

Conceptualizing and Measuring Institutions: A View from Political Science

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The following paper identifies and takes issue with a potentially troubling development in the study of comparative politics. While political scientists have traditionally deployed *objective* indicators of “political organization and administrative capacity” (Reynolds 1983, p. 976), including tax ratios (Organski and Kugler 1980; Benson and Kugler 1998), tax structures (Kling 1968; Krasner 1985), political participation (Przeworski and Sprague 1971), and the nature and extent of public service provision (Migdal 1988; Putnam 1992), they are beginning to import *subjective* indicators of “governance” from economics (Sandholtz and Gray 2003; Fish 2005; Gerring and Thacker 2005; Blake and Martin 2006; Cameron et al. 2006). In fact, the customary distinction between objective and subjective measurement is by now less salient than the various differences *among* the subjective or “perceptions-based” alternatives (Kaufmann et al 2005a). Whose perceptions do they capture? Of which institutions or issue areas? And to what effect? We hold that the leading perceptions-based indicators are poorly explicated; give the perceptions and interests of businesspeople (and foreign businesspeople in particular) undue influence; are riddled with measurement error and all but impossible to interpret; and should therefore be either reconsidered or replaced by *improved* objective indicators that take public sector inputs as well as outputs into account. We make the case for reconsideration and/or replacement in three principal sections. Section 1 offers an abbreviated introduction to the perceptions-based literature. Section 2 develops a critique of the conceptual and operational underpinnings of the leading perceptions-based indicators. And Section 3 discusses objective alternatives.

1. Intellectual context.

Economists first brought perceptions-based indicators of corruption, property rights, and the rule of law to bear in cross-national growth research in the mid-1990s. For instance, Paulo Mauro obtained national-level indicators of judicial integrity, corruption, and bureaucratic quality from commercial risk rating agencies as early as 1995 (Mauro 1995). Arthur Goldsmith deployed an index of property rights developed by the Heritage Foundation more or less concurrently and turned to the Corruption Perceptions Index developed by Transparency International for the first time a few years later (Goldsmith 1995, 1999). And Alberto Ades and Rafael DiTella introduced the corruption indicators found in the EMF Foundation’s *World Competitiveness Report* at approximately the same time (Ades and DiTella 1997).

By the end of the decade, however, Daniel Kaufmann and his colleagues at the World Bank Institute had come to believe that “many of these indicators serve as imperfect proxies for one of a smaller number of more fundamental concepts of governance,” including “the rule of law, government effectiveness, and graft” (Kaufmann et al. 1999a, pp. 1-2), and had therefore decided to develop a series of “aggregate governance indicators” designed to capture the fundamental concepts in a more transparent and accurate fashion. They aggregated data from a wide array of sources including—but by no means limited to—the aforementioned foundations, nongovernmental organizations, and commercial risk rating agencies;

developed an unobserved components model; and distilled the aggregate data into six different indicators: voice and accountability, political instability and violence, government effectiveness, regulatory burden, rule of law, and control of corruption.

The so-called Worldwide Governance Indicators (WGI) have gained rave reviews (Gervasoni 2006) and market share in political science, sociology, law, public policy, and of course economics (see, e.g., Fish 2005; Rigobon and Rodrik 2005; Apodaca 2006; Borrmann et al. 2006; Lee 2007)—in no small part due to the creativity and entrepreneurship of the authors. And the WGI are by no means devoid of merit. They are available for more than 150 countries. They cover a ten year period. They include standard errors as well as individual country scores. And their authors issue frequent and welcome caveats as to their various limitations (see, e.g., Kaufmann et al. 2005b, p. 41).

In fact, the WGI arguably provide the best possible case for the perceptions-based measurement of governance. They are by all accounts among “the most carefully constructed and widely used” (Arndt and Oman 2006, p. 49) indicators available. And they are by now more than a decade old. If they fail to convince, we argue, then the entire project of perceptions-based measurement demands reconsideration—if not abandonment.

2. The limits to perceptions-based indicators in prospect: from explication to operationalization.

Do the WGI convince? We are neither the first nor the only scholars to entertain doubts (Arndt and Oman 2006; Thomas 2007; Knack 2007; Bhagwati 2007). Our doubts begin, however, with the explication of the conceptual underpinnings of the measures themselves (see Kurtz and Schrank 2007a, 2007b). Rudolf Carnap famously defined the process of explication as “the transformation of an inexact, prescientific concept, the *explicandum*, into a new exact concept, the *explicatum*” (Carnap 1962, p. 3), and offered a number of apposite examples including the self-conscious transformation of the *explicandum* of “warmth” into the *explicatum* of “temperature.” According to Carnap, the description of the *explicandum* is a necessary starting point in the process of measurement and comparison and is therefore no less central to scientific progress than the eventual interpretation and assessment of the *explicatum*. “Although the *explicandum* cannot be given in precise terms,” he argued, “it should be made as clear as possible by way of informal explanations and examples” (Carnap 1962, p. 3). Otherwise parties to a scientific discussion or dispute would inevitably wind up talking past one another.¹

Our discussion therefore departs from the *explicanda*. What are the prescientific concepts in question? While Kaufmann and his colleagues purport to be measuring institutions, and devote a good deal of time to their operationalization, they offer ad hoc and/or inconsistent definitions of their underlying concepts. Take, for example, their measure of the “rule of law.” They initially define the rule of law as “respect of citizens and the state for the rules which govern their interactions” (Kaufmann et al. 1999b, p. 2). They subsequently abandon their original definition for “the quality of contract enforcement, the police, and the courts, as well as the likelihood of crime and violence” (Kaufmann et al. 2005a, p. 4). And they eventually decide to deploy a more encompassing, if less discriminating, definition that includes the “extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, the police, and the courts, as well as the likelihood of crime and violence” (Kaufmann et al. 2006, p. 4). They offer neither Carnap’s “informal explanations and examples” nor for that matter a rationale for their definitional amendments—let alone a discussion of their implications. Are we to assume that their rule of law indicator (RL) assesses different concepts in different years? Or that

¹ Dumont and Wilson import Carnap’s approach to explication into the social sciences (1967). Przeworski and Sprague (1971, p. 217) embrace Hempel’s extension of Carnap’s approach. And Wesley Salmon underscores the continued relevance—if by no means immutability—of the broader realist program to which Carnap and Hempel contributed (1999).

the definitional changes have no effect on the indicator's interpretation? And, if the latter, why do they introduce the changes in the first place?

The implications are by no means trivial. Let's take a concrete example. Italy's rule of law score all but collapsed between 1996, when the Italians (.97) outperformed the Greeks (.90), Czechs (.84), Slovenes (.86), and Koreans (.71), and 2006, when their estimated governance score fell to .37 with a standard error of .14 and they suddenly found themselves relegated to an ignominious position beneath Tunisia (.38), Jordan (.45), Botswana (.63), and Uruguay (.40)—not to mention a number of monarchies, dictatorships, emirates, and tax havens (Kaufmann et al. 2007b, Table C5). In fact, Italy's estimated RL scores have declined from their previous values in seven out of eight releases of the WGI. Why? Have Italian politics and society really change so dramatically and so consistently over the course of a single decade? Or did Kaufmann and his colleagues de-emphasize the informal relationships and institutions that loom large in the Italian political economy (Putnam 1992) in favor of formal institutions like the police and courts—that are less operative in much of Italy—when they abandoned their original definition of the rule of law? Our point is neither to whitewash Italy's very real governance dilemmas nor to rule out other explanations for the fall. We simply wish to underscore the fact that conceptual shifts—especially those that are neither explained nor justified by their authors—come with real interpretive costs.

Our concerns are neither frivolous nor pedantic. After all, the rule of law's empirical referent is anything but obvious. Legal scholars like Thom Ringer have not only condemned the “conceptual anarchy among development theorists, experts, and donor agencies surrounding the very meaning of the expression” but have gone on to wonder whether the “problem with measuring the success of rule of law reform initiatives is that the parties assessing them may have something quite different in mind to those implementing them” (Ringer 2007, p. 182; see also Daniels and Trebilcock 2004). Nor are they alone. Political scientists and sociologists have expressed doubts about the concept as well (Stephenson 2000; Kurtz and Schrank 2007b). And Dani Rodrik has recently wondered whether he's “the only economist guilty of using the term abundantly without having a good fix on what it really means” or simply “the first one to confess to it” (Rodrik 2007).

Kaufmann and his colleagues appear to be sanguine by way of comparison. While they take comfort in the fact that the various aspects of governance they purport to measure “tend to be quite highly correlated across countries” (Kaufmann et al. 2007a, p. 555), they are in fact assuming what needs to be proven—that is, that their correlated indicators are valid measures of governance in the first place. Otherwise the reported correlations are all but devoid of meaning (see also Knack 2007).

In fact, Kaufmann and his colleagues are at least implicitly adopting the by now discredited “operationist” approach to measurement in which meanings are produced by measures and not vice versa. While they explicitly claim to derive the definitions of the six aspects of governance covered by the WGI from “existing definitions or understandings of the concepts” (Kaufmann et al. 2007c, p. 24), they occasionally admit that their indicators actually drive their definitions—and that the meaning is therefore in the measure. “That is,” they write, “we have just one implicit definition of corruption, which comes from the aggregation of these many data sources across many countries” (Kaufmann et al. 2007c, p. 7). The limits to operationism are by now well known. According to Henry Byerly the “strict application of the more extreme operationist doctrine would lead to as many notions of a quantity such as mass as there are different operational procedures for measuring mass” (Byerly 1972, p. 376.). Less strict applications are untenable, however, for efforts to justify—rather than simply invoke—particular operational definitions inevitably appeal to meanings that are independent of the measures themselves.²

² See Hempel (1956) for a seminal critique of operationism.

Are the WGI valid indicators of governance? We will address the question by focusing primarily upon the most recent iteration of their rule of law indicator, for “measurement validity is specifically concerned with whether operationalization and the scoring of cases adequately reflect the concept *the researcher seeks to measure*” (Adcock and Collier 2001, p. 529; our emphasis), and Kaufmann and his colleagues hold that RL in particular captures the “norms of limited government” (Kaufmann et al. 2007a, p. 555) that are central to the literature on institutions and growth (Kaufmann et al. 2007a, p. 561; see also Kaufmann and Kraay 2002, p. 192).³

Table 1 recapitulates the definition of the rule of law that appears in the most recent iteration of the WGI—i.e., the concept that Kaufmann et al. purport to measure—and includes summary information on the sources, respondents, and questions incorporated into the RL indicator. The questions tend to fall into three principal subgroups: crime, property rights, and judicial and security institutions. We evaluate RL’s content validity by reflecting upon each subgroup and asking (i) whether key elements are omitted or (ii) inappropriate elements are included in the indicator’s construction (Adcock and Collier 2001, p. 538).⁴

PLEASE INSERT TABLE 1 ABOUT HERE

Crime. RL includes data on both crime in general and a number of specific offenses (i.e., kidnapping) or categories of crime (i.e., organized crime). The underlying data sources include expert assessments of crime prevalence rates and trends, household survey data on victimization, and subjective assessments of the costs that criminals impose on business. And the rationale for their inclusion is more or less obvious: A society that is marked by the rule of law is unlikely to be crime-ridden and vice versa.

Our concerns therefore derive not from the incorporation of crime data *per se* but from the manner in which they’re incorporated. After all, the data summarized in Table 1 are unlikely to present an unbiased portrait of the level and distribution of crime in society. On the contrary, they are likely to give undue weight to crimes that affect business and elite interests. While the surveys undertaken by the World Bank and the World Economic Forum are self-consciously designed to tap the costs crime imposes on business (see, e.g., Kaufmann et al. 2007b, Table B5), the household surveys undertaken by Gallup and the various regional barometers are likely to prioritize the interests of the elite for a less obvious reason. Respondents in crime victimization surveys are known to overreport property crimes that tend to affect the better off and underreport violent crimes that tend to affect their less fortunate compatriots (Bergman 2006, p. 221).

Thus, the crime data incorporated into RL are best portrayed neither as proxies for the rule of law writ large nor as random variables but as indicators of the elite’s ability to insulate itself from what Alejandro Portes and Brian Roberts have labeled the “forced entrepreneurship” (2005, p. 67) of the poor in the Latin American context. According to Portes and Roberts, the relentless onslaught of debt, austerity, and free market reform has provoked the unprecedented growth not only of poverty and inequality but of anger

³ Adcock and Collier worry that scholars who fail to put their conceptual difference aside for the purposes of measurement validation will find themselves paralyzed by intractable disputes over concepts (2001, pp. 538-9). We therefore follow their advice and take Kaufmann et al.’s definition of the rule of law as a given for the purpose of the discussion of validity—despite our skepticism about their conceptualization more generally. We should note briefly that we find the conceptualization—but not the operationalization—lying behind their “government effectiveness” measure to more adequately capture the classical Weberian notion of good governance. See Kurtz and Schrank (2007a, 2007b) for a discussion.

⁴ An alternative approach would assess convergent and discriminant validity by asking whether RL (i) is strongly correlated with different measures of the rule of law and (ii) weakly correlated with indicators of distinct but related governance concepts. While Kaufmann and his colleagues tend to *incorporate* the available indicators of rule of law into their own metaindicator, and thereby render RL more or less immune to convergent validation, they boast that RL is *strongly* correlated with their other governance indicators (Kaufmann et al. 2007a, p. 555)—and thereby raise serious doubts about their discriminant validity.

and resentment in Latin American cities. “Property crime may rise in these contexts,” they maintain, “as some members of the subordinate classes take matters into their own hands in order to redress both absolute and relative deprivation.”

Our own admittedly crude data analysis confirms their sense that Latin America’s current crime wave betrays a pattern of “entrepreneurial rationality” (Portes and Roberts 2005, p. 75). We regressed crime victimization data from *Latinobarometer* (1 = victim within the past year) on a ten point scale designed to capture the respondent’s income—an admittedly crude proxy for social class—and a series of country dummies in 2005 and found a substantial positive effect (odds ratio 1.06; p value < .0001; n = 19,764).⁵

Our point is most assuredly neither to rationalize property crimes nor to deny their relationship to the rule of law but to underscore the fact that they are neither (i) inclusive of the full array of crime in most societies nor (ii) random occurrences and that RL therefore *omits* important elements of the rule of law (i.e., violent crime, violations of labor and environmental law, etc.) and inappropriately—if indirectly—*includes* the various correlates of crime at the country level (e.g., poverty, inequality, the age distribution of the population, collective efficacy, etc. [see, e.g., Sampson et al. 2007]).

Property rights. RL includes data on property rights and threats to their sanctity (i.e., expropriation) as well as information on a number of specific categories of property right (i.e., intellectual property). The property rights indicators are drawn from commercial risk rating agencies, firm-level surveys, and inter- and nongovernmental organizations. But the rationale for their inclusion is unclear, for Kaufmann et al.’s definition of the rule of law neither makes specific reference to property rights nor militates against their compromise or outright transgression by *legal* means.

Take, for example, eminent domain provisions in United States property law. They give the government the authority to override legitimate property claims in a host of well-defined situations, and are therefore perfectly consistent with the rule of law, but nonetheless give rise to virulent opposition in practice, and are therefore likely to undercut the RL score if used too frequently.

Our objection is readily addressed, however, neither by asserting the infrequency of eminent domain nor by retroactively including a reference to property rights in the definition of RL, for the expropriation and confiscation of property are frequently deployed in the war *against* corruption and crime and their use is therefore no less consistent with the growth than the absence of the rule of law. Park Chung Hee’s campaign against the “illicit accumulators” in South Korea offers a case in point (Schrank 2007), and more recent examples are readily available in Africa, Asia, and Latin America (see, e.g., Economist 1981; Arnold 1999).

But one need not travel to the developing world to witness the confiscation and expropriation of assets deployed in defense, rather than contravention, of the rule of law. The Racketeer Influenced and Corrupt Organizations (RICO) Act offers a no less apposite North American example. After all, RICO gives US government prosecutors the right to seize the assets of allegedly corrupt or criminal organizations before they have attained a conviction—and thereby threatens the economic lifeblood of suspect enterprises. A number of observers have given RICO credit for the demise of the Mafia in the 1990s (Laurence 2004). And James Jacobs and Lauryn Gouldin have therefore labeled the act “the most important substantive antiorganized crime statute in history” (Jacobs and Gouldin 1999, p. 169).

The statute has not, however, been free of criticism. While law enforcement officials are justifiably fond of RICO, and favorably disposed toward asset forfeiture in particular, their critics decry “policing for profit” (Blumenson and Nilson 1998) and worry that the law not only gives the government a “license to

⁵ Complete results are available from the authors upon request.

steal” (Levy 1996) from legitimate businesses but constitutes a “nuclear deterrent to rational negotiations” as well (Arkin in Newsweek 1989). Is the cure of RICO worse than the disease of corruption and crime? The answer is anything but obvious, for the RICO experience suggests not only that the same institution can simultaneously militate in favor and against the rule of law but that property claims themselves are by their very nature subjective and controversial—even in the advanced industrial countries.

They are arguably more controversial in the developing world, however, and are therefore decidedly ill-equipped to play a meaningful role in perceptions-based indicators of the rule of law. “A government that evicts squatters will in all likelihood be portrayed as a threat to private property by the squatters and a bulwark against expropriation by the landlords,” we have argued, “and the problem is likely to be compounded by the fact that in much of the developing world this year’s squatters are likely to be next year’s landlords and vice versa” (Kurtz and Schrank 2007b).

Nor are the examples of allegedly ill-gotten gains in the US and squatters in the developing world unique. The problem of ambiguous property claims is widespread and will in all likelihood be aggravated by the growing salience of intangible or intellectual property in the future (see, e.g., Evans 1997), for different states and societies have radically different conceptions of the appropriate length and scope of patent, copyright, and trademark protection.

Judicial and security institutions. RL also includes questions on courts, contract enforcement, and the police. The “courts, cops, and contracts” cluster, as we call it, incorporates data from firm and household surveys as well as expert assessments; tracks the government’s ability and willingness to ensure the safety and security of the population; and plays an almost indisputable part in the assessment of the rule of law.

We nonetheless wonder why Kaufmann and his colleagues simultaneously omit or de-emphasize a series of regulatory agencies that are no less central to the security and well-being of the population including but by no means limited to the tax, labor, food, drug, health, and safety inspectorates; banking and financial overseers; and environmental monitors. Regulatory authorities are involved in *law enforcement*, after all, and their virtual exclusion from RL arguably says more about the types of laws valued by the indicator’s authors and advocates than about their importance to the concept itself.⁶

In short, RL appears to suffer from a pronounced and systematic pro-business bias. The crimes covered, the institutions included, and the interests served all point in the same direction—as do the interests, institutions, and crimes overlooked. And the biases will arguably prove not only threatening but fatal to content validity when aggravated by what for lack of a better term we’ll call sampling error. After all, the bulk of the data incorporated into the measure—approximately two-thirds of the “representative” sources that are weighted most heavily in the final indicator (see Table 1)—are derived from either private investors or their commercial advisers.⁷

Kaufmann and his colleagues admit that pro-business biases are possible in theory but belittle their impact in practice (2007a, p. 556). We’ve already established that reported crime victimization varies by social class. Do business perceptions of other aspects of the rule of law differ substantially as well? Fortunately, the Latinobarometer data employed by Kaufmann and his colleagues invite a direct test.

PLEASE INSERT TABLE 2 ABOUT HERE

⁶ A brief review of the WGI “regulatory quality” (RQ) indicator does little to assuage our concerns. RQ addresses the degree to which tax, labor and environmental laws compromise business competitiveness, for example, but fails to ask to what degree they protect workers or the environment (Kaufmann et al. 2007, Table B4).

⁷ Another comes from a nongovernmental organization that the authors themselves describe as “conservative.” And only one source includes any citizen surveys at all.

Table 2 includes the results of two ordered logistic regression models. The dependent variables are the answers to the questions about trust in the judiciary and police that are incorporated into the RL indicator. They are coded from 1 (much trust) to 4 (no trust). They are regressed on an indicator variable that assumes the value of 1 wherever the respondent is a businessperson and a series of country dummies. And they underscore the differences between businesspersons who consistently evince less faith in police and judicial institutions than their compatriots.⁸

A defender of the WGI might rebut charges of bias by portraying businesspersons and their advisers as particularly desirable data sources whose opinions should loom large in their indicators. Private investors not only have more experience with institutions like the police and courts than the average citizen, they might argue, but are also in the vanguard of the struggle for growth and development in market societies. But non-businesspersons are neither ignorant nor unimportant to the process of growth and development; on the contrary, they make countless individual decisions—about everything from personal savings and voting to the treatment and education of their children to the decision as to whether to open a business in the first place—that aggregate into important societal outcomes. Such decisions not only influence but are influenced by the broader socio-political context, however, and a governance indicator worthy of its name will therefore take the ideas and interests of non-businesspersons seriously.

In short, we have argued not only that the concept of rule of law is poorly explicated but that the RL indicator itself is invalidated by skewed questions addressed toward a biased sample of respondents. The consequences are particularly acute when RL is incorporated into growth regressions as an exogenous variable, for a positive coefficient could reflect any combination of at least four different underlying processes: first, a positive relationship between *actual rule of law* (e.g., crime control, confidence in the courts) and growth; second, a positive relationship between *some but not all aspects of the rule of law* and growth; third, a positive relationship between the *underpinnings of the rule of law* (e.g., equity, opportunity, etc.) and growth; or, fourth, *herd behavior* on the part of investors who receive their advice from the same consultants and risk rating agencies and thereby animate growth regardless of the so-called fundamentals.

We should note, by way of conclusion, that by examining RL rather than one of their other indicators we have arguably offered a generous test of the World Bank Institute's project. After all, the other Worldwide Governance Indicators are not only poorly explicated and subject to the aforementioned selection and perceptual biases but also tend to conflate institutions with policies in a manner that renders econometric findings all but uninterpretable. Take, for example, the indicator of "Regulatory Quality," which incorporates questions about trade policies and price controls, or the indicator of "Government Effectiveness," which asks how "easily foreign investors can go about their business" (Kaufmann, Kraay, and Mastruzzi 2003, p. 87). The point is not simply that these indicators capture potentially transitory policies rather than the enduring rules of the game that loom large in the institutional literature (see, e.g., Kurtz and Schrank 2007b, p. 564) but that in so doing they undermine their alleged utility. Do positive regression coefficients reflect the true effects of institutions? The effects of policies that are conflated with institutions? The correlates of those policies? Herd behavior on the part of investors who purchase their data from the same sources as the WGI? Or something else entirely? Unfortunately, the answer is

⁸ Kaufmann et al. appear to believe that systematic differences between business and non-business perceptions can only contaminate their estimates if they alter the rank order of countries (2007a, p. 570). We can neither confirm nor rule out alterations in rank order given the complete absence of citizen surveys for most of their indicators but we are not convinced that they are necessary to introduce bias into RL—which is, after all, an interval measure. Furthermore, the Afrobarometer data reported in Schrank and Kurtz (2007b, p. 566, Table 1) suggest that businesspeople themselves offer inconsistent evidence on the quality of governance. While they actually report better access to public services than their compatriots, they nonetheless hold the government in lower esteem.

unknowable in light of the conceptual confusion that pervades perceptions-based indicators and unacknowledged by their authors.

3. Conclusion: bringing objectivity back in?

Where do we go from here? We begin to provide an answer by briefly reviewing the experience of one of the more successful subjective measures in the history of the social and medical sciences: self-reported health status. A quarter of a century has passed since Jana Mossey and Evelyn Shapiro first realized that self-reported health status provided a better predictor of seven year survival rates among the Canadian elderly than either data drawn from medical records or self-reports of specific conditions (Mossey and Shapiro 1982) and much has been learned in that time. Subjective assessments of global health status are by now known to: (i) be highly correlated with other indicators (or correlates) of health and well-being; (ii) add enormous explanatory power to multivariate models of mortality (Idler and Benyamini 1997); and (iii) suffer from conceptual limitations that sharply circumscribe their practical utility (Krause and Jay 1994).

Medical professionals know that individuals who label their own health “poor” are likely to die sooner than people who consider themselves fit (Idler and Benyamini 1997), but they don’t know why they do so and are therefore unable to act upon their knowledge. Some suspect that self-reports are more inclusive than objective data, tap undiagnosed diseases, or capture the effects of co-morbidity. Others think they capture trajectories rather than levels. Some point to underlying correlations with family history, socio-economic status, or behavioral characteristics. And still others to self-fulfilling prophecies. But unless and until an actual causal mechanism is adduced medical professionals will continue to act not upon subjective but upon objective data when choosing treatment.

Perceptions-based measures of governance have much in common with subjective assessments of health status. They are, after all, highly correlated with each other and with the presumed covariates of good governance (e.g., GDP per capita, school attainment). They, too, add explanatory power to multivariate models. And they also suffer conceptual shortcomings that undercut their practical utility. Take, for example, the rule of law. A positive RL coefficient in a cross-national growth regression is difficult to interpret let alone act upon. It could reflect the influence of one or more of the actual inputs to RL (e.g., crime, courts, contracts), which are themselves endogenous and are therefore not readily altered by public policy. Or it could betray the impact of the direct effects of the social and economic conditions that influence those inputs (i.e., per capita income, social capital, etc.), which are arguably even less susceptible to policy manipulation. It could be a product of pure measurement error of the sort admitted (but perhaps underestimated) by the indicator’s authors. Or it could be the product of herd behavior among investors who purchase their information from the indicator’s underlying sources. Unless and until we have clear answers to these questions, however, the WGI will be of limited practical utility.

The problem is aggravated, we believe, by the implicitly contradictory targets embedded in the indicators and subindicators that go into the WGI. After all, the sources employed by Kaufmann and his colleagues reward governments for policies that are almost certainly in tension with each other. What, then, should developing country policymakers who hope to improve their WGI scores *do*? Outlaw the expropriation of assets in order to maximize RL or adopt laws like RICO and use them to expropriate corrupt businesspeople and politicians so as to gain better scores on the control of corruption (CC) indicator? Extract and invest resources in schools and roads so as to bolster “government effectiveness” (GE) or cut taxes in order to ensure “regulatory quality” (RQ)? Clamp down on protest so as to ensure “political stability” (PS) or give the protesters and the press a free pass in an effort to maximize “voice and accountability” (VA)? Almost every potential solution aggravates another problem, and the WGIs therefore punish poor countries for their very poverty. If they could solve their social and economic problems, after all, they wouldn’t be worried about their governance scores in the first place.

What, then, is to be done? The medical professionals who collect and analyze self-reported health data ultimately turn to objective alternatives when diagnoses and treatments are necessary. Unfortunately, however, objective governance data are not readily available. The traditional proxies—e.g., tax ratios and the like—have been largely discredited for one reason or another. And nobody has seen fit to invest in the creation of alternatives. We think such an effort is long past due, however, and would yield important payoffs not only for social scientists and policymakers who are skeptical of perceptions-based measures but also, ironically, for their defenders, for the existing subjective indicators arguably cry out for the legitimating aura of an objective benchmark.

What would objective governance indicators look like? First, they would of necessity be issue-specific. While subjective health data are “global” in nature, the objective data that guide diagnosis and treatment are necessarily circumscribed to particular health problems. Similar specification would be required for governance data, especially in light of the fact that governance is known to vary across issue areas and sectors within countries as well as between countries more generally (see, e.g., Johnson 1982). Second, they would focus on issues with measurable outputs: education, health care provision, and postal delivery come immediately to mind (see Putnam 1992). Third, they would incorporate data on *inputs* as well as outputs. One problematic feature of the WGI is their almost complete inattention to the problem of opportunity cost. A government that bankrupted itself in a successful war against corruption would presumably see its corruption control score improve—at least in the short run. But it’s not clear that it would have made a wise choice in doing so. And, finally, it would differentiate the efficacy of governance—that is, the enforcement of the rules of the game—from the quality of policymaking—that is, the creation of the rules of the game—as well as from the socio-cultural context that underpins both the rules and their enforcement. It would thus demand independent data on inputs and outputs as well as policies and contextual factors that might affect both.

We can do no more than provide a brutally abbreviated example at present. Neither available data nor the space we have left would tolerate more. But we’ll try to illustrate the *sort* of approach we think desirable by examining the issue of labor law enforcement in Latin America—which at least in theory meets the aforementioned criteria. It is not only narrow in scope, at least when compared to “global” issues like the rule of law, but informality provides a straightforward indicator of the output. Successful enforcement should issue in a relatively low level of informality *ceteris paribus*. The number of enforcement personnel constitutes the input. And the ratio of enforcement personnel to the active labor force constitutes the standard measure of the intensity of the enforcement effort (ILO 2004, p. 13). Control variables are available to capture the effects of policy differences and social structure. And residuals from a regression of informality on the aforementioned indicators should therefore provide an admittedly crude proxy for the *quality* of the enforcement effort.

We implement the strategy by regressing the informal percentage of the labor force (Gasparini and Tornarolli 2007, Table 3.3) on GDP per capita (World Bank 2006)—which serves as a simple proxy for the overall level of social and economic development—and an index of job security that is positively, if inconclusively, related to informality (Heckman and Pagés-Serra 2000). Figure 4 plots the cross-national variance in informality unexplained by the combination of GDP per capita and job security regulation by the ratio of labor inspectors to laborers in 17 Latin American countries in the early 2000s (Schrank and Piore 2007, Table 5; and updated data where available).⁹

PLEASE INSERT FIGURE 1 ABOUT HERE

⁹ Guatemala is dropped due to missing job security index data. We present the results this way for clarity of exposition. Alternative specifications (e.g., entering all three predictors simultaneously; using different indicators of informality, dropping the job security index and including Guatemala, etc.) yield parallel results.

The data in the figure suggest several interesting conclusions about governance. First, they fly directly in the face of the widespread notion that “limited government,” in Kaufmann et al.’s formulation, is necessarily the best government. While the cross-sectional data permit no causal claims, they are at least consistent with the idea that labor market regulations inhibits, rather than promotes, informality in Latin America. Second, they suggest that quality matters too. The three largest negative residuals—Chile, Costa Rica, and the Dominican Republic—have all taken efforts to improve the quality of enforcement in recent years (see Schrank and Piore 2007). By way of contrast, two of largest positive residuals, Brazil, and in particular Mexico, have moved in the opposite direction. And, third they suggest that objective governance indicators could, with a good deal of additional effort, provide a valuable complement or even alternative to the existing perceptions-based measures. One would not only need longitudinal as well as cross-sectional data from a larger number of countries but would also have to deal with questions of endogeneity and selection. But these issues arise with subjective data as well—and there they’re compounded by issues of conceptual ambiguity, systematic and random measurement error, and causal indeterminacy.

A thoroughgoing collective effort to aggregate and disseminate objective data would not only help researchers combat such problems but would also provide an alternative if such a campaign were to fail. Data on inputs and outputs are available across a wide array of issue areas in a large number of developing countries. It’s high time that intergovernmental organizations began to aggregate them and make them available to policymakers, scholars, and activists around the world.

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Table 1. Components of the WGI Rule of Law Indicator

Rule of law—measuring the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, the police, and the courts, as well as the likelihood of crime and violence				
	Source	Type	Respondents	Questions
Representative	Global Insight Global Risk Service	Commercial information provider	Staff	Costs of crime Kidnapping Contract enforcement (public and private)
	Economist Intelligence Unit	Commercial information provider	Correspondents	Violent crime Organized Crime Judicial speed/fairness Contract enforcement Expropriation
	World Economic Forum Global Competitiveness Survey	Nongovernmental organization (with business representation)	Firms (survey)	Common crime (cost to business) Organized crime (cost to business) Money laundering Police Quality Judicial independence and reliability Intellectual property rights Financial protection Tax evasion Illegal political donations
	Gallup World Poll	Commercial polling firm	Households (survey)	Confidence in police Confidence in courts Crime victimization
	Heritage Foundation Index of Economic Freedom	Nongovernmental organization (conservative)	Staff	Property Rights
	Cingranelli & Richards Human Rights Database	Academics	Expert codings of US Dept. of State and Amnesty International reports	Judicial independence
	Merchant International Group Gray Area Dynamics	Commercial information provider	Staff	Organized crime Legal safeguards
	Political Risk Service International Country Risk Guide	Commercial information provider	Staff	Law and order
	Business Environment Risk Intelligence Quantitative Risk Measure in Foreign Lending	Commercial information provider	Staff	Financial fraud Money laundering Organized crime
	US Department of State Trafficking in Persons Report	USDOS	Expert assessments	Human trafficking
	Global Insight Business Conditions and Risk Guide	Commercial information provider	Staff	Judicial independence Crime
	Non-representative	African Development Bank Country Policy and Institutional Assessments	African Development Bank	Country economists
Afrobarometer		University/NGO collaboration	Household survey	Police response time
Asian Development Bank Country Policy and Institutional Assessments		Asian Development Bank	Country economists	Rule of law
Business Environment and Enterprise Performance Surveys		World Bank & European Bank for Reconstruction and Development	Survey (firms)	Judicial quality Property rights Crime and business
Business Environment Risk Intelligence		Commercial information provider	Panel of experts	Contract enforcement
Bertelsmann Transformation Index		Nongovernmental organization	Staff	Rule of law Private property
Freedom House Countries at the Crossroads		Nongovernmental organization	Staff and consultants	Rule of law
World Bank Country Policy and Institutional Assessments		World Bank	Country Economists	Property rights
Freedom House		Nongovernmental organization	Staff and consultants	Rule of law
Global Integrity Index		Nongovernmental organization	Local country experts and peer review	Executive accountability Judicial accountability Rule of law Law enforcement
IFAD Rural Sector Performance Assessments		IFAD	Country economists	Access to land Access to agricultural water
Latinobarometer		Nongovernmental organization	Survey (household)	Trust in judiciary Trust in courts Crime victimization
Institute for Management Development's World Competitiveness Yearbook		Education organization	Survey (business)	Judicial quality Intellectual property Crime

Source: Adapted from Kaufmann et al. 2007b, esp. Table B5. Representative sources are available for most countries and therefore weigh more heavily in the final indicators.

Table 2: Do businesspeople and their neighbors perceive government in the same way?

Question	Responses	Businessperson
Trust in the judiciary	1 = much trust; 2 = some trust;	Odds ratio = 1.15 (p < .010)
Trust in the police	3 = little trust; 4 = no trust	Odds ratio = 1.25 (p < .001)

Self-identified businesspeople are coded 1; others are coded 0; country dummies are suppressed; and odds ratios for businesspeople are presented next to their parenthesized p values. Non responses and "don't know" are dropped. The data are from Latinobarometer (2005); the more recent data available to KKM are not publicly available.

Fig. 1: Labor law enforcement in Latin America: quantity and quality



