Discussion of Sebnem Kalemli-Ozcan, Xiaoxi Liu and Ilhyock Shim's paper[1]

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Exchange rate appreciations can be expansionary or contractionary for an economy whose currency appreciates vis-à-vis the US dollar. The standard Mundell-Fleming model predicts a contractionary effect as a result of a decline in net exports with an appreciating currency. However, it is possible that investment responds positively to an exchange rate appreciation. This can work via two channels: the interest rate channel and the balance sheet channel. To be able to guide the policy debate, one needs to know which of these channels dominate in the aggregate. Kalemli-Ozcan et al (2018) provide evidence on this conjecture, focusing on the balance sheet channel. They ask whether firms will take on more debt if the exchange rate of their home country appreciates vis-à-vis the US dollar.

Their results show increased leverage (ie risk-taking) as a result of a positive exchange rate-related balance sheet shock to firms. When faced with a local currency appreciation against the US dollar, firms with larger FX debt before the exchange rate appreciates increase their leverage relatively more than those with smaller FX debt after the appreciation. The authors control for country- and industry-level demand and supply shocks and policy changes by using country-sector-year fixed effects.

They do not observe large appreciations: the largest is 17%. This may explain the small effects in their paper. Their benchmark estimate of 0.035 implies that a firm with more FX debt than the typical firm will increase its leverage ratio 3.5 percentage points more than the firm with FX debt lower than the typical firm after a 10% appreciation of the exchange rate. This represents a 22% increase over the sample mean of leverage. Their estimates are larger for the firms in the non-tradeable sector. The estimate for the average firm in the non-tradable sector is 0.06, representing a 6 percentage point increase in relative leverage between high- and low-FX debt firms, which corresponds to a 37% increase relative to the sample mean of leverage.

The paper's starting point is the evidence that capital inflows are expansionary, which means that the effect of lower borrowing overcomes the contractionary effect of the exchange rate appreciation. In this framework, the authors seek to test the channel of leverage behind this empirical correlation, using firm-level data. As result, they find that firms with higher foreign currency (FX) debt increase their leverage relatively more after the appreciation: with a 10% appreciation of the exchange rate, a firm with above-average FX debt will increase its leverage by 22% more than the average.

The paper makes two contributions to the literature. The first is to help solve Blanchard's puzzle: capital flows are associated with output expansion because, since they cause appreciation, they induce firms with higher FX exposure (which therefore will see their balance sheets strengthened) to take on even more debt (intermediate

[1] "Exchange rate appreciations and corporate risk taking" by Sebnem Kalemli-Ozcan, Xiaoxi Liu and Ilhyock Shim is not included in this volume but has been published as BIS Working Paper no 710.

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result) and therefore increase output (final outcome). On the negative side, higher debt will cause potential vulnerability should a depreciation eventually occur. The paper's second contribution is the use of firm-level information, which is the only way to ascertain the micro-foundation of such mechanisms (ie the individual relative FX exposure across firms matters).

While thoroughly researched and well executed, the paper raises a number of issues, mostly related to the availability of firm-level FX debt data and to the econometric estimation. Below it is a summary of the perceived weaknesses.

First, as the FX exposures are not available at the firm level, the authors make the strong assumption that every firm's FX share of debt is in the same proportion as the country's. This is very unrealistic, since in reality a firm's FX share of debt would be dispersed. Factors that might affect it would be the firm's size and its foreign exposure (ie firms that export more have more foreign debt). Moreover, this assumption implies that a heterogeneity feature, critical to underpinning the very purpose of the paper – the importance of the FX exposure BY FIRM – is absent. The authors take into account the firm's size in the regressions and consider tradable sector and non-tradable sector firms separately. I would suggest correcting the firm's FX share of debt by retrieving the share of FX exposure by sector, drawing on a database where this info is available (for instance, in Europe), and using it to correct the data set; and, as they mention (on page 6 of the paper) that large publicly listed companies report their loans and bonds denominated in foreign currency, collecting this data and running the same regressions. They would be able then to compare the results and check the robustness of their strong assumption.

Secondly, the authors make an interesting list of stylised facts and show that the issue of FX exposure is actually high in Latin America, but not particularly worrisome in Asia, the chosen region for their application. Looking at the chart, it would seem that, for Asia, it is only in Indonesia that the share of FX exposure is relatively high – about 20% – and increasing, while for the rest of the region it is low and generally declining. Since the authors claim – in the conclusions – that their work has strong policy implications, particularly related to the reversal of the appreciation-related debt build-up, one could question the adequacy of the selected data set of countries for that purpose.

Third, the paper starts from observations that are macro: capital flows, indebtedness, growth. The micro-foundation could matter given that the samples are representative by individual countries.[3] Still, we need to know, for instance: whether they are meaningfully comparable across countries (eg CompNet) and if the data set that they used is balanced or unbalanced. Furthermore, what we gathered is that the samples contain small and very small firms, which do not hold debt. Are these the firms we want to consider? Would it not be better to go straight to Compustat, which includes only listed firms, which are more likely to engage in complex FX trading/borrowing activities (page 3 of the paper)? In this context, the authors provide separate results for the tradable and non-tradable sector, and it would be good to show separate results for large and small firms. Granularity is good to consider in general, but can be misleading when we lose the contact with the macro phenomenon.

[3] Jingting Fan and Sebnem Kalemli-Ozcan, "Emergence of Asia: reforms, corporate savings, and global imbalances", *IMF Economic Review*, June 2016, volume 64, issue 2, pp 239–267.

Fourth, regarding the estimation, in addition to the control variables of firm's size, collateral and profitability – used in the paper – the standard theory of indebtedness also uses the volatility of the operational results (dispersion/average of the EBIT in the industry), the uniqueness of the goods produced, and the median industry indebtedness.

Finally, the choice of using OLS can be criticised, because the main independent variable (high FX debt) leads to an endogeneity issue: therefore, an instrumental variable approach should be used. The authors may also want to consider a GMM estimation, because the indebtedness of a firm is a dynamic and persistent process.

To conclude, despite its good intentions, the paper seems to need some substantial reconsideration/strengthening in relation to the underlying data (especially the non-availability of firm-level FX exposure) and to the econometric procedure used. Actually, I wonder if the paper, rather than considering debt as a dependent variable, should not have considered instead as a dependent variable a more explicit final outcome, such as value added. The idea being to test directly the extent in which the exchange rate movements differentially influence the firms' outcomes – and, at the macro level, GDP, which is one of the starting points of the initial puzzle of Blanchard's puzzle – regardless of the channel of transmission (the higher debt). In this context, CompNet – the Competitiveness Research Network – has done relevant research on establishing the role of the exchange rate in explaining trade, against the background of firms' differing productivity and size.

Having said all of the above, the authors must be commended, since they are opening up an important avenue of research. Data problems will eventually be solved, and at that point we will have a framework of analysis ready to use.