

GETTING INSTITUTIONS RIGHT

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There is now widespread agreement among economists studying economic growth that institutional quality holds the key to prevailing patterns of prosperity around the world. Rich countries are those where investors feel secure about their property rights, the rule of law prevails, private incentives are aligned with social objectives, monetary and fiscal policies are grounded in solid macroeconomic institutions, idiosyncratic risks are appropriately mediated through social insurance, and citizens have recourse to civil liberties and political representation. Poor countries are those where these arrangements are absent or ill-formed. Of course, high-quality institutions are perhaps as much a result of economic prosperity as they are their cause. But however important the reverse arrow of causality may be, a growing body of empirical research has shown that institutions exert a very strong determining effect on aggregate incomes.¹ Institutions are *causal* in the sense that a poor country that is able to revise the rules of the game in the direction of strengthening the property rights of entrepreneurs and investors is likely to experience a lasting increase in its productive capacity.

Much less well understood are the implications of this line of reasoning. Indeed, the empirical finding that “institutions rule” has sometimes been interpreted as a form of property-rights reductionism—one that views the formal institutions of property rights protection as the end-all of development policy. In the academic literature, this has led to a tendency to oversimplify the issues at stake—for example by treating institutional development in a mono-causal manner (i.e., linking it exclusively to colonial history) or by identifying “institutions”

¹ See in particular Hall and Jones (1999), Acemoglu, Johnson, and Robinson (2001), Easterly and Levine (2003), Rodrik, Subramanian, and Trebbi (forthcoming).

solely with the formal, legislated rules in existence. In the policy field, the new focus on institutions has led to an overly ambitious agenda of “governance” reforms aimed at reducing corruption, improving the regulatory apparatus, rendering monetary and fiscal institutions independent, strengthening corporate governance, enhancing the functioning of the judiciary, and so on. Sometimes called “second-generation reforms,” these new reforms are meant to overcome the apparent inefficacy of the earlier wave of reforms relying heavily on liberalization, stabilization, and privatization. Simple policy changes are ineffective, the argument now goes, unless they are grounded strongly in institutional reforms.

In this article, I elaborate on these and some other issues. My own perspective is that the empirical literature on institutions and growth has pointed us in the right direction, but that much more needs to be done before it can be operationalized in any meaningful way. Many of the policy implications drawn from this literature are at best irrelevant and at worst misleading.

An instrument does not a theory make.

The empirical work on what one may call “macro-institutions” received a big boost with Acemoglu, Johnson, and Robinson’s (2001; AJR) important paper called “The Colonial Origins of Comparative Development.” This mis-titled (see below) paper came up with an ingenious solution to a dilemma that had long stymied serious empirical work in this arena.

The difficulty with the empirical analysis of institutional development has been that institutional quality is as endogenous to income levels as anything can possibly be. Our ability to disentangle the web of causality between prosperity and institutions is seriously limited. AJR proposed using colonial history to achieve econometric identification. Parts of the world that confronted would-be colonizers with greater health hazards, they argued, were less likely to be

permanently settled by Europeans, who as a consequence were less likely to build institutions protecting property rights, limiting their efforts to pure extraction. Settler mortality rates three centuries ago could help identify which countries acquired good institutions and which did not, and plausibly help account which countries grew rich and which remained poor. This encounter of ecology with history could be used to test for the causal impact of institutional quality on levels of development. Moreover, the fact that natives had immunity to the diseases to which settlers succumbed helped support the point that settler mortality (the instrumental variable) was not necessarily a stand-in for the local health environment. Using this strategy, AJR were able to show that a substantial part of the variation in today's income levels among former colonies can be explained by differences in investors' perceptions with regard to the likelihood of expropriation.

What AJR seemed to suggest with their title was that they had identified differing encounters with colonialism as the root of the variance in income levels around the world. But this is a problematic interpretation. The variation in average income levels among countries that have never been colonized is almost as large as that in the colonized sample (Rodrik, Subramanian, and Trebbi, forthcoming; RST). If the roots of underdevelopment lie in contrasting encounters with colonizers, how can we explain the fact that countries that have never been colonized by Europeans are among both the poorest and richest of today's economies? Consider for example countries such as Ethiopia and Afghanistan at one end of the spectrum and Japan at the other end, with middle-income countries such as Turkey and Thailand lying somewhere in between.

The correct interpretation of AJR, in my view, is that colonial experience—as captured by the settler mortality variable—simply provides a convenient “instrumental variable,” without

in itself holding much explanatory power for patterns of global inequality. Finding an appropriate econometric instrument is not the same as providing an adequate explanation—a distinction that is somewhat blurred in AJR. One should therefore not read too much into AJR with regard to the role played by colonialism in shaping today's contours of wealth and poverty.

Geography-based instrumental variables do not imply geography-based explanations.

Settler mortality was obviously a function of ecological conditions, and this raises the question of whether AJR unwittingly gave a starring role to geography. Indeed, since few things other than geography are exogenous in economics, most instruments for institutional quality are likely to have a significant geographical component (resource endowments, latitude, and so on). Indeed, there is a long and distinguished list of scholars who have pointed to the importance of geography. Jeffrey Sachs has forcefully argued that geography exerts a strong independent effect through its impact on the public health environment and on transport costs (Sachs 2003; Gallup, Sachs, and Mellinger 1998). Jared Diamond (1997) has shown how apparently innocuous accidents of geography (such as the alignment of continents) can have long-lasting effects on patterns of technological development and diffusion. So can these studies still effectively parse out the respective roles of geography and institutions in determining income levels?

The answer is yes. To see how this can be possible, consider a different, but analogous, exercise. Suppose we were interested in explaining differences in income levels among German *länder* lying on both sides of the Berlin wall prior to 1989. Suppose also that our hypothesis was that these differences were due primarily to differences in the degree of protection of private property rights. Cognizant of reverse causality, we might want to look for an instrumental

variable—something that is correlated with institutions, but is not a determinant of income levels through another channel. Longitude provides such a variable, because *länder* in the eastern part of the country were much more likely to fall under Soviet occupation and acquire communist institutions. We might then use longitude as an instrument for the system of property rights and rightly conclude that the protection of private property rights is a superior means for generating wealth. The correct inference here would not be that geography (longitude) is the cause of income differences: geography, in interaction with history, simply provides a convenient source of exogenous variation to identify the role played by *institutions*.

Of course, geography variables have to be given a fair chance to compete against institutions as ultimate explanators of income differences. In RST, we tried a large number of geography variables and found their direct impact on income to be either insignificant or non-robust. That led us to conclude that “institutions rule.” Similar results were also reported in AJR and Easterly and Levine (2003). However, there are other studies that find a role for geographical determinants such as malaria ecology (Sachs 2003) or climate, latitude and East-West orientation (Hibbs and Olsson 2004) even after controlling for institutional quality. It would be fair to say that scholarly opinion remains divided on the significance of geography as a direct determinant of income levels.

But the centrality of institutions does not preclude an important indirect role for geography.

At the same time, there is wider agreement on the *indirect* role played by geography. In particular, when one endogenizes institutional formation, one often finds geographical determinants to be an important part of the story. For example, Engerman and Sokoloff (1994) have linked the contrasting patterns of institutional development in North and South America to

the differences in resource endowments: large-scale plantation agriculture is much more conducive, compared to smallholding, to the emergence of inequality and of autocratic institutions that repress non-elites. Sala-i-Martin and Subramanian (2003) have provided systematic evidence that shows abundance of natural resources and rents to be damaging to the quality of institutions. RST find that distance from equator is a significant (positive) contributor to institutional quality. This line of work suggests that even if geography and endowments do not exert an important independent impact on incomes, contra Sachs (2003), they may have a significant indirect impact *through* institutions.

The challenge for the empirical literature on institutions is to explore these patterns without falling into the trap of reductionism or of historical and geographical determinism. For as we shall see, the process through which countries acquire “good” institutions is typically quite idiosyncratic and context-specific. Luck plays an important role, as does human agency.

Institutional quality, as it is typically measured, remains a nebulous concept.

The manner in which institutional quality is measured in the empirical literature that is discussed above leaves a lot of questions unanswered. The most commonly-used indices of institutional quality are based on surveys of foreign and domestic investors, in which the respondents in a particular country are asked whether they consider their investments safe or how they rate the “rule of law” (see for example Kaufmann et al., 2002). So these indices capture investors’ perceptions, rather than any of the formal aspects of the institutional setting. They measure how well the rules of the game with regard to property rights are perceived to operate, and not what those rules are. This in turn raises two difficulties, one more serious than the other.

The first difficulty is that these perceptions are likely to be shaped not just by the actual operation of the institutional environment, but also by many other aspects of the economic environment. Most importantly, investors are likely to rate institutional quality high when the economy is doing well, regardless of whether causality goes one way or another. But this is just another instance of endogeneity and reverse causation. If the researcher has a valid instrumentation strategy, it ought to take care of this problem too. So the fact that our measure of institutional quality is perception-based does not invalidate inferences drawn from its use (subject to the caveat below) as long as proper care is taken in econometric identification.

The more serious issue is that, even if causality is properly established, the results do not tell us what specific rules, legislation, or institutional design is actually responsible for the institutional outcome being measured. All that we can infer is that performance is superior when investors feel their property rights are protected (or the rule of law is upheld). The results are silent on what it is that makes investors feel that way.

To appreciate the significance of this, compare Russia and China. In Russia, an investor has in principle the full protection of a private property-rights regime enforced by an independent judiciary. In China, there is no such protection, since private property has not been (until very recently) legally recognized and the court system is certainly not independent. Yet during the mid- to late-1990s, investors consistently gave China higher marks on the rule of law than they did Russia. They evidently felt better protected in China than they did in Russia. This is perhaps no big surprise to anyone who has observed the evolution of the Russian legal system over the last decade. But the important point from the current perspective is the apparent disconnect between the perception of the rules and the actual rules.

Consequently, the empirical finding that effective property rights are critical yields very little operational guidance as to how they are established. As the Russia-China comparison nicely illustrates, it does not even imply that a legal system based on *private* property rights dominates one where property rights are held collectively! What matters is that investors feel safe, regardless of how that safety is achieved. The empirical literature does not tell us how that safety is attained, only that it matters a lot.

Institutional functions do not map into unique institutional forms

So how is it that Chinese investors could feel more secure than Russian investors despite the absence of formal private property rights legislation in China? We do not know, but here is a plausible story.

To be effective, a formal legal regime protecting investors' rights requires a non-corrupt, independent judiciary with enforcement power. Let us posit, without doing great injustice to reality, that setting up such a judiciary is hard at low levels of income and takes time. So enhancing property rights by simply rewriting domestic legislation—changing the formal aspects of the institutional environment—is naturally of uncertain efficacy. That seems to have been the trap in which the Russian transition was caught up for some time.

How did China evade this trap? The largest boom in “private” investment in China took place (at least until the mid-1990s) in Township and Village Enterprises (TVEs). These were firms in which ownership was typically held by local governments. Private entrepreneurs were effectively partners with the government. In a system where courts cannot be relied upon to protect property rights, letting the government hold residual rights in the enterprise may have been a second-best mechanism for avoiding expropriation. In such circumstances, the

expectation of future profits can exert a stronger discipline on the public authority than fear of legal sanction. Private entrepreneurs felt secure not because the government was prevented from expropriating them, but because, sharing in the profits, it had no interest to expropriate them.

This is a specific illustration of a broader point, namely that there is no unique, non-context specific way of achieving desirable institutional outcomes. China was able to provide a semblance of effective property rights *despite* the absence of private property rights. The Russian experience strongly suggests that the obvious alternative of legal reform would not have been nearly as effective. We can multiply the examples. For instance, China provided market incentives through two-track reform rather than across-the-board liberalization, which would have been the standard advice. Hence, in agriculture and industry, price efficiency was achieved not by abolishing quotas and planned allocations, but by allowing producers to trade at market prices *at the margin*. In international trade, openness was achieved not by reducing import protection, but by creating special economic zones with different rules than those that applied for domestic production.

The important point is that effective institutional outcomes do not map into unique institutional designs. And since there is no unique mapping from function to form, it is futile to look for uncontingent empirical regularities that link specific legal rules to economic outcomes. What works will depend on local constraints and opportunities. The best that we can do as analysts is to come up with contingent correlations—institutional prescriptions that are contingent on the prevailing characteristics of the local economy. At the moment we are very far from being able to do this for any but a few institutional areas.²

² One example where a fair amount of work has been done relates to the choice of an exchange-rate regime. The optimum currency area literature can be interpreted as the search for prescriptions that are sensitive to the structural characteristics of an economy.

In the short-run, large-scale institutional reform is rarely necessary to accelerate growth.

The bad news, as the foregoing discussion indicates, is that the literature on the institutional determinants of economic prosperity has yet to yield solid policy prescriptions. The good news is that everything that we know about economic growth suggests large-scale institutional transformation is hardly ever a prerequisite for getting growth going. It is true that sustained economic convergence eventually requires acquiring high-quality institutions. That is the whole point of the empirical literature I have discussed above. But the initial spurt in growth can be achieved with minimal changes in institutional arrangements. In other words, we need to distinguish between *stimulating* economic growth and *sustaining* it. Solid institutions are much more important for the latter than for the former (Rodrik 2003). Once growth is set into motion, it becomes easier to maintain a virtuous cycle with high growth and institutional transformation feeding on each other.

Ricardo Hausmann, Lant Pritchett and I recently examined growth accelerations in the period since about 1950 (Hausmann et al., 2004). We identified more than 80 such episodes, in which a country increased its growth rate by 2 percentage points or more for a period of at least seven years. The surprise was not only that there were, ³ so many cases of growth accelerations, but that the vast majority of them seemed unrelated to major economic reforms of the conventional type—i.e., economic liberalization and opening up. To the extent that we can identify triggers for growth, they seem to be related to the relaxation of specific constraints that were holding back private economic activity.

Even in the better known cases, institutional changes at the outset of growth accelerations have been typically modest. I have already mentioned some of the gradual, experimental steps

³ Our filter almost certainly understates the true number of growth accelerations. We excluded very small countries, countries with less than two decades of data, cases where the pickup of growth represented a recovery from a crisis, and instances where growth stood at below 3.5 percent per annum even after the acceleration.

towards liberalization that China undertook in the late 1970s without recourse to system-wide transformation. South Korea's experience in the early 1960s was similar. The military government led by Park Chung Hee that took power in 1961 moved in a trial-and-error fashion, experimenting at first with various public investment projects. The hallmark reforms associated with the Korean miracle, the devaluation of the currency and the rise in interest rates, came in 1964 and fell far short of full liberalization of currency and financial markets. As these instances illustrate, an attitudinal change on the part of the top political leadership towards a more market-oriented, private-sector-friendly policy framework often plays as large a role as the scope of policy reform itself. Such an attitudinal change appears to have had a particularly important effect in one of the important growth miracles of the last quarter century—India since the early 1980s (Rodrik and Subramanian 2004).

This is good news because it suggests countries do not need an extensive set of institutional reforms in order to start growing. Instigating growth is a lot easier in practice than the standard Washington recipe, with its long list of institutional and governance reforms, would lead us to believe. This should not be surprising from a growth theory standpoint. When a country is so far below its potential steady-state level of income, even moderate movements in the right direction can produce a big growth payoff. This is encouraging to policy makers, who are often overwhelmed and paralyzed by the apparent need to undertake ambitious reforms on a wide and ever-expanding front.

The trick is to be able to identify the binding constraint on economic growth at the relevant moment in time. In the South Korea of 1961, the major constraint probably was the large gap between the social and private return to investment. In the China of 1978, the constraint was obviously the absence of market-oriented incentives. In the India of 1980, it was

a government that was perceived to be too hostile to the private sector. In the Chile of 1983, it was an overvalued exchange rate. Of course, it is much easier to determine these things ex post than it is to do it ex ante. A major task for growth economists in the years ahead is to develop a framework of “growth diagnostics,” to enable the identification of areas with the biggest bang for the reform buck.

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