

Discussion

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Introduction

It is a pleasure to discuss this paper on regulatory governance by my colleagues at the International Monetary Fund (IMF)—an institution that, together with the Bank for International Settlements (BIS), has done so much in recent years to improve the so-called international financial architecture.

In the spirit of leading by example and good governance, and in this age of conflicts of interest, let me be completely transparent: the authors are obviously preaching to the converted here! I am absolutely convinced that standards and codes are the way to go in improving the global financial infrastructure, broadly defined. And I am absolutely convinced that good regulatory governance is an important element of that infrastructure. In other words, I am absolutely convinced that the paper's key conclusion is correct!

This puts me in an awkward position, however. How can I play the role of devil's advocate, normally expected from a discussant, when I agree one hundred per cent with the paper's message? I thought, therefore, that I would ask a different question: Would the paper persuade the skeptics? The answer is not obvious. Perhaps the unconvinced will remain so. Their convictions are unlikely to be fundamentally shaken, although they could well become a bit frayed at the edges.

I hasten to add that this is not the fault of the authors. I enjoyed reading their paper and I think they have done a fine job, given their serious data limitations. Rather, any difficulty is a reflection of the size of the challenge they have set for themselves.

The structure of my remarks is the following. I will first recall what the authors do. I will then briefly describe their methodology, namely, cross-country, cross-section regressions. I will go on to raise questions about the tests used before offering conclusions.

The Gist of the Paper

What, then, do the authors do? In the introductory sections, they note the importance of standards and codes and the prominent role played by the IMF in monitoring/helping to enforce them (plus, occasionally, in their development, as in the case of the transparency code for financial and monetary authorities). I have no comments on these sections, other than to state that I agree with the authors.

Das, Quintyn, and Chenard then test the hypothesis that better regulatory governance is conducive to financial stability (financial system soundness). Because of lack of data, they focus only on the banking sector. They develop indexes of financial system soundness and of the quality of regulatory governance. They carry out cross-sectional, cross-country regressions controlling for other factors (macroeconomic stability, presence of foreign banks, the quality of public sector governance). I will focus my remarks on this part of the paper.

The authors make at least two key findings. First, the quality of regulatory governance has a statistically significant effect on financial system soundness, even after other factors are controlled for. Second, the quality of regulatory governance matters even if one simply uses the predicted value from a regression on public sector governance, which the authors interpret as a correction for endogeneity.

General Methodology: A Skeptical Remark

I approach these types of statistical exercise with ambivalence. On the one hand, it is no doubt important to try to find good empirical evidence concerning claims about the significance of codes and standards in enhancing financial system soundness. On the other hand, I cannot help thinking that these econometric, cross-country exercises are not necessarily that convincing. Both the dependent and independent variables are hard to measure. The countries are often too heterogeneous, and controlling for the relevant factors is very difficult. Moreover, matters are made worse, as the authors recognize, by the lack of a theory to guide the construction of the variables and tests. The reader is typically left with the suspicion that the results could be overturned if one had only tried hard enough. This reminds me of Bhagwati's critique of Dani Rodick's trenchant objections to cross-

country analyses of the impact of globalization on economic growth, etc. Bhagwati observes that detailed case studies could be more convincing. I agree.

Having said that, I recognize that this is too general and not particularly constructive. Let me therefore turn to the specifics of the tests to illustrate some of these points and suggest possible improvements.

The Tests

I would like to raise five issues regarding the specific tests: the definition of the dependent variable; the definition of the independent variable; the control variables; the time frame; and the question of endogeneity.

Definition of the dependent variable

The first point about the dependent variable—the financial system soundness index—is that some of the variables included in its construction may be too backward-looking. It is, of course, obvious that soundness is very difficult to measure—I have considerable sympathy for the authors, having grappled with similar issues myself. At the same time, considerable empirical evidence indicates that, by themselves, non-performing loans (NPLs) and bank capital have relatively little information content about *future* potential problems (i.e., for existing vulnerabilities). Moreover, the regressions are based only on recent observations. Thus, they are more likely to tell us something about the past and current conditions of banks than about future prospects, given the current degree of observance of the standards. If the authors had the relevant data, it would be helpful if they showed us the past correlation in their sample between banking problems and lagged NPLs or measures of bank capital. But I suspect they may not have sufficient data.

A second point is that the measurement of the dependent variable is not independent of the . . . independent variable. In particular, the authors include measures of the quality of supervision in the construction of the financial system soundness index. But this seems to assume that the underlying hypothesis being tested is true, i.e., that better supervision improves financial stability. If it did not, the observance of regulatory governance standards would be of no help. A skeptic would not uncritically accept this premise. Moreover, to what extent is the judgment of the quality of supervision itself reached independently of regulatory governance

aspects?¹ A better strategy might be to include measures of the quality of supervision (ideally reached independently of regulatory governance aspects) as an independent variable, and then determine whether better regulatory governance and better supervision, possibly interacted, can help to explain the cross-country dispersion in different measures of financial system soundness.

Finally, the method behind the aggregation of the index (“neutralization of correlations”) is not transparent: the authors may wish to expand on this. I had difficulty understanding the logic of the calculation. More generally, without a theory, it is hard to see how aggregation could best be done. One possibility might be on the basis of the degree of correlation with episodes of banking distress. But, as noted, I suspect that data limitations might prevent this.

Overall, for robustness, it would seem useful to test the hypothesis based on alternative measures of financial system soundness, even at the cost of dropping some observations. The Moody’s index, for example, could be one possibility.

Definition of the independent variable

Similar difficulties relate to the construction of the regulatory governance index—the index for the key independent variable. In the absence of a theory, it is difficult to establish weights. To limit misspecification, however, one may wish to use alternative specifications and to rely on more disaggregation.² One would expect some dimensions of regulatory governance to matter more than others for good enforcement. For instance, in my view, independence may well be more important than transparency per se. And here I would like to draw attention to some thoughtful work that one of the authors of the paper has recently undertaken, drawing attention precisely to this aspect of regulatory governance (Quintyn and Taylor 2002).

1. For instance, one might expect that the better the regulatory governance, the more accurate and unbiased the measurement of capital and NPLs. The “optimistic” interpretation is that this correlation would bias downwards the coefficient on the regulatory governance index (the weaker the regulatory governance index, the higher the “measured” financial system soundness). This would work against the point the authors want to show and hence actually strengthen their case. The more “critical” interpretation is that the dependent variable can be of very low and uncertain quality. Of course, the very different cross-country definitions of NPLs and accounting standards further complicate the picture.

2. A similar point could be made about another of the independent variables included in the analysis, namely, the public sector governance index.

Controls in the regression: adequate?

Three points merit attention regarding the control variables. First, macroeconomic stability may not be well captured by inflation and fiscal deficits alone. These variables better reflect the “public sector crises” of the past. But evidence suggests that private sector crises may be more common in the current environment of liberalized financial markets and anti-inflation credibility; at least they have been more common since the liberalization efforts of the 1980–90s (e.g., Borio (2003) and Borio and White (2004)). This has been the case, for instance, for the banking crises observed in Nordic countries, Japan, and East Asia. Taking the left-hand-side variables as given, what we would like to see is whether in countries with inferior regulatory governance there is “excess sensitivity” to those economic fluctuations that tend to result in financial distress. It would appear appropriate to control for real economic activity (contemporaneous with the financial system soundness index) as well as for booms/busts in credit/asset prices—variables that in previous work have been found to have considerable predictive power for banking crises (e.g., Kaminsky and Reinhart (1999) and Borio and Lowe (2002)).

Second, the share of foreign banks per se is not easy to interpret. For instance, how should one do so when talking about industrial countries of similar degree of development, such as the United States and Europe? If possible, it would make sense to control for the relative quality of foreign banks. I wonder whether, based partly on the BIS international banking statistics, it would be possible to take this aspect into account.

Finally, there is an issue about the need to set priorities in codes and standards. This is of paramount importance in emerging-market countries, given initial conditions and limited resources. The paper analyzes only regulatory governance, but codes and standards are much richer. Even the “core principles” for banking supervision are much more extensive than “transparency” and “good governance” alone. While there may be a sense in which good governance is more basic, this is not so clear. The authors do not test for this in the paper. Of course, this goes well beyond the scope of the exercise, but it might be helpful to have a sense of what the authors think about the relative importance of the various codes and standards.

Time frame: adequate?

The issue of the time frame covered by the test is critical. The acid test of the soundness of a financial system is the frequency and severity of financial distress. But financial distress is, by its very nature, a rather rare event, not occurring in every business cycle. Assuming that the quality of regulatory

governance is constant over time, a proper test would require a period long enough to observe vulnerability to problems. And if the quality of governance changes over time, such as a result of reforms, one would need time to observe the impact of those changes and to allow for this explicitly in the regressions. Neither can be done based on the two to three years of recent data to which the authors are inevitably restricted, years in which the reforms have also tended to cluster. Unfortunately, there is nothing that the authors can do about this.

Endogeneity

As regards endogeneity, I think the authors are actually too harsh with themselves. It is not entirely clear to me what the issue really is. I think of endogeneity, loosely, as reverse causation. For instance, the regressions would suffer from this problem if “because” of greater financial system soundness, one could “afford” better regulatory governance.³ But it is not easy to think of circumstances in which this would be a major problem. Nor is this what the authors seem to have in mind. They appear to mean that a high value of the regulatory governance index may be due to good public sector governance. If so, one could easily ascertain whether regulatory governance has an effect over and above public sector governance. If one wished to have a kind of lower bound for this effect, one could use the predicted value of the regulatory governance index based on the public governance index (which the authors do) and then allow for a separate effect of the regulatory governance index, e.g., include the regulatory governance index minus the regulatory governance index (predicted) as an independent variable.

Conclusions

Let me summarize. I agree fully with the importance of the issue and with the authors’ conclusions. The authors should be commended for a fine job at identifying an important question and for taking a first pass at it, given the serious data limitations they inevitably faced. I do have some reservations about the overall approach: I think there are steps that the authors could take to make their results more convincing, and I have suggested a few. However, inveterate skeptics will probably be waiting for . . . more weighty evidence. But then, I can assure my fellow travellers in the journey towards a stronger international financial architecture that skeptics will likely remain so. And writing for them may be setting the bar too high!

3. Excluding, of course, the measurement issues raised above concerning the dependent variable and its relationship with the independent variables.

References

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