The Perils of Empirical Work on Institutions

Comment

by

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1 Introduction

Empirical work on the effects of legal institutions on development and economic activity has been spectacularly successful over the past decade, if success is measured by the volume of journal articles and citations. If, in the alternative, success is measured by the extent to which research generates credible estimates of the causal effects of legal institutions on economic activity, this literature is doomed to fail. This failure is not due to a lack of skill or creativity on the part of the scholars examining these issues. Instead, it is endemic to the subject matter itself.

While modern microeconometrics has made great progress on the issue of causal inference during roughly the same time period that this empirical institutional literature has developed, its methods are simply not applicable in this area. For example, despite ACEMOGLU, JOHNSON, AND ROBINSON's [2001] extremely clever use of the mortality rates of European settlers as an instrument for institutional type in growth regressions, it is not possible to rule out the possibility that historical mortality rates affect growth rates through channels other than institutions.[1]

Similar concerns apply to the use of natural experiments since the kinds of shocks that are large enough to change legal and political institutions are likely to change many different kinds of unobservable social phenomena, leading to the strong possibility of omitted variables biases, and our theoretical models of the adoption and evolution of institutions are far too underspecified to allow for successful structural modeling. Panel data methods offer little hope, as well, given institutional inertia and the difficulty in controlling for time-varying national idiosyncrasies that are almost certainly affected by forces that also impact institutions.

With these empirical problems, it should come as no surprise when new papers enter the literature finding that relatively small changes to the coding of institutional

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[1] Although ACEMOGLU, JOHNSON, AND ROBINSON [2001] provide evidence that the instruments pass the test for overidentifying restrictions, the test requires that mortality rates, settlement patterns and early institutions are not all endogenous in similar ways. If they are, the test will indicate that the instruments satisfy the exclusion restriction even if they do not solve the endogeneity problem.

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variables lead to large changes in regression outcomes. If previous identification strategies "worked," measurement error should not lead to large changes in the coefficients of interest.

It is against this backdrop that SPAMANN [2010] enters the literature, finding that newly collected data on the complexity of civil procedure by country and the length of time it takes to collect a contracts judgment, as well as the costs associated with that collection, do not exhibit systematic differences between civil and common law countries. Perhaps even more surprisingly, he also shows there is no systematic difference in the level of procedural complexity involved in this legal scenario between civil and common law countries. These results draw into question the more general conclusions regarding the economic effects of legal institutions.

While Spamann's general conclusion is almost certainly correct, given the identification problems noted above, the results he presents are largely superfluous to making this point. There is little reason to believe that Spamann's results are any more or less credible than earlier results. When simultaneity problems exist, one is generally left with an empirical second best problem. Mitigating one issue, in this case improving a measurement error, does not necessarily move estimates closer to the true coefficient value and could worsen estimates. Short of strong identification strategies, we are largely left in the dark, and this needs to be kept in mind when formulating conclusions regarding the effects of legal institutions.

That said, empirical study of the effects of institutions is still an important endeavor, though it is one about which researchers need to remain appropriately humble. The best a researcher can do in this area is to present results as purely descriptive, abandoning the search for causality and strong empirical tests of hypotheses.

Along those lines, it is useful to examine the data with: (1) an eye toward robustness to high leverage observations, (2) effect heterogeneity, and (3) robustness to conditioning on other variables.

2 High Leverage Observations

Spamann notes in some of his results that Bhutan appears to exert a strong effect in the relationship between the cost of collecting a debt and the steps required to collect that debt (i.e., procedural complexity). Especially when survey results are used to determine covariate values with relatively little assurance that wording changes and their interaction with cultural context have been appropriately accounted for, one needs to worry about such effects driving results. This suggests that even more investigation of the influence of high leverage observations is necessary in a context like this. When various robust regression techniques are applied to the data used in Spamann, the model collapses, effectively implying that all observations are high leverage. This is a concern.

A related issue involves how to weight observations, if the leverage issue is ignored. While there is not much detail available on country-specific response rates to the World Bank's Doing Business data, one can reasonably presume that there

are differences in the available pool of respondents across countries. For example, while there may be many experienced corporate lawyers in the UK willing to answer the survey, there are likely to be fewer in Bhutan. Weighting of the observations in the regressions should account for this since it would imply that the various metrics are more precisely estimated for the UK observations than they are for the Bhutan observations. While clustering standard errors at the country level, which Spamann does, will mitigate this problem, it would be better to cluster and include sample weights. While some obvious candidates that may proxy for the sample weights (e.g., country population, GDP per capita) did not substantially change any of the reported results when used as weights, researchers in this area need to pay more attention to this issue when using survey results to construct institutional metrics.

3 Effect Heterogeneity

Examining Spamann's first difference regressions from Table 2 Panel B (focusing on specifications 2 and 4, though the point generalizes to the other specifications as well), if we allow for a different effect of procedural complexity between countries with civil and common law origins, we find interesting and potentially important differences. As shown in Table 1 below, the positive effect of complexity on length of time to collect is driven by civil law countries with the civil law complexity coefficient three times as large as its common law counterpart (statistically significant difference at the 10% level).

While the difference is not statistically significant in the cost regressions, it is interesting to note that while the common law complexity coefficient has a negative

Table 1
Relationship between Procedural Complexity and Legal Expedience

	ln(days)	ln(cost)	ln(cost)
	(2)	(4)	(5)
ln(steps) × civil origin	1.77***	0.30	0.51**
	(0.58)	(0.30)	(0.21)
ln(steps) × common origin	0.51	−0.04	−0.00
	(0.43)	(0.08)	(0.01)
ln(GDP pc)	−0.01	−0.22	−0.02
	(0.05)	(0.21)	(0.04)
Constant	−0.01*	−0.00	−0.00
	(0.00)	(0.00)	(0.00)

Source: SPAMANN [2010]: Table 2, Panel B: Regressions Allowing for Effect Heterogeneity.

sign, the civil law complexity coefficient has a positive sign. If Bhutan is omitted in the cost regressions, the difference between the civil and common law complexity coefficients is statistically significant at the 5% level. While the lack of a strong identification strategy limits the inferences we can draw, there does appear to be a difference between civil and common law countries as a descriptive matter when it comes to the relationship between procedural complexity and legal expedience.

4 Robustness to Other Specifications

Examining other specifications provides a more detailed description of the experience of countries coming from different legal traditions that may raise important questions for qualitative study. For example, if we examine the trends in procedural complexity in Spamann's data, allowing for separate common year effects to account for unknown differences in sampling methods and survey design from year to year, we find that procedural complexity has been growing in common law countries relative to civil law countries, as has the length and cost associated with collecting the judgment, though the effect in the length regression is not statistically significant. Specifically, if we examine first difference regressions of procedural complexity, days to collect the judgment, and the cost of collecting the judgment (omitting Bhutan as Spamann identifies it as a high leverage observation in the cost regressions) on log per capita GDP and an indicator variable for common law origin, also allowing for common year effects to account for year-to-year differences in survey design and sampling methods, we find that complexity, duration, and cost have all been growing in the common law countries relative to the civil law countries, though the duration result is not statistically significant. These results are presented in Table 2.

There is no way to know if this specification is the "correct" one in any sense, but the fact that common law countries appear to be experiencing upward trends in these

Table 2
Growth in Procedural Complexity and Legal Expedience: First Difference Equations
(standard errors clustered on country)

	ln(steps)	ln(days)	ln(cost)
Common law origin	0.002**	0.004	0.003*
	(0.001)	(0.004)	(0.002)
ln(GDP pc)	−0.013	−0.028	−0.020
	(0.015)	(0.053)	(0.037)
Constant	−0.004***	−0.011**	−0.001
	(0.002)	(0.005)	(0.002)

Notes: Regressions use Spamann data and include year fixed effects. The cost regression omits observations for Bhutan. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

variables relative to civil law countries on average might bear some investigation. Other specifications might generate other interesting questions.

In conclusion, the empirical study of the effects of legal institutions should be approached as a descriptive exercise because we do not have strong identification strategies available in this area to evaluate hypotheses about the causal effects of institutions on outcomes. However, detailed descriptive analyses have the potential to spur important questions.

References

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