urban living costs, but gave farming households the freedom to sell the remaining output after meeting the state quotas at free market prices. However, this policy decision is conceptually distinct from the issue in this section, namely democracy versus authoritarianism.

a country where a despot cancels these rights at his whim, or a democratic government does so at the behest of politically important special interests.

### *B. Formal versus Informal Governance Institutions*

Institutions to protect property rights and enforce contracts can be formal – the state’s apparatus of legislation, policing, and the judiciary – or informal – social networks, communication channels, and norms, and for-profit information and enforcement services including but are not limited to organized crime. Many variants and mixtures of formal and informal systems exist in practice in most countries. We see private adjudication that has the backup of formal enforcement; for example industry-based arbitration panels can be recognized by courts, who then merely act to enforce the judgments issued by arbitrators. Rubin (1994) recommended this approach for building governance institutions in transition economies. We also see private enforcement of formal laws, ranging from private initiation of litigation to private infliction of punishments; Berglöf and Claessens (2004) and Hay and Shleifer (1998) favor various forms of this approach.

What are the relative merits or flaws of formal and informal institutions of economic governance? The literature on this question also contains a bewildering and often mutually contradictory spectrum of arguments and evidence.<sup>3</sup>

A useful conceptual framework for studying the formal-informal distinction is the distinction between relation-based and rule-based governance of contracts. In relation-based governance, transactions occur between parties both of whom are members of the same group or network, based on ties of language, ethnicity, locality, or the like. Information circulates within the network, and helps each member both in the search for

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<sup>3</sup> I restrict attention to institutions of property right protection and contract enforcement, but institutions and agencies for regulation of product and labor markets, taxation, trade and so on show an equally bewildering variety, and equally complex considerations as regards their relative merits. Islam (2004) considers the institutions that affect international trade, and concludes that “increasing trade integration is consistent with a wide array of institutional choices.” However, it is less easy to specify which choice is best under what circumstances.

a partner with whom to transact, and in obtaining information about the capability and trustworthiness of a prospective partner. Contracts are personal and implicit agreements. Any breaches of promises are communicated to the group, and other members of the group can punish the miscreant by refusing to trade with him in the future, or by extra-economic sanctions such as social ostracism or worse. Rule-based governance, by contrast, relies on formal contracts and their enforcement by the courts, or the police if necessary.

Although this distinction was known and studied for many years in many contexts, Li (2003) gave it sharper analytic content by clarifying the main difference in the costs of establishing and operating the two systems. Relation-based governance has few fixed costs. It does not require a framework of laws, regulation, or of course, of courts; one just starts dealing with one's friends and neighbors. But its marginal costs are significant and increasing as the scope of trade expands: one starts by dealing with close neighbors whose trustworthiness is better-known, and who are more likely to behave well in dealings with one because there are likely to be more frequent and larger future links among close neighbors. As one's business expands, it becomes necessary to deal with more distant strangers, who are less likely to be trustworthy. By contrast, rule-based governance has high fixed costs of legislation, regulation (such as reporting and auditing requirements that create information about traders), and enforcement. But once these are in place, one can deal with strangers at low marginal cost. Because of these cost differences, the relation-based system is better at small scales of transaction, and the rule-based system at large scales. Dixit (2004, ch. 3) constructs a formal model to examine and compare equilibria under the two systems; this elucidates the interaction of different parameters and mechanisms that determine the limits of the relation-based system.

This was precisely what Greif (1993, 1994, 1997) found when comparing two groups of traders around the Mediterranean in the medieval period. The Maghribis relied on relation-based governance within their tight group of Jewish traders; this worked well up to a point but eventually could not cope with the expansion of trade to newer and farther locations. The Genoese traders used a more formal system of registering contracts



with the authorities in Genoa, and having any disputes investigated and adjudicated by these authorities; this proved better as trade expanded. More recently, microcredit has been successful in many small communities and groups, but expanding its scope to larger financial markets for lending, borrowing, and investing has proved more problematic. A possible approach is to link the small communities together at an upper tier using the kind of system Bernstein (1992) describes for the diamond industry, or the “community responsibility system” in medieval Europe studied by Greif (2004).

Worsening quality of information and communication is not the only problem that limits the expansion of informal institutions. Another issue is inherent in the concept of a “community” that develops these informal institutions. This is a group of “insiders,” which by its very nature requires a concomitant definition of “outsiders” who are to be excluded. Therefore informal institutions usually erect entry barriers that inhibit fuller exploitation of opportunities and development of capital markets. Fafchamps (2004, pp. 455-456) develops this idea and offers examples.

When comparing the relative merits of formal and informal, or rule-based and relation-based, institutions of governance, it may be important to distinguish between enforcement of contracts and protection of property rights. Informal, relation-based systems seem to be less successful in protecting property rights than in enforcing contracts. Li (2003) confines most of his discussion to contracts, and in fact tacitly assumes a background of formal institutions that include property right protection: he says that relation-based governance “requires only minimal public order – that is, the general absence of rampant robberies or confiscation” to get going. And Gambetta’s (1993) study of the Sicilian mafia indicates that their performance in enforcing contracts could be socially beneficial, whereas in their role of protecting property, “protectors, once enlisted, invariably overstay their welcome” (ibid, p. 198). Even with contracts, information services about trustworthiness do not generate problems of violence, whereas enforcement services to punish breaches do (ibid, p. 18); see also Dixit (2004, ch. 4).

De Soto (2000) emphasizes the importance of formal recognition of property rights (pp. 5-6): “in Asia, Africa, the Middle East, and Latin America, ... most of the poor already possess the assets they need to make a success of capitalism. ... But they hold these resources in defective forms: houses built on land whose ownership rights are not adequately recorded, unincorporated businesses with undefined liability, industries located where financiers and investors cannot see them. Because the rights to these possessions are not adequately documented, these assets cannot readily be turned into capital, cannot be traded outside of narrow local circles where people know and trust each other, cannot be used as collateral for a loan, and cannot be used as a share against an investment.” But he recognizes the importance of building upon local knowledge (De Soto, 2004, p. 108): “As I strolled through rice fields [in Bali], I had no idea where the property boundaries were. But the dogs knew. Every time I crossed from one farm to another, a different dog barked. Those Indonesian dogs may have been ignorant of the formal law, but they were positive about which assets their masters controlled.” So officials who wanted to set up a formal property system could “by traveling their city streets and countryside and listening to the barking dogs, ... gradually work upward.”

Most theoretical and empirical studies find that purely informal institutions of governance eventually act as a constraint on growth and expansion of trade. No country switches to purely formal institutions; even in the most advanced countries, much economic activity continues to be governed by relational and private ordering “under the shadow of the law”; see Bernstein (1992, 2001) for some examples of such industry-based adjudication in the U.S. But it remains important for less-developed countries and former socialist countries to develop more formal institutions that can govern increasing volumes of arm’s length transactions. This transition brings its own difficulties, and evidence as well as arguments on this issue yield mixed results. Some formal institutions can meld well with the existing informal ones, for example Rubin (1994) and Dixit (2004 ch. 2) argue that arbitration does, although Widner (2000) has challenged the efficacy of specialized courts. In other instances, the two institutions clash with each other and the formal one may lose; Ensminger (1997) gives an example of land title reform in Kenya. Traditional property rights to land in many parts of Africa are a complex system. Clan

chiefs grant titles to individual families. Sales are subject to their approval and also that of family heirs since all sons usually have expectations of equal division. Many family stakeholders have usufruct rights. When the Kenyan government attempted to impose a system of formal land titles, this ran into conflict with the traditional arrangements. The expected capital market did not develop because lenders knew that foreclosure was infeasible in the face of opposition from family and community, so the land could not be used as collateral. Attempts to consolidate scattered holdings for scale economy reasons did not work because there was a good economic reason (insurance) for the scattering. Many formally registered titles are now being allowed to lapse and revert to older arrangements, and the laws are being changed to resemble traditional forms more closely. Finally, theoretical literature using a repeated game framework shows how a partial improvement of an imperfect formal system, by providing a better outside alternative and thereby making the consequences of breaking a relational contract less bad, can worsen the outcomes from the existing informal system (Baker, Gibbons, and Murphy 1994, Dixit 2004 ch. 2).

### *C. Comprehensive and Rapid, versus Sequential and Gradual, Reforms*

This is an old debate in the development field, and continues unabated. Arguments for speed include the following: (1) Windows of opportunity for reform present themselves rarely in political negotiation, so they should be seized when they do arise. (2) Reforms cause some pain to some people or groups, and gradualism prolongs this period of pain, thereby risking the whole reform. (3) Policy actions convey useful signals about the government's intentions to financial markets and investors, and in the standard Spencian manner, a reforming government must engage in excessive signaling to separate itself from a less purposeful one. (4) The different dimensions of reform are strategic complements, and results will be poor unless all dimensions are tackled jointly (see Kramer 1993 a for the "O-ring" theory of this general class of phenomena). Others favor a more cautious approach. Their arguments include the following: (1) Drastic reforms, by inflicting severe pain, and likely to arouse more opposition and thereby create a greater risk of political opposition leading to failure. (2) Institutional change is a

slow process since it requires changes in long-held expectations. Prominent early supporters of speed and comprehensiveness include Åslund (1995); those on the side of caution include Murrell (1992) and Desai (1995). The World Bank (1996), in its World Development Report, on balance emerged in favor of speed, but with many cautions and caveats. Heybey and Murrell (1999) offer an empirical assessment based on more recent data. They find that the initial conditions of a country are more important than policy changes in determining its economic performance during the first few years of transition; that is, speed is less important either way. They also find a negative feedback in reform: higher levels of initial liberalization are likely to slow down subsequent reforms.

A somewhat different recent argument in favor of one kind of gradualism, and a new view of optimal sequencing, says that the most productive institutional changes in the early phases of development are very small and easy to implement; the harder job is to build on them and sustain growth beyond a medium level. Thus Pritchett (2003, p. 148) concludes from an analytical case study: “Under a regime that has reasonable institutional stability and is not completely dysfunctional, a rapidly increasing level of GDP per capita is possible up to semi-industrialization. ... a rough and ready system for enforcing contracts and providing for stability of investors’ expectations” suffices to “support quite rapid growth if beginning from a low base.” Even some corruption can be tolerated so long as it is “predictable,” that is, investors are clear as to whom to bribe and how much, and can rely on the official delivering on the quid pro quo. “However, what trips countries up is the *transition* from one set of institutions to another,” (emphasis in the original) because this requires a change in expectations and creates uncertainty during the transition. Rodrik (2003, p. 17), in his overview of Pritchett’s paper and several other others, similarly concludes that “the policies required to initiate a transition from a low-income equilibrium to a state of rapid growth may be qualitatively different from those required to reignite growth for a middle-income country.” This fits with the ideas of Li (2003) and Dixit (2004, ch. 3) discussed above: so long as some basic protection of property rights exists, even formal contract enforcement may not be needed in a small economy – self-enforcing relation-based governance can work well. But to go beyond that stage, formal institutions of laws, regulatory agencies, courts, and the police are

needed, and a malfunction in any part of this complex can create serious obstacles and setbacks to growth.

This argument can be linked to the idea of strategic substitutes and complements. Where different institutions are substitutes, albeit imperfect ones, it is fine to start with the simpler one and then move to a more complex and better one. But if the institutions are mutually complementary, it is necessary to implement them in a package; for example, privatization may fail unless there is an adequate structure of regulation, and a good forum for adjudicating commercial disputes may be futile unless there exists a way, public or private, to enforce the decisions of this forum. Therefore, to implement this recommendation for gradualism successfully, a policy practitioner must identify which policies or institutions are substitutes and which ones are complements. This is a difficult task, for which the existing literature offers only a few and incomplete hints.

#### *D. Imitation versus Innovation*

Linked to the conflicting arguments and evidence in the sections B and C above is the question of whether to use the readily available models of Western legislative and judicial institutions or to develop ones specifically tailored to ones country. Many of the pertinent issues have been outlined above, so to save space I will not discuss them again but merely mention their bearing on this question.

#### *E. Crises – Good or Bad?*

Also linked to the question of speed and comprehensiveness of reforms is that of the possibly beneficial consequences of crises. Olson (1982) presented this thesis in the clearest form. According to him, many inefficiencies in prevailing institutions arise to serve organized special interests, and the entrenched political power of these groups prevents reforms that would be beneficial in the aggregate. Crises weaken or dissolve these special interests, and allow a fresh start. Williamson (2000, p. 598) makes some related observations. And Calvo (2005) points out that many recent crises have been

followed by a sharp recovery in which the usual factors like domestic bank credit, current account deficit, and investment were only small contributors – a miracle of a “Phoenix-like” rise from ashes.

It is hard to imagine a country deliberately risking a grave crisis in order to start the Olsonian process; this might be called the “We had to destroy that country in order to save it” recipe. Moreover, other researchers argue that crises are not necessarily beneficial. The theoretical model of Hsieh (2000) clarifies the issues. The dynamics negotiation on policy reforms following a crisis is a “war of attrition” or “chicken” game; the question is who will concede first. With incomplete information about the alternatives, value systems, and impatience of other players, such games can have equilibrium strategies that involve lengthy periods without agreement, and may sometimes result in total breakdowns. Whether crises change these equilibria for the better is ambiguous. Dramatic practical examples of crises leading to total collapse are in Diamond (2004).

#### *F. Population – Resource or Curse?*

Thus far I have outlined several contrasting results and conflicting prescriptions in the literature on institutions and their reform. A similar situation also exists with regard to policies on specific problems. Population is a good example. A general presumption dating back to Malthus is that a large and rapidly growing population is bad for economic success. In the standard neoclassical growth model, a larger rate of population growth leads to a steady state with lower output per capita. When one considers depletion of natural resources, the consequences can be more drastic; Diamond (2004) is only the latest of a long line of prominent scientists who have sounded alarms on this issue.

But once again, there are strong counterarguments. Boserup (1981) has argued that a high density of population is conducive to faster technical progress. And growth models that incorporate such positive feedbacks, for example Lee (1988) and Kramer (1993 b), find superexponential growth. Platteau (2000) finds that high density is also conducive to the emergence of better networks and institutions.

### *G. Whom to Believe, and What to Do*

I have sketched pairs of starkly conflicting research findings on a number of important issues of institutions and policy. This can leave a user who is not an expert in a particular area in a thorough state of confusion and indecision. Perhaps that is no bad outcome. In any such situation, readers may find one side to be more convincing, but rarely are the arguments and evidence from the other side clearly wrong or unambiguously inferior, and many readers will differ in their judgment. Confronted with such uncertainty about the true model and the factual details, practitioners who judge the balance of considerations to favor one side would nonetheless be wise to proceed with caution. The decisions being made here have immense consequences, and they are very difficult to reverse. Therefore any uncertainty as to which is the better choice weighs heavily in the decision. The favored choice must be not merely better than the other, but better by a sufficiently large margin, to justify going ahead. Otherwise waiting for better information is optimal; there is an option value to waiting. (See Dixit, 1992 or Dixit and Pindyck, 1994.) This important lesson of the “real options” theory of irreversible decisions under uncertainty is very relevant in this context.

There is another equally important problem when it comes to action. Even if you are convinced by one side in the argument, and want to follow it through into institutional reform or policy action, there may not be much you or almost anyone can do. Suppose you are the Minister for the Economy in an authoritarian regime. You read the writings of Rodrik and others about the virtues of democracy, and are thrilled by the thought of having this “meta-institution” that harnesses “local knowledge” in your country. What do you do? Of course, if you are the Minister for the Army in a democracy and are convinced by the pro-authoritarianism arguments, you may find it a little bit easier to implement your favored institutional reform!

### **3. Shifts of Consensus**

In the previous section, I laid out several stark disagreements that exist among researchers at the same time, and showed how difficult it is to find reliable and feasible policy recommendations from this chaos. This “cross-section problem” is augmented by a “time-series problem.” Among researchers as well as practitioners, there may be a reasonable consensus at any one time, but the consensus can shift dramatically over time. Here is a very brief outline of some such changes, and the warning they should convey.

#### *A. Chasing the Latest Success Story*

At any time, some country is doing well, and academic as well as practical observers are tempted to generalize from its choices and recommend the same to all countries. After a decade or two, this country ceases to do so well, some other country using some other policies starts to do well, and becomes the new star that all countries are supposed to follow.

In the 1950s, many intellectuals expected the U.S.S.R. to grow rapidly, and some form of central planning, including import-substitution and state investment in heavy industry, was offered as the key to success. Starting in the 1970s through the 1990s, Japan and other emerging East Asian economies became the heroes, and markets and openness became the vogue. Now China is the great example to follow. What next?

#### *B. Role of International Institutions and Organizations*

For the last 50 years or longer, the World Bank, the IMF, and other international institutions and organizations have played significant roles in giving advice, finance, and aid to less-developed countries and transition economies. But there is shifting perception as to exactly what their role should be. At one time they were supposed to be the enforcers and guarantors of a country’s commitment to policy reform; conditionality was the key. But the conditions were criticized as “one-size-fits-all” recommendations. Designing policies differently in each country based on local knowledge to fit its conditions was argued to be vital, and countries’ “ownership” of reforms became the



buzzword. I like the idea that these institutions can act as facilitators or “catalysts” (McCarthy, Bader, and Pleskovic, 2003); it can combine the features of discipline and ownership. And it conjures up a nice image of a furnace or cauldron from which shining reform will emerge. Dollar and Levin (2005) present evidence that the World Bank’s project finance and aid are most productive in countries with good institutional quality; thus countries and international institutions appear to be strategic complements in the production of economic success.

#### **4. The Napoleon Prescription**

Faced with all these contradictions and shifts, I can identify only one consistently valid policy prescription.<sup>4</sup> It is the quality Napoleon valued most in his generals, namely luck. Researchers want to identify causes, and practitioners want to know what they can choose and change; therefore both sides may have neglected the important role that luck has played in many countries’ development successes or failures. Easterly (2001, chapter 10) is rare among economists in giving luck a substantial role and discussing it in considerable detail. I offer a few supplementary examples.

Japan’s success with the auto industry owes much to luck. The improvement in the quality of their small cars came at just the right time in the mid- to late-1970s, when gas price rises had shifted the U.S. demand toward small cars and the U.S. industry had not shifted its designs and production to match. And the Japanese quality upgrading in the early 1980s also hit it just right, as the “voluntary” export restraints imposed by the U.S. on Japan added an equal absolute shadow value of the quota to the prices of all cars and thereby changed the relative price ratio in favor of larger cars. As for bad luck, think of the many instances where large investments of resources and time in the building of tourism in a country go to waste because of some natural disaster or political turbulence.

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<sup>4</sup> I have left out a few even more whimsical ideas, such as “The Mouse That Roared” recipe. In that movie, a small country in Europe (the Dutchy of Grand Fenwick) declared war on the United States, with the idea that when it lost the war, the United States would give it large amounts of aid. Unfortunately for the plan, Grand Fenwick won.

## **5. Division of Labor and Diagnostics**

Why do we find so many contradictions in the research on the determinants of economic growth and development, and corresponding conflicts in the recipes for development success? Some of the explanation can be found in the diversity of theories and perspectives that different economists and academics from other field bring to this inquiry: development economics is undergoing a paradigm shift, from theories where resource and technology constraints were the key obstacles to growth, to theories where information asymmetries constrained the operation of markets, and now to theories where institutions are the key. One must expect that in the process of a paradigm shift, no one view will dominate and major differences will persist for a while. Of course this is not very satisfactory for practitioners, who cannot afford to wait until the dust settles and a new paradigm emerges.

Another part of the explanation for the current state of differences can be found in the diversity of data sources and periods over which econometric studies have been carried out. But much of the explanation is surely to be found in the diversity of the countries themselves. Each has some special aspects of its history, geography, religion, society, polity, culture, and other features that influence how well or poorly a particular prescription for institutional reform or policy change will fare when applied to it.

Another way of saying this is that econometric and theoretical studies alike are not the best way to generate policy prescriptions. Most of the cross-country regressions fit far from perfectly; the myriad explanatory variables that have been tried explain only a fraction of the variance. Theoretical modeling explores the implications of one cause or mechanism in depth, deliberately isolating it from others, whereas policy prescriptions require one to look at one country and consider all the different causes or mechanisms at work there and how they interact with one another. The question policy prescribers must address is not “What creates success on average across countries?” but “What is going wrong in this country and how can we put it right?”

This seems to suggest a division of labor – academic researchers should look for general tendencies and patterns, and should not offer specific prescriptions for individual countries. That should be left to policy practitioners in the countries or in international organizations who have much more detailed knowledge of the circumstances and needs of individual countries. Of course some academics do combine theoretical knowledge with deep knowledge of some countries; for example Fafchamps (2004) examines many African countries through the lens of game-theoretic modeling of self-enforcing governance mechanisms, and comes up with different prescriptions for three countries in different circumstances and at different stages of institutional evolution.<sup>5</sup>

How can practitioners combine their knowledge with academic research? Must this be a totally ad hoc matter, or can one develop something of a system for it? I would like to suggest a framework building on the idea of “growth diagnostics” introduced by Hausmann, Rodrik, and Velasco (2004). Their basic idea is to identify the crucial or binding constraints in any situation, and focus attention on remedying those. They develop a theoretical framework for this, and operationalize it using a “tree” much like the branching decision diagrams one finds in the “troubleshooting” sections of product manuals – “Does the computer produce four beeps when switched on? If yes, try such and such next; if no, ...” Most importantly, Hausmann et al. ask the practitioner to look at other symptoms that may be associated with low growth, and use a succession of these to pin down a cause: “Are returns to investment low? If so, is the problem a micro risk like corruption or a macro risk like financial instability? Or is it the case that investment projects with high potential returns are available but are not undertaken because the cost of finance is too high? And is the source of the high cost of finance to be found in local or international conditions? ...”

This approach has much to recommend it. Most importantly, it looks at multiple dimensions of economic outcomes and tries to narrow down from a set of multiple

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<sup>5</sup> Even such detailed and well thought-out combination of academic and field research can be blown off-course by unforeseen shocks. For example, Fafchamps regards Zimbabwe as having relatively good institutions and offers several suggestions for building on this basis to improve their financial intermediation. Just a few years later, Zimbabwe’s problems look quite different.

possible causes. But its sequential mode of thinking based on a tree structure seems problematic. In reality, each case of development failure may have multiple causes acting simultaneously. A related but more general point concerns the idea of “diagnosis” itself. Dictionary definitions of the word are some variants of “identification of a disease or disorder based on review of signs, symptoms, and laboratory findings.” This sounds like a problem of Bayesian inference, and is perhaps better captured in a table than in a tree.

Table 1 shows such a schematic representation. The first column has the various causes  $C_1, C_2, \dots$  in its rows; each “cause” could in fact be a grouping or composite of several causes. And the listing can be quite wide and comprehensive; it can include political and institutional obstacles as well as specific policy errors of commission or omission. The second column has the prior probabilities  $\pi_1, \pi_2, \dots$  with which the various causes can occur. Historical analyses can give us estimates of these probabilities; specific knowledge of a country can help us refine these estimates.<sup>6</sup> The rest of the columns have as their headings the various conceivable outcomes  $E_1, E_2, \dots$  in the top row; again each outcome could be a cluster or composite of several outcomes. The entries in the cells are then the conditional probabilities,  $P_{ij}$  being the probability that outcome  $E_j$  will occur when cause  $C_i$  is present. In principle, econometric research can give us such probabilities, although this is not how regression results are usually presented.

Table 1 – Causes, Effects, and Probabilities

Causes	Prior probs.	Effects			
		$E_1$	$E_2$	...	$E_n$
$C_1$	$\pi_1$	$P_{11}$	$P_{12}$	...	$P_{1n}$
$C_2$	$\pi_2$	$P_{21}$	$P_{22}$	...	$P_{2n}$
...	...	...	...	...	...
$C_m$	$\pi_m$	$P_{m1}$	$P_{m2}$	...	$P_{mn}$

<sup>6</sup> Medical diagnostics proceed using just such a combination of general statistical information about the relevant population and the doctor’s specific knowledge of an individual patient.

If we observe a particular effect, say  $E_7$ , then the Bayesian posterior probability that a particular cause, say  $C_5$ , is present becomes

$$\frac{\pi_5 P_{5,7}}{\sum_{i=1}^m \pi_i P_{i,7}}$$

If we want to be nearly certain whether or not a cause, say  $C_5$ , is present, we need to find an outcome, say  $E_7$ , which will more typically be a cluster of outcomes or symptoms and might be called a “syndrome” in a continuation of the medical analogy, such that

(1) it is very unlikely to occur when the underlying cause is any other cause, that is,  $P_{i7}$  is close to zero when  $i$  is not equal to 5, so the posterior probability of  $C_5$  is close to 1 conditional on observing  $E_7$ , and

(2) it is very likely to occur when  $C_5$  is present, that is,  $P_{57}$  is close to 1, so the rest of the  $P_{5j}$ 's are close to zero, and if some other effect is observed, the posterior probability of  $C_5$  becomes close to 0.

The ultimate aim would be to find a complex of causes, or a syndrome, that satisfies both these conditions. Then we can reduce the table to a two-by-two case, where the syndrome we have identified is labeled  $C_1$  and everything else is, whereas the outcome or effect we are trying to explain is  $E_1$  and everything else is  $E_2$ . In that ideal setting, the conditional probabilities would be  $P_{11} = P_{22} = 1$  and  $P_{12} = P_{21} = 0$ . Then the posterior probabilities are 1 for  $C_1$  if  $E_1$  is observed and for  $C_2$  if  $E_2$  is observed, so the outcome pins down the cause precisely. In reality we are unlikely to get a situation where causes can be discerned from syndromes quite so well, but this gives us an ideal to work toward.

I hope this very rough conceptual scheme can be developed into something more concrete and that it proves useful, not merely for thinking about the causes of some development problems, but also for identifying and then tackling them in practice. If this works, I will be delighted to have concluded a largely nihilistic tour of contradictions, conflicts, and confusion in the literature on a positive note.

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