

Measuring Institutions

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Institutions are “the rules of the game in a society, or more formally, [the] humanly devised constraints that shape human interaction” (North 1990). In an abstract sense, there is a great deal of agreement on this definition. A perusal of recent literature suggests, however, that there is much less agreement on how to measure institutions empirically. How much better or worse are the economic institutions in the United States than those in France? Does the difference in institutional quality affect explain differences in economic outcomes between the two countries, and if so, how much of the difference?

Much work has been done and some progress has been made in recent years to identify the causal impacts of institutions on growth. Path breaking work by Mauro, Knack and Keefer, and others identified a correlation between measures of expropriation risk and corruption on the one hand, and economic outcomes on the other. The question then turned to one of causation. Does corruption cause negative outcomes, or do negative outcomes lead to more corruption? On this front, the work of Acemoglu, Johnson and Robinson (2001), and Engerman and Sokoloff (2002) are particularly significant contributions. These authors address the issue of causation by looking for deep historical differences that affected the formation of institutions in colonies of the European powers—mortality rates in the case of Acemoglu, Johnson and Robinson and inequality driven by scale economies in the case of Engerman and Sokoloff.

But there is not yet universal agreement that these papers measure institutions properly. Glaeser et al (2004) argue that the majority of the current measures of institutions used in the literature are measures of outcomes rather than institutions. Motivated by the example of North and South Korea, Glaeser et al claim that the empirical measures of institutions used in the literature “cannot be plausibly interpreted as reflecting durable rules, procedures or norms that the term ‘institutions’ refers to.” They suggest that differences in institutions are more properly measured by “objective institutional rules,” which essentially come down to differences in the structure of constitutions: judicial review, terms of appointments of supreme court justices, and the like. Glaeser et al show that these measures have little relationship to aggregate economic outcomes. I will revisit this argument in Section 2.

A separate issue is that even if we accept the evidence that institutions cause growth, we know little about which institutions are fundamental in this process. One problem, as we will see when we look at the data later in this paper, is that the various measures of institutions are very highly correlated. This makes separating the effect of different institutions extremely difficult. There are a few recent attempts to isolate the specific institutions which are responsible to better outcomes, including an important attempt to “unbundle” institutions by Acemoglu and Johnson (2005), who examine the effect of broad property rights institutions and narrow contracting institutions, finding that only the latter are important for economic outcomes. I discuss this effort in more detail in Section 4 of this chapter, because it illustrates many of the challenges involved in measuring institutions.

The intention of this chapter is to examine the various measures of institutions which have been used in the literature. We begin by discussing three methods of measuring the institutional quality which have been proposed in the literature. The measures can be divided according to several criteria. Some measure formal institutions, some measure a combination of formal and informal institutions; some are broad measures of property rights, others are narrow measures of specific institutions; some are based on impressionistic surveys of legal experts, academics, or business people, and others are based on an analysis of laws and constitutions. Are all of these measuring the same thing? Clearly they are not, but that may not matter if the measures are all sufficiently correlated with one another.

Examining the correlations between measures of institutions commonly used in the literature in Section 3, we find that measures using a common measurement method are generally highly correlated. This is less true when we compare measures using different measurement methods. Does this matter? That is, do measures produced with different measurement methods produce different empirical results? I show that they do, using both an example and evidence from the literature. In fact, I argue that the different methods of measuring institutions are actually measuring something different. The literature is generally consistent in telling us that formal institutional structures have little effect on outcome, while informal institutions—how laws are enforced—do matter.

Section 2: The meanings and measurements of institutions

Empirical measures of institutions can be divided along several dimensions. Here I discuss the distinction between formal and informal institutions and between broad and

narrow institutions. Usually, the measure of formal institutions will be “hard” while measures of informal institutions will be “soft.” Hard measures are based on written documents which are verifiable and not subject to judgment. Soft measures are impressions of experts or participants in an economy. Two questions that should be asked of any measure are: What are we measuring and What is left out?

A clear discussion of and rationale for measuring formal institutions is provided in Glaeser et al (2004), who argue that we should focus on measuring institutions through “objective institutional rules.” They discuss several examples of this approach, including differences in electoral laws (proportional representation vs. majoritarian elections) and judicial independence. On the latter, Glaeser et al use two measures of judicial independence developed in La Porta et al (2004). One of these relates to the term of appointment of judges to the supreme court. The second relates to judicial oversight of legislation.

These are clearly hard, objective measures of constitutional differences. As a result, they are not subject to the criticism that their measure is influenced by the outcomes they are meant to predict. Experts, for example, may judge a country to be more corrupt after an economic crisis, but the measure of the term for which supreme court justices are elected cannot be similarly affected. The other critical issue—endogeneity caused by reverse causation or unmeasured differences—can be overcome by proper instrumentation. We will see in Section 3 that many differences in political institutions are highly correlated with legal origin. Using this instrument, Glaeser et al show that the connection between these measures and economic outcomes is not strong.

Measuring institutions through formal, hard, measures resolves issues of subjective bias. But, how complete a picture of the institutional environment do these measures capture? Take the example of Peru and judicial independence, as measured developed by La Porta et al (2004). The La Porta et al (2004) data on judicial independence are based on the length of the terms of supreme court and administrative court judges, the supreme court's control over administrative courts, and the power of administrative judges. Peru receive a perfect score of seven according to these measures, which were captured in 1995, during the Fujimori administration. Constitutionally, it was a perfect place for an independent judge. As McMillan and Zoido (2004) colorfully point out, the reality was somewhat different. In fact, Peru's judicial opinions were available for purchase, the sales agent was Fujimori's right hand man. Clearly something remains uncaptured in the picture of the institutional environment painted by the hard measures of constitutional design.

If formal measures of institutions involving an analysis of constitutions leave something out, what are the alternatives? One answer is that the institutional environment varies both in the formal laws that govern interactions and in the way in which formal laws and rules are implemented and enforced in a country or region. I refer to the differences in the way in which laws are enforced as differences in the informal institutions which govern a country. As the Peru example makes clear, there is unquestionably something fundamental about the institutional environment which is not captured by the formal measures. From an empirical point of view, the existence or importance of differences in informal institutions is only part of the question. The other

part is whether it is possible to develop a measure of informal institutions which is independent of the outcomes we are trying to explain.

Social scientists have used several measurements which capture both differences in formal constitutional arrangements and differences in informal institutions. The most commonly used measures of this sort are those derived from the opinions of panels of experts—academics, practitioners or consultants. Examples of these are the Political Risk Services measures of country risk, including the widely used risk of expropriation measure, Transparency International's index of corruption, and the World Bank's World Business Environment Survey.

How do these measures differ from the measures of constitutional differences previously discussed? Take the example of indices produced by Political Risk Services (PRS), a consulting firm providing information to investors such as multinational firms. PRS produces various measures of investment risk. One which has been widely used is expropriation risk (Acemoglu, Johnson and Robinson; Knack and Keefer), which measures the likelihood a private investment will be captured by the state. A private investor's ability to protect an investment depends partly on the formal institutional structure of a country—constitutional rules governing the independence of the judiciary, for example. But expropriation risk depends on how the constitution is implemented and how laws are enforced as well—that is, on informal institutions. Expropriation risk, then, measures a combination of formal and informal institutions. Assuming the endogeneity issues can be properly dealt with, these measures will allow us to determine the effect of the overall institutional quality on economic outcomes.

At one extreme we have hard measures of formal institutions and at the other extreme, soft measures which represent a mixture of formal and informal institutions. There are also measures which fall between these two extremes—that is, they are “harder” than these impressionistic measures, but softer than the constitutional measures. Here I will discuss two of these. The first is one which has been widely used in the economics and political science literature, the Polity IV measure of constraint on the executive. The description of this index in the Polity IV manual indicates that it measures the independence of the legislature and judiciary from executive control. One can justify a focus on constraint on the executive because the executive is in the best position to unilaterally divert the power of the state for his or her own gain.¹ There are two potential issues with the Polity IV measure. The first is pointed out by Glaeser et al (2004), who argue that though the measure has the appearance of being a hard measure based on constitutional differences, it should be viewed as a measure based on expert opinion. Glaeser et al view the constraint on the executive as representing a measure of outcomes rather than institutions. They point to rapid changes in the constraint index unaccompanied by any changes in the country’s constitution—for example following elections during the 1990s in Haiti or Peru.

¹ The Polity IV constraint measure includes some reference to the judiciary, but focuses mainly on the legislature as the constraining power. For example, the users manual describes the highest level of executive constraint as:

- 1) A legislature, ruling party, or council of nobles initiates much or most of the important legislation.
- 2) The executive (president, premier, king, cabinet, council) is chosen by the accountability group and is dependent on its continued support to remain in office (as in most parliamentary systems).
- 3) In multi-party democracies, there is chronic “cabinet stability.”

The national legislature is less likely than the judiciary to provide constraints on lower level executives, for example, mayors.

The second issue with the Polity IV measure is that the diversion of the state's power can occur at all levels of government, while the Polity IV measure applies to *the* executive. In practice, this may be less of a concern where the measure focuses on the independence of the judiciary. Institutions which constrain *the* executive are also likely to constrain lower level authorities as well.

The measure of the constraint of the executive is clearly harder than the impressionistic measures from consulting firms and surveys. But the measure still has some impressionistic component. Another important set of measures of the institutional environment which might be placed in a similar position along the hard-soft line are those associated with the World Bank's *Doing Business* project. These include formal indicators of the costs of entry, labor regulations, contract enforcement, and other aspects of running a business. While most of the *Doing Business* measures are developed by law firms and consulting firms, they are intended to be comparable measures of the time and cost required to take certain well specified actions. For example, contract enforcement is based on responses from lawyers to questions related to a very well specified situation: the collection of a debt representing 200% of GDP, in the capital city, where the plaintiff is 100% in the right, and so on. So although based on surveys, these measures are harder than those from other surveys.

As with the formal constitutional measures, we might ask whether the *Doing Business* indicators fully capture the relevant institutional environment. One answer to this question is anecdotal: On legal formalism, Vietnam scores somewhat better than France and Germany. One doubts there are many business people who would rather trust their fate to courts in Vietnam's than to courts in France or Germany. A more balanced

answer comes from the analysis undertaken by Djankov et al (2003), who show that formalism is a highly significant determinant of broader measures of the enforceability of contracts, the impartiality of the legal system, and so forth.² Though they do not report the R-squares for their regressions, it is clear that formalism explains only a part of the broader measures of the effectiveness of the legal system, and that these broader measures of the ability to enforce contracts are affected by factors other than legal formalism, such as ethnic fractionalization.

A final issue worth noting is the time over which measures are available. Empirical studies of the impact of institutions on economic outcomes did not become the subject of academic investigation until the first part of the 1990s. Even though consulting firms were publishing indicators before that time, most measures of institutions used in the literature go back no earlier than 1980. The IRIS-3 data set, compiled by Knack and Keefer (1995), cover the period 1982-1997. The Heritage Foundation has produced the Freedom Index, and its components, annually since 1995. Mauro uses *Business International* data from the 1980-1983. A few indices have been created retroactively. One notable example of this is the Polity IV data, which extend back to 1800.

Section 3: Consistency across sources

How closely correlated are the various hard and soft measures of the institutional environment? We examine that question here using a variety of measures commonly used in the literature. If the measures are all very highly correlated, then perhaps differences in methodologies are not empirically important. We divide the measures into three

² Djankov et al use various measures from the World Bank's World Business Environment Survey and the measure of contract enforceability from the Business Environment Risk Intelligence service.

categories: broad property rights and corruption; and the legal and regulatory environment; and constitutional differences determining political institutions. We also examine correlations between these variables and three variables which have been most commonly used as instruments: settler mortality, English legal origin, and distance from the equator.

The most varied set of measures relates to what we might think of as broad property rights. How well does the state protect investments of private individuals? In the short run, the state has an incentive to use its power to expropriate. What constrains it from doing so? The current measures of choice for broad institutions are the risk of expropriation developed by Political Risk Services, and the Polity IV measure of constraints on the executive. The political economy literature (see Persson, Tabellini and Trebbi (2003); Gerring and Thacker 2005; and Kunicova and Rose-Ackerman 2005) uses indices of corruption from Transparency International (TI) and the World Bank (WB, Kaufmann, Kraay and Zoido). Mauro's early paper in this literature used Business International's (BI) corruption perceptions index. The correlations among these five variables are shown in the top half of Table 5.

The three indices of corruption—TI, WB, and BI—are all very highly correlated, with correlations of 0.77 or higher. The index of risk of expropriation appears to be measuring something very similar. The correlations between expropriation risk and the corruption measures are also generally quite high—above .70 in the case of the TI and WB indices. Constraint on the executive, which is constructed in a different way from the other indices, does appear to be measuring something different from the pure perceptions

indices. The correlations between constraint on the executive and the measures of corruption range from 0.45 to 0.55.

Table 1 include six measures of political institutions used in recent papers. The first two, unitarism (anti-federalism) and parliamentarism are from Gerring and Thacker (2005). Unitarism measures the degree of centralization of power.³ District magnitude and proportional representation are the political measures used by Persson, Tabellini and Trebbi (2003). These are measures of electoral rules, with district magnitude representing how close the electoral system comes to single member districts and proportional representation reflecting the use of party lists as opposed to direct election of members of the legislature. Finally, judicial independence and constitutional review by the judiciary are taken from La Porta et al (2004). A higher value for judicial independence indicates that supreme court and administrative justices are appointed for longer terms, while judicial review indicates the constitutional right of the judiciary to review the constitutionality of the country. Each of these, then, represent quite formal measures of political institutions, based on constitutional differences rather than expert opinions.

Of the formal measures of political institutions, only parliamentarism is strongly correlated with broad measures of property rights, and even in this case, the correlation is 0.57 or lower. The formal measures of political institutions are clearly measuring something quite different from the measures of broad institutions. Note that the measures of judicial independence and judicial review are only very weakly correlated with constraints on the executive. This could be, as Glaeser et al argue, because constraints on

³ Gerring and Thacker define unitarism on a scale of one to five. Five is non-federal, 4 is semi-federal and 3 is federal. They then subtract one if the legislature is weakly bi-cameral and two if the legislature is strongly bi-cameral. Federalism is measured from one to three, with one representing a presidential system, three a parliamentary system, and two a semi presidential system.

the executive is endogenously constructed to measure something very different from formal constraints on the executive. Alternatively, the lack of correlation may suggest that the appointment terms and constitutional reviews of laws are poor measures of the power of the judicial branch, or that the power of the judicial branch is not the most important constraint on the executive branch.

Finally, Table 1 includes three instruments for institutions most often used in recent literature. Settler mortality is strongly correlated with distance from the equator (0.56), and somewhat correlated with English legal origin (0.32). With the exception of parliamentarism, English legal origin is more closely associated with formal political measures, and distance to the equator and settler mortality are more closely associated with the broad property rights and corruption measures.

Table 2 replaces the measures of broad institutions with measures of the quality of the legal system and the regulatory environment. Some of these measures are derived from impressionist surveys—the Heritage Foundation measure of property rights and the World Bank measure of rule of law—and others are based on procedural differences—labor regulations and contract enforcement from the *Doing Business* series. Here, there is a clear divide between the impressionistic measures of legal and regulatory quality on the one hand, and the *Doing Business* measures on the other. For example, the WB measure of legal institutions is correlated with the Heritage legal measure and the WB and BI measures of regulatory environment at levels of between 0.76 and 0.91. The correlations with the *Doing Business* measures of legal and regulatory efficiency range from 0.16 to 0.43. The strongest correlates with the *Doing Business* measure of contract enforcement are the *Doing Business* measures of labor regulation, and vice versa.

The correlations on Table 2 also indicate that the *Doing Business* measures are more closely associated with the measures of formal political institutions, and English legal origin, while the impressionistic measures of legal and regulatory efficiency are more closely associated with settler mortality and distance from the equator. As with the broad measures of institutions on Table 1, among the measures of political institutions, only parliamentarism is consistently associated with the impressionistic measures.

Table 2 also includes one of the corruption measures, as representative of the quality of broader institutions. There is an extremely high correlation between the World Bank's corruption measure and the World Bank and Heritage measures of legal institutions (0.91 and 0.87, respectively). The correlations between the corruption measure and the various impressionistic regulatory measures are only slightly weaker. Again, the correlations between corruption and the *Doing Business* measures are much lower.

Overall, the data on Tables 1 and 2 suggest that there is a divide between hard measures of institutions, based on constitutional differences and formal procedures, and soft measures of institutions, those with a stronger impressionistic component. Constitutional differences in elections, appointment of judges, and so forth, are strongly correlated with the *Doing Business* measures of procedural formalism and with English legal origin. The impressionistic measures of broad institutions and legal / regulatory institutions are highly correlated with one another, and with settler mortality and distance from the equator. These measure are more weakly correlated with formal constitutional differences. Interestingly, the formal constitutional measure which is most strongly correlated with the measures of broad institutions and the impressionist measures of the

legal and regulatory environment—parliamentarism—is also the formal measure most highly correlated with settler mortality and distance from the equator.

Section 4: What do we know about which institutions matter?

Empirically, why does it matter how institutions are measured? One answer to this question begins with a specific reference to current efforts to “unbundle” institutions (Acemoglu and Johnson, 2005).⁴ Acemoglu and Johnson examine the impact of two distinct measures of institutions—broad property rights and narrow contracting institutions—on income per capita and other macroeconomic outcomes. As in their earlier work, they focus on countries which were formally colonies of European powers. Their clever idea is to identify separate instruments for the two institutional variables. Key to the strategy is that the instruments separate along institutional lines. They show that settler mortality and indigenous population density in 1500 affect their measures of broad institutions, but not their measures of contracting institutions, and that English legal origin affects contracting institutions but not broad property rights institutions. Using these instruments to address the endogeneity of both institutional variables at the same time, they conclude that broad measures of property rights cause economic outcomes, but narrow measures of contracting institutions do not.

Is this the correct conclusion to reach from the data? In their paper, broad institutions are measured using the PRS index of expropriation risk or the Polity IV measure of constraint on the executive. Contracting institutions are measured by legal formalism—indices of the number of procedures needed to collect on a bounced check or

⁴ I choose the Acemoglu and Johnson paper as an illustration of what I believe is a more general point. I am not trying to pick on this particular paper. Indeed, it is only because they have been so careful in citing the sources for their data that the exercise I carry out here is possible.

to evict a derelict tenant from a rental property which were developed in Djankov et al (2003). The measure of expropriation risk clearly measures some combination of formal institutional factors and the informal institutional environment. As Glaser et al (2004) point out, the Polity IV measure of constraints on the executive often changes even without changes in formal constitutional constraints. Thus, the constraint on the executive should also be seen as measuring a combination of formal and informal institutions.

As the correlations on Table 2 indicate, the legal formalism indices developed in Djankov et al (2003) do not measure quite the same thing. The indices are based on a combination of measures on the number of steps involved in prosecuting a claim, the channels for appearing, and so forth. As discussed above, Djankov et al do show that formalism is a highly significant determinant of broader measures of the enforceability of contracts, but also that these broader measures of the ability to enforce contracts are affected by factors other than legal formalism, such as ethnic fractionalization. In this sense, legal formalism should be viewed primarily as a measure of formal institutional structure, while the broader measures of enforcement of contracts are combinations of formal and informal factors.

There are therefore two dimensions along which the measures of institutions used by Acemoglu and Johnson vary. The first is that expropriation risk is a measure of broad property rights while legal formalism is a measure of narrow contracting. The second is their measures of broad property rights measure both formal, constitutional, differences and informal institutional differences, while legal formalism is more purely a measure of formal institutions. There are also, then, two alternative interpretations for the finding that the property rights measures enter significantly in outcome regressions while the

contracting institutions do not. The first is that given by Acemoglu and Johnson, that broad institutions are more important than narrow institutions. The second is that informal institutions (and anything else that might be bundled into the impressionistic measures) are more important than formal institutions.

One way to differentiate these two interpretations is to replace the legal formalism measure with a broader measure of the functioning of the legal system. Table 3 shows results from an exercise which begins by reproducing (nearly) the regressions in Acemoglu and Johnson's Table 4.⁵ The first three columns of the table measures broad property rights with the index of expropriation risk, while the second three columns uses the index of constraints on the executive. Columns 1 and 4 reproduce the results from Acemoglu and Johnson. The second and fifth columns report OLS regressions replacing the legal formalism measure of eviction with the Heritage Foundation's Property Rights Index. The Heritage index measures the "efficiency within the judiciary, and the ability to enforce contracts." That is, it is a broader measure of the legal institutional environment, constructed in a manner similar to the expropriation risk index. Using the Heritage measure of contracting institutions, both property rights and contracting institutions are significant in OLS regressions. Moreover, the two instruments no longer separate in the first stage regression. The log of settler mortality enters significantly in both first stage regressions. In the second stage of the IV regression using expropriation risk to measure broad institutions (Column 3), neither institutional variable enters significantly. When

⁵ I have one fewer observation than Acemoglu and Johnson do when I use the legal formalism measure based on eviction. I have 5 more observations when I use the measure based on checks, and the results are further from those in the original paper. I therefore focus on the eviction measure, though the story is similar when the checks measure is used. The regressions use the average value of the Heritage index for the 1995-2004 period. However, the results are almost identical if the average over the period 1995-1999 and 2000 log GDP per capita are used instead.

constraint of the executive is used instead (Column 6), expropriation risk enters significantly and the contracting environment does not. But, we should interpret these results with much caution given the lack of separation of instruments in the first stage.

The results on Table 3 suggest that the Acemoglu and Johnson regressions might more appropriately be read as indicating that formal institutions have little effect on broad economic outcomes, while informal institutions have a more significant effect. This makes the results consistent with those found by Glaeser et al. They also follow a pattern in the empirical work in the literature: measures of institutions which are found to have significant effects on broad economic outcomes are almost without fail measures which are based on measures incorporating a mixture of formal and informal institutions.

If one accepts this interpretation, then the Acemoglu and Johnson results are consistent with those of Glaeser et al: formal institutions do not matter. Is there, then, any evidence that formal constitutional differences affect economic outcomes? The answer is at least a tentative yes. The best evidence in support of a formal institutional effect is provided by Persson and Tabellini (2003). They examine the effect of political institutions on two output measures—output per worker and total factor productivity. The most robust results come from their measures of district magnitude (having districts with multiple seats better) and the use of party lists (less is better). They also find that these same variables are significant when measures of corruption are used as the dependent variable. Less robust results show that presidential systems are associated with lower output per worker than parliamentary systems.

Acemoglu (2005) challenges the Persson and Tabellini results on several grounds. The most important of Acemoglu's criticisms relates to endogeneity issues. The issues in

this case is not so much reverse causation as missing variables. In Acemoglu's words: "political institutions are equilibrium outcomes, determined by various social factors that are not fully controlled for in the empirical models." Although Persson and Tabellini do present results in which political institutions are instrumented, the instruments are subject to some criticism. I won't repeat Acemoglu's critic here. But an example of one of the issues is the use of ethno linguistic fractionalization as an instrument. Ethno linguistic fractionalization is a measure which is certainly affected by the quality of a country's education system, which is itself a determinant of output per worker. This is not the only instrument which fails the exclusion restriction. In spite of these issues, however, Persson and Tabellini's work suggests that there may well be a connection between formal institutional measures and broad economic outcomes.

The work of Persson and Tebelinni and that of other researchers suggest that one specific channel through which political institutions affect economic outcomes is through their effect on the level of corruption. This argument has theoretical as well as empirical support. An early theoretical argument connecting political institutions to the level of rent seeking was made by Myerson (1995).⁶ In addition to the work of Persson and Tabellini, several other recent papers have focused on the link between political institutions and corruption. Most of these have used broad measures of the latter—either Transparency International's index of corruption perception or an index developed by Kaufmann, Kraay, and Zoido at the World Bank. They differ in their choice of right hand side variables. Gerring and Thacker (2004) focus on the presidential / parliamentary divide and the degree to which power in the country is centralized—a combination of the extent

⁶Persson, Tabellini and Trebbi (2003) and Kunicova and Rose-Ackerman (2005) review the theoretical literature.

of federalism and the structure of the national legislature, as described above. They find that higher degrees of centralization (less federalism) and parliamentary systems are associated with lower levels of corruption. Persson, Tabellini and Trebbi (2003) and Kunicova and Rose-Ackerman (2005) focus on electoral rules. Persson et al find that a higher percentage of single member districts and a larger percentage of legislators elected through party lists are both associated with higher levels of corruption. Kunicová and Rose-Ackerman (2005) use the Kaufmann, Kraay and Zoido measure of corruption and show that the results from these first two papers hold even when they are in regressions which control both for electoral rules similar to those use by Persson et al, and for federalism and presidential, as in Gerring and Thacker.⁷

Again, none of these papers address endogeneity concerns in a satisfactory manner, but they do connect empirical evidence with theory. At present this appears to be the most promising area for isolating the effects of specific formal institutions. The measures of corruption shown on Table 1 do appear to stand up to the Acemoglu-Johnson instruments.⁸ If in fact formal political institutions do affect the level of corruption, then this is an important step in identifying specific institutions which can be changed and which affect economic outcomes. There are other possibilities, of course. One is that corruption has two components, a growth-benign component which is affected by the formal institutional structure, and a growth hampering component that is not. In that case, even a change which reduced corruption might not have the expected effect on output.

⁷ Among these papers, only Persson et al discuss endogeneity issues. Reverse causation, which is clearly an issue when one consider the effect of corruption (or other institutional measures) on aggregate economic outcomes, is less clearly a problem in measuring the effect of political institutions on corruption. Concerns with endogeneity caused by unmeasured factors causing both the choice of political institutions and

⁸ They are highly significant in an IV regression of the form used in Table 4 of Acemoglu and Johnson (2005). Given the correlation between these measures an the measure of risk of expropriation, this is not surprising.

While measures which isolate formal institutions are beginning to appear with more frequency in the literature, measures which isolate informal institutions are much more challenging to develop. Most indicators of institutional quality which capture the enforcement of laws are based on impressionistic surveys of judges, lawyers or academics. These might reasonably be expected to be influenced both by formal laws and the efficiency with which those laws are enforced. Laeven and Woodruff (2005) use cross-regional data from a single country—Mexico—to isolate the impact of informal institutions. Mexico is an interesting case in this regard, because a single political party controlled all branches of state and federal government for a period of 70 years. Hence, the formal laws governing economic relationships were very homogeneous at the end of this period of one party rule. As a result, perceived differences in institutional quality reflect differences in the enforcement of laws rather than differences in laws themselves. Laeven and Woodruff show that higher quality informal institutions—instrumented with historical indigenous population and prevalence of high scale economy crops—are associated with larger firm sizes.

Section 5: Conclusions

The past ten years have produced many studies providing credible evidence that the quality of institutions has a causal effect on broad economic outcomes. But which institutions? And what are the policy changes which will improve the quality of the institutions which do matter? From a policy perspective, it is unfortunate that the strongest evidence we have relates to the impact of informal institutions on broad economic outcomes. Informal institutions result from particular equilibria largely

determined by history. As a result, they are likely to be the most difficult to change. One cannot propose that countries change their history, or relocated further from the equator.

The project of unbundling institutions is critically important to developing more practical policy prescriptions. This work has begun from two different perspectives. First, Acemoglu and Johnson have attempted to unbundle institutions by identifying distinct and separable instruments. Second, Persson, Tabellini and others have begun looking for specific political institutions, and specific channels through which those institutions affect outcomes. Neither of these lines of research has reached the point of providing definitive evidence. is critically important. As we have seen, the Acemoglu and Johnson unbundling is subject to alternative interpretations. And the political institutions to corruptions channel, while quite credible, is not yet convincing in its handling of endogeneity issues.

One of the striking features of the correlations shown on Tables 1 and 2 is that variables closely associated with English legal origin rarely stand up to instrumentation, while those associated with settler mortality and distance from the equator are much more likely to do so. This would suggest that parliamentarism is most likely to stand up to instrumenting. But finding a significant result for one of many measures of political institutions should leave us a little worried.

We need not all agree on a single correct measure of institutions to use, because the various indexes are measuring different phenomena. But we should realize that these measures are on the one hand measuring different forms of institutions, and on the other hand, correlated with one another. These two attributes makes the challenge of unbundling institutions quite difficult. Indeed, the challenge of identifying the impacts of

specific formal institutions on economic outcomes is a daunting one. Almost any cross country or cross regional study will be subject to the criticism that a measured institution is correlated with other measured or unmeasured institutions. We may quickly reach the limits of what can be said with this approach.

The alternative to the broad cross-regional study is to examine the effects of more focused policy changes. We are beginning to assemble evidence from “policy experiments” which show that, at the micro level, changes in formal structures do result in changes in economic outcomes (Reinikka and Svensson 2004; Olkun 2005). These micro studies may, when aggregated together, be able to provide guidance on which specific institutions have larger effects on the economy, and what the overall effect might be. The micro studies are also a source of evidence on how difficult combating corruption is. Both Di Tella and Scargrotsky (2003) and Yang (2005) examine cases where changes in incentives led not to the elimination of corruption, but to its diversion. But perhaps by replicating policy experiments in countries with different institutional environments, we will learn something more definitive about how the institutional environment affects incentives.

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	(1) Political Risk	(2) Const on exec	(3) Corrupt. Percept	(4) WB Corrupt	(5) BI corrupt	(6) Unitarism	(7) Parliament	(8) Prop rep	(9) Dist mag	(10) Judicial indep	(11) Const review	(12) Settler mortality	(13) English legal
Measures of Broad Institutions													
Political risk index													
Constraints on executive	0.37												
Corruptions perception index	0.76	0.55											
World Bank corruption index	0.74	0.52	0.94										
Business international corruption index	0.55	0.45	0.82	0.77									
Measures of Political Institutions													
Political unitarism	-0.33	-0.23	-0.04	-0.08	0.07								
Parliamentarism	0.43	0.54	0.54	0.57	0.32	-0.04							
Proportional representation	-0.21	0.4	0.2	0.15	0.18	0.05	0.08						
District magnitude	-0.23	0.26	0.22	0.16	0.19	0.13	0.11	0.83					
Judicial independence	0.31	0.15	0.23	0.25	0.23	0.06	0.29	-0.17	-0.18				
Constitutional review	-0.15	0.11	-0.16	-0.18	-0.12	-0.26	-0.07	-0.16	-0.13	0.07			
Instruments													
Log settler mortality	-0.65	-0.52	-0.71	-0.64	-0.55	0.34	-0.56	0.02	0.02	-0.21	0.15		
English legal origin	0.38	0.04	0.13	0.15	0	-0.17	0.22	-0.54	-0.56	0.49	-0.07	-0.32	
Distance from equator	0.49	0.51	0.81	0.81	0.72	0.03	0.52	0.18	0.15	0.24	-0.07	-0.56	-0.13

Notes: Correlations significant at the .01 level shown in bold; correlations significant at the .05 level shown in italics.

Sources by column: (1) Acemoglu and Johnson, 2005; (2) Polity IV data base; (3) Transparency International from Gerring and Thacker 2004; (4) Kaufmann, Kraay and Zoido; (5) Mauro 1995; (6) and (7) Gerring and Thacker, 2004; (8) and (9) Persson, Tabellini and Trebbi 2003; (10) and (11) La Porta et al 2004; (12) and (13) Acemoglu and Johnson 2005; (14) Levine, Loayza and Beck (2000).

TABLE 2
Correlations of Measures of Legal / Regulatory Institutions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	WB legal	Heritage	DB check	DB eviction	BI red tape	WB regulatory	DB employ	DB collective	WB corruption	Unitarism	Parliament	Prop rep	Dist mag	Judicial indep	Const review	Settler mortality	English legal
Measures of Legal/Regulatory Institutions																	
Heritage property rights	0.91																
Doing Business check index	-0.43	-0.47															
Doing Business eviction index	-0.34	-0.35	0.86														
BI red tape	0.76	0.74	-0.39	-0.34													
World Bank regulation	0.84	0.8	<i>-0.25</i>	<i>-0.13</i>	0.7												
Doing Business employment laws index	-0.43	-0.47	0.58	0.43	-0.43	-0.36											
Doing Business collective bargaining index	<i>-0.16</i>	<i>-0.22</i>	0.46	0.44	<i>-0.26</i>	<i>-0.19</i>	0.49										
WB corruption index	0.91	0.87	-0.47	-0.35	0.77	0.75	-0.45	<i>-0.14</i>									
Measures of Political Institutions																	
Political unitarism	<i>-0.08</i>	<i>-0.14</i>	<i>0.02</i>	<i>0.06</i>	<i>0.08</i>	<i>-0.03</i>	<i>0.04</i>	<i>-0.01</i>	<i>-0.08</i>								
Parliamentarism	0.59	0.59	-0.44	-0.31	<i>0.28</i>	0.46	-0.38	<i>-0.1</i>	0.57	<i>-0.04</i>							
Proportional representation	<i>0.13</i>	<i>0.05</i>	0.31	0.31	<i>0.09</i>	<i>0.21</i>	0.32	0.35	<i>0.15</i>	<i>0.05</i>	<i>0.08</i>						
District magnitude	<i>0.16</i>	<i>0.08</i>	0.32	0.32	<i>0.14</i>	0.3	0.33	<i>0.29</i>	<i>0.16</i>	<i>0.13</i>	<i>0.11</i>	0.83					
Judicial independence	<i>0.31</i>	0.38	-0.36	-0.38	<i>0.33</i>	<i>0.29</i>	-0.48	-0.35	<i>0.25</i>	<i>0.06</i>	<i>0.29</i>	<i>-0.17</i>	<i>-0.18</i>				
Constitutional review	<i>-0.01</i>	<i>0.02</i>	<i>0.16</i>	<i>0.13</i>	<i>-0.09</i>	<i>0.01</i>	<i>0.05</i>	<i>0.29</i>	<i>-0.18</i>	<i>-0.27</i>	<i>-0.07</i>	<i>-0.16</i>	<i>-0.13</i>	<i>0.07</i>			
Instruments																	
Log settler mortality	-0.66	-0.59	<i>0.34</i>	<i>0.24</i>	-0.59	-0.5	0.45	<i>0.19</i>	-0.63	<i>0.34</i>	-0.56	<i>0.02</i>	<i>0.02</i>	<i>-0.21</i>	<i>0.15</i>		
English legal origin	<i>0.17</i>	0.23	-0.57	-0.5	<i>0.08</i>	<i>0.13</i>	-0.59	-0.58	<i>0.15</i>	<i>-0.17</i>	<i>0.22</i>	-0.54	-0.56	0.49	<i>-0.07</i>	<i>-0.32</i>	
Distance from equator	0.84	0.74	-0.44	-0.32	0.64	0.64	<i>-0.28</i>	<i>-0.09</i>	0.81	<i>0.03</i>	0.52	<i>0.18</i>	<i>0.15</i>	<i>0.24</i>	<i>-0.07</i>	-0.56	<i>-0.13</i>

Notes: Correlations significant at the .01 level shown in bold; correlations significant at the .05 level shown in italics.

Sources by column: (1) and (6) World Bank Governance Indicators; (2) Heritage Foundation web site; (3) and (4) Djankov et al 2003; (5) Mauro 1995; (7) and (8) Botero et al 2004; (9) Kaufmann, Kraay and Zoido; (10) and (11) Gerring and Thacker, 2004; (12) and (13) Persson, Tabellini and Trebbi 2003; (14) and (15) La Porta et al 2004; (16) and (17) Acemoglu and Johnson 2005; (18) Levine, Loayza and Beck (2000).

TABLE 3
Regressions on log per capita income

	IV original	OLS	IV	IV original	OLS	IV
Average Protection against Risk of Expropriation	1.09 (0.21)	0.32 (0.10)	2.76 (2.44)			
Constraint on the Executive				0.75 (0.18)	0.21 (0.06)	0.80 (0.33)
Legal formalism (eviction measure)	0.39 (0.17)			0.05 (0.18)		
Heritage Foundation index of quality of legal system		-0.43 (3.00)	2.63 (3.23)		-0.57 (0.11)	0.13 (0.54)
First Stage Results						
Log settler mortality	-0.72 (0.15)		-0.72 (0.15)	-0.94 (0.21)		-0.94 (0.21)
English Legal Origin	0.39 (0.34)		0.39 (0.34)	0.39 (0.46)		0.39 (0.46)
Log settler mortality	0.18 (0.12)		0.48 (0.11)	0.13 (0.12)		0.46 (0.11)
English Legal Origin	-1.98 (0.27)		-0.54 (0.24)	-1.90 (0.26)		-0.50 (0.24)
Number of observations	42	42	42	41	41	41

Notes: Standard errors in parentheses. Bold indicates significance at the .10 level or higher.