
Evaluating Recipes for Development Success

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This article offers a provocative critique of the ability of research on the impact of institutions on growth to offer immediate and practical recommendations for reforming and redesigning institutions in developing countries and transition economies. The literature traces the sources of growth to unalterable historical and geographic features. It contains equally plausible recommendations for opposite courses of action. It is sometimes driven by fads or recommends imitation of the latest success story. Some recommendations are too vague or too general to constitute practical advice. The article suggests a Bayesian diagnostic procedure to identify the causes of economic failure in an individual country as a first step toward remedying the failure. JEL codes: O43, O17, O20, P30, P48.

The main purpose of the 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 endpoint 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. Listeners usually find my responses meager and unsatisfactory, and I am compelled to agree. In this article, I apply the same, admittedly stringent, test of immediate policy relevance to all academic researches on institutions and development. I interpret the 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 provocative and critical, but try to be evenhandedly so. I hope to give everyone some incentives 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 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 to the broad question of the emergence and reform of deep institutions such as democracy, insofar as that influences the shape and function of the institutions of economic governance and in turn affects economic performance. It also relates to some specific organizations that implement the rules of the game established by the institutional setting and even to some specific policies that 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, with the focus blurring toward each 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 available. However, as the policy recipes that emerge from this literature are almost invariably unsatisfactory, being omitted from the list and the implied criticism, however friendly and constructive, may perhaps be regarded with relief.

Critical assessments of the contribution of academic research to development policy have a long tradition. Most recently, Easterly (2001) has produced a comprehensive, compelling, and entertaining critique of theories of economic growth. He concludes that the prescriptions derived from these theories—policies to increase saving, investment, foreign aid, and education; to modernize technology; and to reduce population growth and debt—failed mostly because other policies of the developing country governments often maintained or created perverse incentives. He concludes that the path to success must begin by setting up “quality institutions” (p. 252), but he says little about how to do this. 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 await 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:

- Some of the works find that development success is contingent on some historical or geographic preconditions that most countries do not meet.

- On many crucial issues, from deep institutional divides of democracy or authoritarianism, to general policy approaches such as comprehensive change or gradualism and sequencing of reforms, to specific policies toward population, inflation, regulation, and so on, the literature includes support for diametrically opposed prescriptions, each seeming quite plausible when presented on its own. Thus, unlike the theories of economic growth, which Easterly criticizes for serially offering definitive answers that did not work in practice, theories of institutions simultaneously offer different and conflicting answers.
- The recipes for success are often motivated by ideological fashions or a herding instinct that follows the latest perceived success.
- The recipes that seem most promising are often at a level of generality that stops far short of practicability. Examples are advice to build institutions to 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 the research 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 suggest a framework or methodology of research that combines general conceptual and empirical findings from academic research and the experience of practitioners to help narrow or identify the causes of failures in individual countries. I hope that it can serve as the first step toward remedying the failures or removing the obstacles.

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 interpretations. To make them amusing and memorable, I call these findings “Irish” recipes for success, after the story of the Irishman who, when asked for directions, replied “If I wanted to go there, I wouldn’t start here.”

Many geological and geographic characteristics have the appearance of determinism. The fixed or very long-run processes described by Diamond (1997) are

the most extreme examples of such determinants of economic outcomes: an east–west rather than a north–south alignment, the right kinds of native animals and plants, 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 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 this is debatable. A considerable body of literature argues that resource abundance worsens your prospects. Sachs and Warner (2001) summarize and extend these findings; see also Engerman and Sokoloff (1997).¹ The problem is not confined to mineral resources; if your land is suitable for plantation agriculture, that can also be 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 that a large fraction of your population became fluent in English (Hall and Jones 1999) and inherited British rather than French or other European legal and administrative institutions, giving better protection to the rights of small shareholders and therefore allowing broader and more efficient capital markets to develop (La Porta and others 1998, 1999).² If local diseases were more likely to take a heavy toll on 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 developing 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. At best, they might 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 through 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 (2004a, p. 4), who adds cautiously: “Finding an appropriate econometric instrument is not the same as finding an adequate explanation.” The supposed proximate determinants of success are categorized as institutions (Engerman and Sokoloff 1997, 2002; Acemoglu, Johnson, and Robinson 2001, 2002) or social infrastructure (Hall and Jones 1999). Here are some examples of the suggested channels.

If a colony had rich mineral resources or climate 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 decision making, greater access to education and economic opportunities, and protection of everyone’s property rights are needed for success. 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 contracted local diseases and experienced high mortality rates, 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 supportive institutions conducive to long-run economic success. (See Acemoglu, Johnson, and Robinson 2001; see also the critique by Albouy 2004 and the reply by Acemoglu, Johnson, and Robinson 2005.) More broadly, Hall and Jones (1999, p. 100) 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 anti-diversion 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 (2004a) 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 , x_i be a vector of measures of its institutions, and z_i a vector of other relevant variables. The hypothesis being tested is

$$y_i = a_1 + a_2x_i + a_3z_i + e_i. \quad (1)$$

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

$$x_i = b_1 + b_2y_i + b_3z_i + b_4g_i + u_i. \quad (2)$$

Equation (1) can then be estimated by two-stage least squares, estimating the x_i from the first-stage equation

$$x_i = c_1 + c_2z_i + c_3g_i + v_i \quad (3)$$

and using the estimated values in the regression for equation (1). This requires that the instrumental variables g_i do not appear directly in equation (1) and are uncorrelated with the error term e_i in that equation. Various stories can then be constructed for why that should be so. For example, Hall and Jones (1999, p. 101) comment 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 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 sound plausible. But how does one formally test them? In other words, how does one test the difference between the system of equations (1) and (2) and an equation like this:

$$y_i = d_1 + d_2z_i + d_3g_i + w_i \quad (4)$$

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

If there is just one instrument, for example, colonial mortality in the primary specification of Acemoglu, Johnson, and Robinson (2001), then the system of equations (1) and (2) is exactly identified and econometrically indistinguishable from equations (3) and (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). It involves some other untested assumption. If there is more than one instrument (g_i is a vector), one can perform over-identification tests, but these are weak and too likely to accept the null hypothesis that the g_i terms are excluded from equation (1). In Acemoglu, Johnson, and Robinson (2002), the geographic reversal of fortune instrument is tested in such a way, assuming that mortality among colonists is a valid instrument. But that is not problem-free, as was just argued.

My view is that the notion 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 more compelling. More and sharper research is badly needed.

But what policy implications follow if we proceed as though we were convinced by these stories and accept that institutions are important? The literature reviewed above gives us no guidance. Whether geography or history have a direct effect or an effect through institutions, the recommendation to change one's geography or history is useless. We have to forget about history and geography and try to affect the relevant institutions directly. For advice on that, we must turn to some other research.

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, Rodrik (2000) titles his paper "Institutions for High-Quality Growth: What They Are and How to Acquire Them." This literature presents some convincing evidence and arguments that institutions that offer credible commitments to protect property rights and to enforce contracts are good. But there is much disagreement about which institutions are better for this purpose. For every paper that endorses one kind of institution or policy, there is another that claims the opposite. Each is written by a prominent economist and contains impressive arguments and evidence to support its recommendation.

The following sections examine several dimensions of the contradictory conclusions of the literature on institutions and policies. They range from broad choices about the deepest institutions that govern society and polity, to minutiae on 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.

Democracy or Authoritarianism?

Many cross-country regressions examine whether the democratic or authoritarian governments are better for growth and find mixed results. For example, Barro (1999, p. 61), who finds a relatively poor fit and an inverse U-shaped relationship, suggests that “more democracy raises growth when political freedoms are weak, but depresses growth when a moderate amount of freedom is already established.” Persson (2005, p. 22), using cross-sectional as well as panel data, finds that the crude distinction between democratic and nondemocratic 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 (2004b, p. 256), 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 can explain the claimed superiority of democracy? Rodrik (2000) emphasizes the importance of local knowledge for successful institution building and argues that participatory democracy is a 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. Democracy succeeds by facilitating voice and participation. Bardhan (2005) 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 the costs of disruption in a transition to democracy. For them, Rodrik and Wacziarg (2005) bring good news. Using annual panel data for more than 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 percent a year for Sub-Saharan Africa.³ Growth also becomes less volatile after a transition to democracy. Declines in growth precede, not follow, such transitions.

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: 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 there is an equally impressive emerging literature that makes a serious case for authoritarian governments and institutions for starting growth and development. Glaeser and others (2004) argue that the developing 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 (2005) 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 could 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 policy feature or features are important for good economic outcomes, regardless of the kind of government that makes those policies? There is broad agreement in much of the literature that the credibility of commitments is vital. Acemoglu (2003) argues that the lack of third-party enforcement in political contracts makes it harder to make credible commitments, and that this explains the absence of a Coase theorem ensuring efficient outcomes in political bargaining. In a comprehensive overview of the dimensions of governance and their effects on economic development, Keefer (2004a, 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 an illuminating analytical narrative of Argentine politics, Spiller and Tommasi (2003, p. 281) argue that the institutions and organizations of political

bargaining in Argentina, and the volatility of its economic environment, combined to inhibit the capacity of its policymakers to “undertake efficient inter-temporal 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 (2004b) 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 from authoritarian rulers should be more credible, if anything, so long as they are stable in their rule. And such rulers do have incentives to pursue policies that enhance economic success. Even if their aims are predatory, as long as their positions are sufficiently stable, they will achieve the largest increase in their own take if the pie is larger—this is Olson’s (1993) “stationary bandit.” If they fear being overthrown, improving the people’s economic well-being may be the best way of postponing that fate.

China is usually cited as an authoritarian regime in which property rights are not formally protected but are *de facto* quite secure to the point that the country attracts large investments from abroad. The literature offers various explanations. Qian (2003) and Rodrik (2004b) attribute this to the Chinese institutional innovation of township and village enterprises, which “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” (Rodrik 2004b, p. 11). This puts the local authorities in a role similar to Olson’s stationary bandit. However, the strategy sounds 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 the 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, China appears to retain several aspects of democracy at lower and middle levels of institutions and economic policy-making: 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.⁴ 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 up front 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 the credible protection of property rights is essential for investment and innovation and is therefore a necessary condition for development success. But this literature provides no guidance on the operational question of what can be done if despots cancel these rights on a whim or democratic governments do so at the behest of politically important special interests.

Formal or 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 organized crime. Many variants and mixtures of formal and informal systems exist in most countries. In some countries, systems of private adjudication are supported by formal enforcement; for example, industry-based arbitration panels can be recognized by courts, which enforce the judgments issued by arbitrators. Rubin (1994) recommends this approach for building governance institutions in transition economies. In some countries, private enforcement of formal laws, ranging from private initiation of litigation to private infliction of punishment, is also practiced. Berglöf and Claessens (2004) and Hay and Shleifer (1998) favor forms of this approach.

What are the relative merits or flaws of formal and informal institutions of economic governance? The literature contains an often bewildering and mutually contradictory spectrum of arguments and evidence.⁵

A useful conceptual framework for studying formal–informal variants is the distinction between relation-based and rule-based governance of contracts. In relation-based governance, transactions occur between parties who are both members of the same group or network based on such ties as language, ethnicity, and locality. Information circulates within the network and helps each member in searching for a partner with which to transact and in obtaining information about the capability and trustworthiness of prospective partners. Contracts are personal and implicit agreements. Any breaches of promise are communicated to

the group, and other members of the group can punish offenders by refusing to trade with them or by imposing extra-economic sanctions such as social ostracism or worse. Rule-based governance 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, regulations, or courts; one just starts dealing with one's friends and neighbors. But its marginal costs are substantial and increase as the scope of trade expands. One deals initially with close neighbors, whose trustworthiness is better known and who are more likely to behave well than are more distant acquaintances or strangers because future links among close neighbors are likely to be larger and more frequent. But as one's business expands, it becomes necessary to deal with those more distant acquaintances or even strangers, who are less likely to be trustworthy. In 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's (2004) formal model examines and compares equilibria under the two systems and 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 more distant locations. The Genoese traders used a more formal system of registering contracts with the authorities in Genoa, who investigated and adjudicated any disputes. This system proved better as trade expanded. More recently, microcredit has succeeded in many small communities and groups, but expanding its scope to larger financial markets for lending, borrowing, and investing has proved 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).

The 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 in these informal institutions. This is a group of "insiders," which by its nature requires a concomitant definition of the "outsiders" who are to be excluded. Therefore, informal

institutions usually erect entry barriers that inhibit fuller exploitation of opportunities and development of capital markets. Fauchamps (2004, pp. 455–56) 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 and relation-based systems seem to be less successful in protecting property rights than in enforcing contracts. Li (2003, p. 657) confines most of his discussion to contracts and tacitly assumes a background of formal institutions that include property right protection: he says that relation-based governance “requires only minimum public order—that is, the general absence of rampant robberies or confiscation” to get going. And Gambetta’s (1993, p. 198) 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.” Even with contracts, information services about trustworthiness do not generate problems of violence, whereas enforcement services to punish breaches do (Gambetta 1993; see also Dixit (2004)).

De Soto (2000, pp. 5–6) emphasizes the importance of formal recognition of property rights:

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 on 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 the purely informal institutions of governance eventually act as a constraint on growth and expansion of trade. No country switches entirely 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.” Bernstein (1992, 2001) offers some examples of such industry-based adjudication in the United States. But it remains important for developing 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 yields mixed results. Some formal institutions can meld well with the existing informal ones. Rubin (1994) and Dixit (2004) argue that arbitration does, although Widner (2000) has challenged the efficacy of specialized courts.

In other instances, the two types of institutions clash, and the formal one may lose. Ensminger (1997) gives an example of land title reform in Kenya. Traditional land rights in many parts of Africa are a complex system. Clan chiefs grant titles to individual families. Sales are subject to their approval and also to 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 more closely resemble traditional forms of ownership. Finally, the 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 lessening the harmful consequences of breaking a relational contract, can worsen the outcomes of the informal system (Baker, Gibbons, and Murphy 1994; Dixit 2004).

Linked to the conflicting arguments and evidence about formal and informal institutions is the related question of whether to use the readily available models of Western legislative and judicial institutions or to develop ones specifically tailored to each country. Many of the pertinent issues have already been touched on here or will be in the following section, so they are not discussed again except to mention their bearing on this question.

Comprehensive and Rapid or Sequential and Gradual Reforms?

The rapid or gradual reform debate is long-standing, and it continues unabated. There are four main arguments for speed. First, opportunities for reform present

themselves rarely in political negotiation, so they should be seized when they do arise. Second, reforms cause some pain to some people or groups, and gradualism prolongs the pain, risking the whole reform. Third, 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. Fourth, the dimensions of reform are strategic complements, and results will be poor unless all dimensions are tackled jointly (see Kremer 1993a for an "O-ring" theory of this general class of phenomena).

Others favor a more cautious approach, presenting two main arguments. First, drastic reforms, by inflicting severe pain, are likely to arouse strong opposition and thereby create a greater risk of political opposition, leading to failure. Second, 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 *World Development Report 1996: From Plan to Market*, 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 a country's initial conditions are more important than policy changes in determining its economic performance during the first few years of transition; that is, whether the reforms are rapid or gradual is less important. They also find a negative feedback in reform: higher levels of initial liberalization are likely to slow the 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 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," meaning that investors are clear about whom to bribe and how much to pay them 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) 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 gradually to a more complex and better one. But if the institutions are complementary, they need to be implemented 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 without a way, public or private, to enforce its decisions. Therefore, for gradualism to succeed, a policy practitioner must know which policies or institutions are substitutes and which are complements. This is a difficult task, and the literature offers only a few, incomplete hints.

Crises—Good or Bad?

Also linked to the speed and comprehensiveness of reforms is the possibility of beneficial consequences of crises. Olson (1982) presents this thesis in the clearest form. He argues that 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) makes some related observations. And Calvo (2005) points out that many recent crises have been followed by a sharp recovery in which such usual factors as investment, domestic bank credit, and the current account deficit were only small contributors—a miraculous Phoenix-like rise from ashes.

It is hard to imagine a country deliberately risking a grave crisis in order to start the Olsonian process. Moreover, other researchers argue that crises are not necessarily beneficial. The theoretical model of Hsieh (2000) clarifies the issues. The dynamic negotiation on policy reforms following a crisis is a war of attrition or a game of “chicken,” the question being 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. Diamond (2004) presents some dramatic practical examples of crises leading to total collapse.

Population—Resource or Curse?

Many specific issues have received similar treatment in the literature as institutions and their reform, with conflicting results and prescriptions. 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 neo-classical growth model, a larger rate of population growth leads to a steady state with lower output per capita. When natural resource depletion is also considered, the consequences can be more dire. Diamond (2004) is only one of the latest of a long line of prominent scientists who have sounded alarms on this issue.

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

Whom to Believe, and What to Do

I have sketched pairs of starkly conflicting research findings on several important issues of institutions and policy. This can leave anyone who is not an expert in a particular area in a state of confusion and indecision. That might not be a bad outcome. 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. Rarely are the arguments and evidence from the other side clearly wrong or unambiguously inferior. The decisions being made have immense consequences, and they are difficult to reverse. Therefore, any uncertainty weighs heavily in the decision. The favored choice must not merely be better than the other, but better by a sufficiently large margin to justify going ahead. Otherwise, waiting for better information is optimal (Dixit 1992; Dixit and Pindyck 1994).

In some circumstances, there is another problem when it comes to action. Even if you are convinced by one side in the argument and want to follow through into institutional reform or policy action, there may not be much you or almost anyone else 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 easier to implement your favored institutional reform!)

Shifts of Consensus

In the previous section, I laid out several stark disagreements among researchers and showed how difficult it is to sort out reliable and feasible policy recommendations from this chaos. This “cross-sectional 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 brief outline of some such changes and the warning they should convey.

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, and 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 Soviet Union 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. 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?

Role of International Organizations

For the last 50 years or longer the World Bank, the International Monetary Fund, and other international organizations have given advice, finance, and aid to developing countries and transition economies. But perceptions are shifting about 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 different policies for each country based on local knowledge to fit its conditions was argued to be vital, and countries’ “ownership” of reforms became the buzzword.

The idea that international institutions can act as facilitators or catalysts for institutional change or policy reform (McCarthy, Bader, and Pleskovic 2003) seems attractive; it can combine the best features of discipline and ownership. 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

producing economic success. But we have little hard evidence on whether local ownership translates into better—more credible, less corrupt—institutions. All of the issues mentioned above in the context of authoritarianism or democracy can arise equally at the local level.

The Napoleon Prescription

Faced with all these contradictions and shifts,⁶ I can identify only one consistently valid policy prescription. It is the quality Napoleon valued most in his generals—luck. Researchers want to identify causes and practitioners want to know what they can choose and change. Both sides may have neglected the important role that luck has played in many countries' development successes or failures. Easterly (2001) 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 rising gas prices had shifted US demand toward small cars and the US 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 United States 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.

Division of Labor and Diagnostics

Why do we find so many contradictions in the research on the determinants of economic growth and development and so many corresponding conflicts in the recipes for development success? Some explanations lie in the diversity of theories and perspectives that economists and academics from other fields bring to this inquiry: development economics is undergoing a paradigm shift, from theories that view resource and technology constraints as key obstacles to growth, to theories that view information asymmetries as key constraints to the operation of markets, and now to theories that view institutions as keys to success or failure. In the process of a paradigm shift, no one's point of view will dominate, and major differences will persist for a while. 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 lies in the diversity of data sources and periods over which econometric studies have been carried out. But much of the explanation surely lies 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 the econometric and theoretical studies are not the best way to generate policy prescriptions. Most cross-country regressions are a far from perfect fit; 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 that one consider all the different causes or mechanisms at work in a country and how they interact. 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 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.⁷

How can practitioners combine their country knowledge with academic research? Must this be a totally ad hoc matter, or can one develop something of a system? I would like to suggest a framework building on the idea of “growth diagnostics” introduced by Hausmann, Rodrik, and Velasco (2005). Their basic idea is to identify the crucial or binding constraints in any situation and then to focus attention on remedying those. They develop a theoretical framework for this and operationalize it using a “tree” much like the branching decision diagrams in the trouble-shooting sections of product manuals. (Does the computer beep four times when you switch it on? If yes, skip to step 10; if no, . . .) Hausmann, Rodrik, and Velasco (2005, p. 2) ask practitioners to look at other symptoms that may be associated with low growth and use a succession of questions about symptoms to pin down a cause:

Is it inadequate returns to investment, inadequate private appropriability of the returns, or inadequate access to finance? If it is a case of low

returns, is that due to insufficient investment in complementary factors of production (such as human capital or infrastructure)? Or is it due to poor access to imported technologies? If it is a case of poor appropriability, is it due to high taxation, poor property rights and contract enforcement, labor-capital conflicts, or learning and coordination externalities? If it is a case of poor finance, are the problems with domestic financial markets or external ones?

This approach has much to recommend it. It looks at multiple dimensions of economic outcomes and tries to narrow down causes from a set of multiple possibilities. 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 a 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 be a grouping or composite of several causes. And the list can be quite wide and comprehensive, including political and institutional obstacles as well as specific policy errors of commission or omission. The second column has the prior probabilities π_1, π_2, \dots with which the various causes can occur. Historical analyses can provide estimates of these probabilities; specific knowledge of a country can help refine these estimates.⁸ The rest of the columns have as their headings various conceivable outcomes E_1, E_2, \dots ; again, each outcome could be a cluster or composite of several outcomes. The entries in the cells are then the conditional probabilities, with P_{ij} being the probability that outcome E_j will occur when cause C_i is present. In principle, econometric research can yield such probabilities, although this is not how regression results are usually presented.

Table 1. Simple Bayesian Diagnostic Procedure for Identifying Causes of Economic Failure in an Individual Country Based on Effects and Prior Probabilities

Causes	Prior probabilities	Effects			
		E_1	E_2	...	E_n
C_1	π_1	P_{11}	P_{12}	...	P_{1n}
C_2	π_2	P_{21}	P_{22}	...	P_{2n}
...
C_m	π_m	P_{m1}	P_{m2}	...	P_{mn}

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 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,” such that

- 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 one conditional on observing E_7 .
- It is very likely to occur when C_5 is present, that is, $P_{5,7}$ is close to one, so the rest of the $P_{5,j}$'s are close to zero, and if some other effect is observed, the posterior probability of C_5 becomes close to zero.

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 subsumed into C_2 , 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 one for C_1 if E_1 is observed and also one 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 provides an ideal to work toward.

I hope this very rough conceptual scheme can be developed into something more concrete and it proves useful, not only 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.

Notes

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1. However, others have rebutted this; for example, Mehlum, Moene, and Torvik (2006, p. 4) find that “the resource curse applies in countries with grabber friendly institutions but not in countries with producer friendly institutions.”

2. However, Lamoreaux and Rosenthal (2005) argue that the institutional differences between France and the USA did not prevail during the nineteenth century, the crucial period for industrialization, but instead evolved slowly as a result of legislation during the 20th century.

3. 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. This suggests an alternative hypothesis. Suppose that purely exogenous random shocks are the main determinants of growth but that the public gives the rulers credit for good results and blames them 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 abysmally whereas democratic governments may fall for smaller shortcomings.

4. One other feature of economic reforms in China is often highlighted and praised (for example, Qian 2003; Rodrik 2004b): 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 of democracy or authoritarianism.

5. 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 to their relative merits. Islam (2004, p. 1) 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 harder to specify which choice is best under what circumstances.

6. 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 Duchy of Grand Fenwick, declares war on the United States, with the idea that when Grand Fenwick loses the war, the United States will give it large amounts of aid. Unfortunately for the plan, Grand Fenwick wins.

7. Even such detailed and well thought-out combinations 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 base to improve financial intermediation. Just a few years later, Zimbabwe’s problems look quite different.

8. 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.

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