Comments on: Channels and determinants of foreign bank lending

Jenny Corbett (ANU)

This paper is, in effect, two in one: a descriptive paper and an analytical one. Each part is interesting in its own right but it is not entirely clear how the parts relate to each other from the paper as it is presented. My own interpretation of the authors' intended link is presented in concluding remarks below.

The first, descriptive, part asks who borrows and lends, and in what form, in the Asia-Pacific region. The underlying question is whether borrower countries in the Asia-Pacific region have become more vulnerable to potential "sudden stops" in credit flows from foreign banks. The authors are fairly sanguine on this front: despite some concentration of lenders after the withdrawal of European banks, the region is not seriously exposed to "common lender risk". Banks in the region are not heavily reliant on non-core liabilities and, although the proportion of foreign lending accounted for by local affiliates has fallen somewhat, the overall share of foreign banks in total credit in the region remains small.

In the second part, on the drivers of different channels of cross-border lending, the authors focus their econometric model on what influences the share of cross-border versus local lending, comparing the effects of trade flows and standard gravity factors with the role of banking system factors. Here the concern is with what gives borrowers access to "safer" local lending rather than more "volatile" cross-border financing. The authors conclude that the local lending channel is more prevalent where borrower systems are more fragile. They suggest that this provides countercyclical, ie stabilising, credit for weaker systems.

Why is it interesting to consider these questions in the context of Asia? There are several reasons. The authors note that much of the work on contagion of bank shocks has been done for Europe, leaving a gap in the research. As they also point out, the region provides a lens on how the international banking system might change because the region's financial system is developing rapidly from a low level of exposure to (or reliance on) cross-border claims.[1] Furthermore, banks headquartered in the region are beginning to lend across borders themselves. There are other reasons why Asia is interesting. Why wasn't the impact of the global financial shock bigger in Asia? Why was it so short-lived? Why was it felt mainly in trade and not in the financial system? Is the financial system more, or less, vulnerable to shocks now? Does involvement of "foreign" banks help offset risks or increase them? The paper provides answers to some but not all of these, which leaves avenues for further research with the valuable data they have compiled.

There are several insights in the first, descriptive, part which owe a considerable amount to the authors' detailed understanding of the structure of the BIS data. The first is to clarify the various ways in which loans can be made, distinguishing

[1] They use the term financial integration for the measure of cross-border lending to GNP but this is a contested terminology, which some authors prefer to avoid.

between cross-border loans by international banks direct to non-banks (companies or individual) in the borrower company and cross-border loans by international banks to local banks, which then on-lend to local borrowers. Foreign banks may also lend via their affiliates inside the borrower country resulting in "foreign" lending that is not cross-border, which the authors call "local lending". Given the importance of this distinction, the diagram of the different channels used in the conference presentation would be a useful addition to the paper.

The descriptive data show increased concentration of lenders and provide some evidence of several common routes for the transmission of shocks: common lender effects, interbank funding exposures and lending by local subsidiaries of foreign banks (considered less volatile than other forms). The paper concludes that the risk of common lender effects seems to have decreased despite a growth in the share of the top three lenders as European banks have withdrawn. This is an important, and new, element of the paper but receives less analytical attention than it might.

The calculation of the "common creditor index" is interesting but captures only one element of the exposure of the region. The CCI measures the commonality of (third-party) creditors between pairs of countries, and we are shown the average value of country-pair CCIs over time, whether particular pairs of countries have more common creditors over time and whether the distribution of the CCI of pairs of countries has widened or narrowed. What question does this allow us to answer for the region? It only indirectly tells us whether the region as a whole has a higher concentration of common creditors because the focus remains at the country-pair level. It allows (with some effort) an understanding of the likelihood that a shock affecting one (or a particular group) of the lender banks which are important to one country might also impact other countries. We can, for example, identify that a shock affecting the creditors to Chinese Taipei has a higher probability of also affecting credit to China, Hong Kong SAR, Indonesia, India, Japan, Korea, the Philippines and Singapore than of impacting on credit to Bangladesh, either because the former have more creditors in common with Chinese Taipei than the latter or because those they share account for a large proportion of their foreign borrowing. This is one part of the picture of common lender effects but here the authors could usefully delve more deeply to think about measures that capture the full network effects of inter-related lending around the region. What we really want to know is whether a shock to a particular set of international lenders will affect many or few countries in the region and to what degree.

The data on interbank funding and local lending patterns provide a useful segue to the second part of the paper, which asks what affects the share of each lending channel in the total. Starting from the proposition (backed by some evidence in the literature but not directly examined here) that "local" lending by foreign creditors (particularly via subsidiaries rather than branches) is more stable than cross-border lending, the second part unpacks what determines the respective shares.

This part of the paper is initially somewhat difficult to follow because the estimated equations are not set out explicitly and there are some issues of the approach that could be clearer.

Section 3.1 presents a definition of total foreign claims divided into the two components of cross-border claims and "local" claims. Each has a share in the total. The authors state that the paper will *estimate* the shares and will examine what factors determine the share of each component. Readers may be distracted at this point by recognising that the shares can be calculated from the data (so why estimate them?) and that they should necessarily add up to one (so, once we know one, we will automatically know the other). What precisely is going on here?

What the authors do, in fact, is to express the identity in terms of rates of change (the growth of total foreign claims is equal to the weighted sum of the two channels, cross-border and local), recognise that there is some noise in the data so that an error term enters[2] and argue that the estimated coefficients representing the shares are not constant across time and geographical space. Given this latter point, a standard regression of the growth in total foreign claims on the growth in values of the different types of claim, delivering a pair of coefficients representing the shares, would be misleading. However such a standard analysis is useful as a benchmark.

They establish that benchmark by regressing the growth of total foreign claims on growth in cross-border and local claims. This explains about 74% of the variance of the growth in the total value and generates single coefficients on each channel that would represent the share of each under the assumption of fixed coefficients. Next they argue that the way to capture the effects of various factors determining different shares for each channel across time and across countries, is to regress the growth of total claims on multiple variables interacted with each of the two channels. This is their method for capturing the effect of multiple underlying variables on the share coefficients for each channel (as they describe in the equations of Section 3.1). They assess how significant these various factors are by comparing the proportion of unexplained variance in regressions with different sets of variables included (ie comparing R^2).

Their results are suggestive but some methodological concerns urge caution in interpretation. First is the question of how much the variation in the share of local claims matters and how best to understand it. We know from Table 3 that the variation in shares of local claims is wide but we don't know the distribution of the variation so it's hard to judge how important it is to explain the differences in shares (do countries cluster around the mean?). And if the question of interest is to explain cross-country (and over time) variation in the share of local claims, then using panel data methods directly on a model of the local claims share would be informative and arguably more obvious,[3] and would give more direct ways to interpret the coefficients contributing to differences between countries.

Second is the problem of sample selection bias. The authors exclude observations in which the creditor country does not do both cross-border and local lending. Their justification is flexibility but the potential sample selection bias from excluding those with no local lending cannot simply be ignored. Creditors' choice about whether or not to establish a local subsidiary is highly likely to be based on local banking characteristics. The impact of those same local characteristics on the choice between local and cross-border lending for those banks that have made a

2 Which explains why the estimating equation is not an identity and why the adding up constraint is not applied.

3 They dismiss a similar method used by Garcia-Herrero and Peria arguing that it imposes an adding up constraint but that would not be necessary if each share were separately used as the dependent variable.

prior decision to go in to a market is likely to be different from the overall impact of those characteristics on cross-border lending.

Third is the method for determining the significance of groups of factors. The authors use comparisons of R^2 to determine which groups of variables are most important but this is less robust than using a full general-to-specific method in which all variables are first included and then systematically excluded on the basis of any of a number of information criteria tests. This matters because the authors draw inferences from the size of the coefficients (eg "trade factors are more relevant for explaining the share of locally intermediated claims than they are for explaining the share of cross-border claims"). They also rely on the change in coefficients on some factors when other factors are added ("when banking system-specific factors are introduced, trade factors are generally insignificant in explaining cross-border claims"). Indeed, they note in footnote 21 that, when all factors are included, some interaction terms become insignificant. Each of these statements suggests that exactly which factors are retained does have an important impact on both the size and significance of other factors. In these circumstances, the only way to reliably draw inference is to systematically exclude factors until the parsimonious model is identified. This also impacts on the test they use to determine which are the most important factors within groups of variables (Table 6). If the size and variance of the estimated coefficients are unreliable because they are not derived from a parsimonious model, then the calculation of their contribution to total variance will also be affected.

Finally there is a matter of terminology. Even if the conclusions on the importance of different factors were sustained under more robust estimation, I would still take issue with the language used in interpretation. The banking factors that have been used show not the stability of a system but its health. These terms are used interchangeably by the authors but should not be confused. High ratios of impaired loans and other indicators of poor banking health do indicate fragility but implications for the *stability* of the system do not automatically follow. There may be no link between poor health and the *variability* of credit for example, and much will depend on policy responses to deteriorating bank health. This point does not undermine the conclusions the authors are trying to draw about the role of local lending in borrower countries with weak banking systems, but it does reduce the clarity of their message.

Conclusions

It must be borne in mind that the authors begin with a view (not a hypothesis that is tested) that "decentralised" banking (ie local lending with local funding) is more stable in times of crisis than cross-border lending. The paper is not testing this for the region (which would be another valuable exercise) but is trying to understand what factors give rise to more decentralised (local) lending by foreign creditors, on the assumption that it is a desirable outcome. This is where the disconnect between the first part of the paper and the second part arises.

The paper does not, despite the claim in the abstract and introduction, shed much light on the optimal form of international banking, nor on the role of foreign lending in transmitting shocks. The reason is that the paper is actually more limited in its ambitions and reasonably so, given the complexity of what is attempted. The main focus is on the empirical model aimed at understanding what factors result in different shares of the channels of foreign lending. The first part of the paper is thus scene-setting. It is useful and quite wide-ranging, but it does not tell us how shocks have been transmitted nor what role is played by foreign lending (via either channel) in shock transmission. It does not, therefore, establish why a focus on the determinants of the shares of cross-border lending and local lending should matter.

Furthermore, the element of the transmission of shocks that is the focus of this paper is only part of the picture. There are many other theories of contagion of a financial shock in lender countries that are not addressed in this paper. Other literature considers whether shock transmission comes primarily via trade effects or via financial flows, even when the initial shock is a financial one in the lender country. If the transmission mechanism is financial, there is still the question of whether the transmission is via quantity or price. While no paper can address everything, the interpretation in this paper that the "decentralised" model of cross-border lending (ie via local lending through subsidiaries) gives greater stability and lower transmission of shocks does need to be set in the context of whether the financial flows channel matters much at all. In the Asian region this is still an open question. The history of two major shocks gives different answers.

Given the implicit assumption here that the quantitative transmission of shocks via flows of foreign lending matters, another elephant-in-the-room is the classic identification problem. Variations in bank lending can be the result of demand (borrower) side changes as well as supply side. The empirical model used here could be regarded as a version of a reduced form, including both demand and supply factors, but the interpretation concentrates on the supply side drivers as if these were the only choice variables of interest.

Despite these observations, we learn a great deal about foreign bank lending in the Asia-Pacific region from this paper. The authors have access to valuable data and are very familiar with their intricacies. They have a strong knowledge of the way foreign creditor banks behave and bring that to bear on important questions that may affect financial stability in the region. Even allowing for some caveats about the robustness of their econometric results in their current form, it's clear that they point in the direction of additional important research that is needed to strengthen policymakers' awareness of the benefits and potential risks in integrating with external credit markets. And even if the size and significance of their estimates may be affected by the methodology, their main conclusions remain plausible and important. These are that:

- the share of local lending is greater in more fragile and less-developed banking systems;
- banking system factors are important determinants of the choice between local and cross-border lending; and
- banking system factors are probably as important as other drivers (trade, gravity) in the variation in the growth of lending.

These observations should certainly give policymakers in the region food for thought.