Discussion of "Credit, Asset Prices, and Financial Stress"*

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Let me start with praise for the paper and also introduce the structure of my comments. The paper by Misina and Tkacz is part of recent, broader literature investigating how asset prices and other financial variables can help document and predict financial stress and financial distress. Clearly, since we have faced the largest financial crisis in advanced countries since the Great Depression, nobody needs to be reminded how important this type of research is. It would have been extremely useful for policymakers to have an objective tool on the basis of which they could have anticipated at least some of the turmoil over the past year. This would also have been helpful for private market participants, since that could have provided a check on the excessive risk taking that went on in many financial markets.

In this context, the paper is a very useful addition to the literature on financial stress indexes (FSIs) and their counterpart—the financial condition indexes (FCIs)—which try to capture respectively the stress and buoyancy of financial markets. It is, by the way, remarkable how the Bank of Canada has been at the forefront of the development of financial stress indexes, first with the paper by Illing and Liu (2006) and now with this paper. Given that Canada seems to be one of the few countries that have largely escaped the global financial crisis, it is tempting to attribute this to development and application of FSIs in policymaking. It will be hard to provide evidence whether this is the case, but I think it would be a great example of the Lucas critique working to the benefit of financial stability.

My comments will start with a review of what FSIs are. It is useful to go through the development of FSIs and their analytical bases since it will help set out the major questions this literature in general is trying to answer. It will also explain how this paper specifically fits in with these questions. I will then review the paper and suggest some improvements in terms of data, offer some suggestions on the methodology, and hint at some possible extensions.

But let me first review the main result. Basically, the authors find that credit and asset prices help predict movements in an FSI specifically developed for Canada. Since others, notably Illing and Liu (2006), have shown that movements in this FSI are related to specific, financially stressful events in Canada, they can thus conclude that credit and asset prices help predict financial stress. They also show that this same relationship holds for other countries, confirming results by others that FSIs help predict financial stress. This in turn suggests that authorities need to consider these indicators in policymaking. Presumably, this would be in a variety of areas: monetary policy; the design and conduct of regulation and supervision; fiscal policy, both tax policy and overall fiscal management; and possibly some other policy areas as well. Here, by the way, the paper does not go into as much detail as I would have liked, but at least we now have a basis for policymaking.

The main results of the paper, I would say, are new and yet reassuring, but also disappointing. They are reassuring in that credit and asset-price variables help improve with predicting FSIs. This confirms the results of others that have shown, in individual country and cross-country work, that credit growth and asset price increases can predict financial stress situations (starting with Borio and Lowe 2002). It also relates to work that has shown that elevated FSIs, in combination with prior credit and asset price growth, have higher economic costs in the aftermath of financial crises (International Monetary Fund 2008). As noted, the paper is also reassuring in that results apply, besides to Canada, to the United States and Japan.

At the same time, the results are somewhat disappointing. For one, adding credit and asset variables still leaves large errors in the ability to predict financial stress. In particular, the predicted FSI does poorly in anticipating the 2007-08 crisis. The work—and this applies not just to this paper but to the broader literature as well— is also less satisfying in that we still know little about the channels

for financial stress. In essence, we find out that "a bust is preceded by a bubble in credit and asset prices," but this is something we had known for some time (Borio and Lowe 2002 document this clearly, but Keynes, Kindleberger 1996, and many others had alerted us to it before). At this point, we need more specificity to avoid these large financial crises. Policymakers and market participants need to know better how these events come about, as that will help them develop specific policy and investment implications.

It is for this reason worthwhile to go back to the objectives of this literature. The story pursued in these papers is well known: financial stress can lead to financial crises which have adverse impacts on the real economy. Of course, there are many feedback loops here and one needs to consider endogeneity and simultaneity, so cause and effects are likely mixed up in this literature. Nevertheless, the objective is to construct FSIs that help predict financial distress, or at least explain distress in the sense of finding that an elevated FSI is correlated with financial stress ex post. This was done for Canada first and around the same time by some private-sector participants, and is now a little cottage industry. The objective of the paper is to build on this literature and to try to predict the Canadian FSI using lagged FSI, financial, and other variables, including macroeconomic variables. This way, one can anticipate financial stress periods better.

But this raises the question, why not relate a bunch of variables directly to financial stress or distress? Choosing between the two alternative approaches—predicting the FSI and thereby financial stress or directly trying to predict financial stress—involves some trade-offs. There are some reasons why one would not want to relate a bunch of variables directly to financial distress or the buildup of vulnerabilities. First, there is the timeliness of available data: financial data, especially prices, are much more readily available than data on the real economy. Constructing an FSI thus leads to a much timelier indicator than building a model that predicts distress on the basis of a bunch of real and financial variables. Second, and related, there are often too few episodes of financial distress. Canada, for example, did not experience a major financial crisis in recent decades, not even in the current period. Also, when using the approach of directly trying to predict distress or the buildup of vulnerabilities, one cannot analyze crises that are prevented. Yet it

would be useful to know about these prevented crises as well, which financial market indicators may have signaled.

One neither may want to relate explanatory variables directly to economic outcomes. That way, one learns little about the channels through which the crisis came about: was it indeed the financial sector or was it something else? Furthermore, financial stability is an objective in itself: from theory, but also from empirical evidence, we know the importance of a well-functioning financial sector for overall development. On balance, there are thus very good reasons to use the approach pursued here of predicting the FSI using its own and other variables.

This then raises the question of how best to create FSIs/FCIs. Most of the development has been done on the basis of some related theory and a mixture of priors and ad hoc reasoning. But it also has involved tests of the ability to explain or predict financial stress or crises. Here, the variables or weights may be chosen on the basis of the fewest missed stress periods. And some approaches, as noted, have involved the ability to explain or predict real economic outcomes. Note the differences here between *explain* and *predict*; in-sample explaining involves, of course, a lower statistical threshold. The various FSIs are derived using a mixture of these objectives and corresponding methodologies—and are thus difficult to compare, but most fall in the explaining category.

The FSI construction raises many methodological choices: there are numerous variables to choose from (one can use fixed, optimally derived, or time-varying weights for each of the variables included in the index), there are different econometric methods to use, etc. In the particular case here, the choice was based on the ability to classify (not predict) correctly "expert-identified" periods of financial stress in Canada. The variables were chosen on the basis of a combined lowest type I and II errors. The nine variables used are mostly asset prices from Canadian capital markets. The index uses fixed weights, based on the "credit" shares of the financial markets' subsegment that the price variable represents in overall financial intermediation in Canada.

In terms of data, my comment would be that while many indicators have already been included, one could consider some additions or changes. For one, the Canadian system is very bank oriented, yet most indicators used in the FSI are capital market

oriented. The logical suggestion would be to focus more on banking and less on capital market conditions in both constructing the FSI and in selecting the explanatory variables that can help predict the FSI. In the FSI, for example, one could add all sorts of bank interest rates—short- and long-term debt, CD/interbank, subdebt, or preferred shares—whichever are more economically and financially important for Canada. In the set of explanatory variables used to predict the FSI, one could add bank data, such as capital ratios, liquidity positions, funding gap, credit growth, interest rate margins, etc. Timelines may be a constraint for some of these variables, yet there are nevertheless some that can be used and that fit closer the heavily banking-dominated system in Canada.

My second comment on the data is to acknowledge that Canada is closely integrated with the global financial market and economy. Canadian corporations finance themselves abroad; at least they were able to do so before the crisis. Canadian markets are also very interlinked and correlated with other markets, especially with the U.S. capital and financial markets. And Canada is dependent on global economic conditions through its trade. The implication would be that financial stress globally, and in the United States especially, is important for Canada's financial sector and real economy, maybe more so than domestic stress is. This in turn means one should expand the list of U.S. variables on the right-hand side of the regressions. For example, one can try to predict the Canadian FSI from a U.S. FSI and then try to explain the residuals, using other Canadian and U.S. data.

Other suggestions related to the data are to use the subindexes of the existing FSI. This breakdown would give a measure of the relative contribution of the banking-system data versus capital market data: this would be worth studying since it would help in getting at the channels by which stress comes about. Also, others creating FSIs have included data on volatility (such as the U.S. S&P 500 volatility index, VIX) and high-yield, TED, and other spread measures. These could be easily added here.

There are also some surprising results that could be explored more. For example, the Japanese and Australian equity indexes are found to be significant in predicting the Canadian FSI. A direct link is not obvious, however, from Australia or Japan to financial stress in Canada. It rather suggests that these equity indexes are proxies

for similarity in commodity exports (as in the case of Australia), the general state of global demand (perhaps the Japan index), or something else. It would thus be useful to try to find other indexes that can show these links more directly. There is also the issue of ex post data revisions, which is now ignored. The paper could adjust for this by going back through the announcements of the data at the time or by adjusting for the proper lag structure. And finally, the data stop before the 2007–08 financial crisis. Of course, it is tempting to ask the authors to extend the analysis fully to cover the current financial crisis.

In terms of empirical methodology, the paper tests many models, and it seems robust in its findings. It includes an endogenous threshold, which is a nice innovation. My main concern is that these approaches, as is common to the literature, suffer from data mining. This is especially a problem for these studies since there are few financial crises or stress periods. The small sample size (fewer than fifty observations) in general is a statistical problem—unfortunately, not easily solvable—and the paper acknowledges this openly.

There are also other choices to be made. For criteria, one can choose (just) to minimize the mean-squared errors (MSEs). But one could also weigh type I versus II errors to account for the fact that a missed financial crisis is "worse" than having "overpredicted" financial crises. In this context, sign tests could be useful since negative errors may be less severe than positive errors. Also, one can try to base the forecasting quality on cumulative abnormal errors; that is, when does the model predict the FSI to be out of line for more than just that single period? Lastly, one can try to weigh the accuracy of the forecast by the impact of the financial stress or crisis predicted.

More generally for this literature, not necessarily for this paper, I would suggest some alternative tests. For example, one can use a signal approach instead of regressions (as this paper does). It is somewhat a matter of preferences, but I am struck by how much use is made of the signal approach ("raising a number of red flags") in international financial institutions and presumably elsewhere. This suggests that for policymakers, the signal approach is easier to communicate, which in turn matters for policy impact. Then there are a variety of other econometric techniques that can be used. For example, I think quantile regressions has a lot to offer since it might allow one to better capture the tails that are of most concern.

Lastly, and most importantly, the literature can benefit from more guidance through formal modeling. This is a new field, so robust models are just coming on line, but there are some models and empirical work already that have specific suggestions for what matters in terms of financial conditions. For example, the recent models on financial intermediation using frictions (Adrian and Shin 2008; Brunnermeier and Pedersen 2009) suggest that leverage and liquidity ought to be used in forecasting models. Insights from the asset pricing literature suggest that one ought to use (principal components from) interest rate yield-curve data, since those help predict macroeconomic and financial events (for example, Ang, Piazzesi, and Wei 2006). Perhaps futures for some asset, commodity, and other prices ought to be used as well, as they can have predictive power. These more formal approaches may help design better machinery and models that help to identify not just stress, but also risks.

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