Comment on "U.S. Monetary Policy and Fluctuations of International Bank Lending" by Stefan Avdjiev and Galina Hale

Jonathan D. Ostry[1]

International Monetary Fund

This discussion has been modified from how it was presented at the conference, in order to incorporate some reflections on the changes that the authors have made to the paper.

The paper by Stefan Avdjiev and Galina Hale asks a very good question—namely, why is there time variation in the observed correlation between the Federal Funds Rate (FFR) and cross-border bank lending flows? The authors find that this correlation varies from negative to positive, in the range of approximately -0.8 to +0.8. And it's clearly of more than academic interest to understand what lies behind this.

The authors propose two factors which influence the sign of the correlation. The first factor is the Fed's motivation in changing the policy rate. Is the Fed motivated by the economic cycle—for example, is there a boom that necessitates a higher policy rate in line with Taylor rule (TR) considerations? Or is the policy rate being altered for some other reason that has little to do with Taylor rule motivations—what the authors refer to as the policy stance?

The second factor identified by the authors is the overall global regime for cross-border bank lending from advanced to emerging market countries. Are global conditions characterized by booming cross-border AE-to-EM flows or instead a broad stagnation in such flows?

The authors obtain an interesting set of results based on this two-way decomposition of the data. Here I will focus on the results pertaining to flows from advanced to emerging market countries (AE-to-EM or North-South flows). The authors also have new results on AE-to-AE flows, but these I ignore here owing to time constraints.

The authors' main result is that the response of cross-border AE-to-EM flows to the FFR is regime-dependent, both in terms of whether the global economy is in the boom or stagnation regime for cross-border bank lending, and in terms of the Fed's motivation for the change in the policy rate.

In the boom regime for cross-border flows, when the Fed raises the FFR in line with the Taylor rule, the higher FFR is associated with *higher* banking flows to EMs. When the policy rate increases for reasons other than the Taylor rule—i.e., represents a tightening of the policy stance in the authors' interpretation—AE-to-EM flows remain broadly unchanged. By contrast, in the stagnation

[1] Views expressed are my own and should not be attributed to the IMF.

regime, increases in the FFR that are not motivated by the Taylor Rule are associated with retrenchment, i.e., lower cross-border bank flows from AEs to EMs. But FFR increases that are in line with the Taylor rule have only a non-robust association with AE-to-EM flows.

I confessed at the conference to being puzzled regarding how closely these correlations can be tied to specific economic mechanisms, especially when I stepped back a bit to consider the results in the context of the existing theoretical and empirical literature. At a fundamental level, I tend to believe that the partial effect of an FFR increase, *ceteris paribus*, is to act as a (possibly powerful) pull factor that operates in the direction of keeping flows in the US, and as such would tend to dampen flows to EMs. If there are higher capital flows from the US to EMs when the FFR rises, I tend to think this has to be as a result of something else changing at the same time. The finding of a positive effect on flows from an increase in the FFR would in this case be in spite of the increase in the FFR, rather than because of it.

It seemed to me, then, that this logic pointed in the direction of some omitted factor(s) from the model that could be driving cross-border bank flows, apart from the FFR. The authors also acknowledged this point when moving from their results to an economic narrative. There is of course a voluminous literature on the drivers of AE-to-EM flows. And it includes an entire panoply of push and pull factors: we need to pay attention not just to US monetary policy, but also to global factors such as commodity prices and risk aversion, which may affect optimism regarding EMs in general, and in addition recipient-country-specific factors such as their own monetary policies, business-cycle conditions and growth prospects.

Correspondingly, my high-level reaction to the paper's empirical strategy at the conference was to ask whether the authors may not be demanding too much from their chosen two-way decomposition of the data, while ignoring a host of factors that may be driving the variable of interest—AE-to-EM banking flows. It is fine to construct a narrative that a higher FFR in the context of a global boom regime signals not just stronger economic conditions in the US, but even better prospects in EMs, which drives flows there. But I saw a need to back up this narrative by controlling for the changing conditions in the recipient countries themselves. How is the changing multi-dimensional landscape in AEs and EMs affecting the direction of flows?

So my comment was, in essence, a plea to consider a richer set of controls in the panel regression analysis—controls that we know should matter based on the previous literature. I wanted the inclusion of direct controls for explanatory variables such as global risk appetite, the EM business cycle, commodity prices (which may act not only as pull factors in commodity exporters, but also push factors—think for example of the recycling of petro-dollars). EM interest rates would also be a relevant control.

I made my plea precisely because I was sympathetic to the narrative that the authors argue lies behind their regression results. Actually, I found it to be quite plausible. Indeed, there is a fair degree of overlap between what the authors are saying in this paper and what the IMF was arguing in the 2014 edition of its annual Spillover Report. That report asked explicitly how EMs were going to be affected by the normalization of monetary policy in the United States, and the case was made, using both simulations and empirical analyses, that much depended on the reason behind the Fed's policy changes. If the Fed tightened owing to stronger fundamentals in the US, EMs would likely experience favorable spillovers (including stronger inflows), but if higher interest rates in the US reflected *inter alia* rising financial-stability risks in the US financial sector, then the spillovers would be unfavorable (including a retrenchment of flows). Comparing the two approaches, however, I believed that the richer set of variables in the IMF's report was actually helpful in building the case for this narrative, and the two-way decomposition of the data that the authors are using here may in the end have been overly constraining.

Since the conference, the authors have conducted additional robustness checks including the addition of variables to proxy for the optimism of lenders with regard to the EM asset class: they have included the averages for EM-wide GDP growth rates and crisis incidence as well as commodity prices. Comparing tables 3 and 6, we can see that doing so reduces the coefficient on the Taylor rule term, because it is no longer having to proxy as strongly for the EM business cycle. In addition, a tightening of the Fed's policy stance now is associated with a contraction in flows in both the global boom and stagnation regimes. The coefficients are not equal across the regimes, so the authors' regime-dependence result still holds, but the distinction between the regimes is mitigated in a manner that is consistent with my high-level reaction at the conference.

The authors have argued that a richer more granular set of borrower-specific and lender-specific controls is unnecessary because they already control in their regressions for total lending and total borrowing between all country pairs included in their analysis. My concern with this approach is that these aggregate variables may be quite imperfect proxies for the fundamentals that are excluded from the analysis, and that these total quarterly flows may be quite volatile as well. In the end, I would need to be convinced that they are doing a good job of capturing the variation in the excluded fundamentals that the previous literature has deemed salient. If data availability is a constraining factor for the authors' desired sample in this paper, then perhaps it is work for the authors to tackle in the future using a smaller subset of countries.

Let me now turn to the two decompositions undertaken by the authors in this paper. First, I would like to talk about the FFR decomposition itself—into its TR and policy stance (MP) components. I mentioned at the conference that when I looked at the graphical result of the decomposition in the paper, one thing that struck me was that there are a couple of periods that seemed important for the FFR decomposition and for identification purposes, but which did not fit very well with the authors' constructed economic narrative.

One is the Volker era. In the authors' narrative, this period is identified as TR-driven, but this is a tough sell in terms of the narrative of high US activity, since it is hardly a time when the US economy was booming and policymakers were attempting to dampen demand. In addition, this early period in their sample was of course not a period where the Fed was actually following a Taylor rule approach, since it was mainly a period of monetary targeting and policy rates were quite volatile.

Apart from the Volker period, the more recent period of quantitative easing (QE) at the end of their sample is identified by the authors as one where policy was too loose on TR grounds. This is not an uncontroversial conclusion—much depends in particular on the specific measures used for the output gap and inflation (a point emphasized recently by former Chair Bernanke). My bottom line on this at the conference was simply to express some worry about the sensitivity of the decomposition to these two periods which seemed essential for the identification in the paper.

Since the conference, the authors have made a reassuring addition to their paper that their results are robust to excluding the Volcker period. The importance of the post-global-financial-crisis period to the identification remains unclear, but perhaps a deeper study of the importance of the decomposition in this period touches on a range of issues related to post-crisis regime changes that are better studied on their own in a separate paper.

While we are on the topic of the decomposition between the TR and MP components, let me note that I have some skepticism about what exactly is contained within the residual MP component. To begin with, its derivation as a residual means that many different underlying variables are included within it, so it is not clear how to intuitively interpret its effect. I have already speculated that outside TR concerns, financial-stability risks may be one factor causing the Fed to act, but there may also be other motivating factors. Separately, it is unclear how the MP component captures announcement effects. We have seen—for example, during the Taper Tantrum episode—that such effects can be powerful drivers of capital flows, even when there is absolutely no change in the FFR. How can the decomposition handle these impacts? It would appear to ignore them, but they may have been important both in the historical sample and may also have salience going forward.

A final issue with the FFR decomposition is that it is probably going to be least reliable when it is most needed for predictive purposes—namely, at the end of the sample when the scope for revision of the underlying data is largest. It might in fact be interesting for the authors to re-estimate their decomposition with the actual data that were available to the Fed (and to market participants) in real time. This real-time information set is what would have been driving the decisions both of the Fed and market participants, and thus presumable cross-border banking flows themselves.

The second important decomposition in the paper is that of the global bank lending regime—into boom and stagnation periods. One appealing dimension of this decomposition is its broad consistency with the insights from collateral-based lending models. In particular, in such models, when EM asset prices are high (perhaps within the boom regime identified by this paper), global banks are far from their balance sheet constraints, and changes in the FFR may have a limited impact because although they may cause changes in asset prices, those changes are not enough to make the balance sheet constraints binding. On the other hand, when asset prices are low and banks' constraints are already binding (in the stagnation regime), the effect of an FFR increase through declining collateral values will be especially important.

Another interpretation that may rationalize the authors' decomposition is the maturity of the lending cycle. In the early stages of the cycle, when leverage is presumably not excessive in EMs, FFR increases might have little effect on the level of cross-border flows. But as the cycle matures and the extent of EM indebtedness rises, there may be more pronounced effects from FFR increases, especially if the US dollar is also more richly valued in these later stages. This interpretation is also consistent with the IMF's own EM vulnerability exercises, where we don't find a robust channel of transmission from an FFR change to crisis vulnerability unless the change is interacted with measures of EM indebtedness.

Despite the appealing nature of the decomposition, I have some unease in the authors defining the global regime using aggregated levels of the very left-hand side variable that they are seeking to explain in the later regressions—i.e., cross-border AE-to-EM bank lending. Is there some kind of circularity inherent in this approach?

And what about switches between the regimes identified by the authors? Regime-switching is surely of interest if the authors' approach is to be used as the predictive tool that they propose: given the different behavior of cross-border flows between the regimes, recipient countries will want to be able to assess the risk that the world is moving from a boom to a stagnation regime or *vice versa*. Crucially, recipient countries worry whether this probability of a switch in the regime is itself heightened by a change in the FFR, or perhaps by a change in risk attitudes that the FFR may generate under certain conditions. This latter point deserves further exploration.

Stepping back a bit, I commented at the conference that given the novelty of the decomposition of the global regime, and the dependence of the authors' results on it, I believed that the authors could do more to motivate the chosen decomposition. One way for the authors to begin, I suggested, would be to ignore regimes at first, and simply to add to their baseline regression a series of interaction terms combining the FFR change and other variables that seem salient *ex ante* in conditioning the transmission mechanism from the FFR to cross-border flows (such as the VIX, credit spreads, the value of the US dollar, the level of EM debt, etc.). They could even consider developing a composite external financing conditions index which encompasses a number of these additional variables, and then interacting that variable with the FFR change. If, and only if, the model with these additional regressors has a poor fit would the authors then consider distinct lending regimes along the lines shown in the paper.

If a regime-based approach turned out to be optimal, I counseled that the authors should then explain the rationale for the number and nature of the selected regimes. I wondered whether the data really supported the notion that flows can be characterized as falling neatly into the two regimes of boom and stagnation that the authors have chosen. My own work and that of others suggested that a three-way classification—into booms, crashes, and normal times (the latter making up the lion's share of the observations)—might fit the cross-border flows data somewhat better than the two-way classification advocated by the authors. This three-way classification is something I suggested the authors could look into. Since the conference, the authors have added a statement that their results are robust to a three-way classification. Either in this paper or in future work, I would encourage the authors to provide more details on exactly how the transmission channels look in each of these three regimes, so that a better connection can be made between this paper and the recent theoretical and empirical literature on capital flows to EMs.

Finally, since the authors propose using their model for predictive purposes, one additional point for them to consider is whether their global lending regime decomposition, which is derived based on the overall level of AE-to-EM flows, is primarily driven by behavior common to all EMs, or instead primarily by the behavior of a couple of large countries within the EM sample. The answer to this question is relevant for an emerging market country which wishes to apply the results in this paper to forecast its own flows. If the regime classification is driven by a few large countries and/or financial centers, the results will obviously be less useful for smaller EMs with more conventional characteristics. But such a problem can be countered and mitigated: for example, the authors could identify their global regime based on an estimated common factor for global AE-to-EM flows, instead of on the aggregated level of such flows.

To conclude, I think the authors have asked a good question, they have brought interesting facts to bear on the question, and they have developed an approach that reveals interesting new information. At the same time, evaluating this new information in the context of the existing literature, I argued at the conference that there was room for a richer explanation of the observed time-varying correlation than what was yielded by the authors' chosen decompositions. I am happy that the authors have made some important advances in this direction since I delivered my remarks.