Comment

Jón Steinsson, *University of California, Berkeley and NBER*, United States of America

Camara, Christiano, and Dalgic have written an interesting and important paper on an important topic in international macroeconomics. The paper has both a substantial empirical component and a substantial theoretical component. The main empirical results of the paper are that a US monetary tightening leads output in other countries to fall and that it leads output in emerging market (EM) countries to fall more than in advanced economies (AEs). The main theoretical result in the paper is that the trade channel explains the bulk of this negative international transmission.

I. Empirical Results

I will start by considering how the authors reach their empirical conclusions. They use monthly data from 2006 to 2019. This is a rather modest amount of data: only a little more than one business cycle in the United States. This choice therefore limits the strength of the statistical inference and may make it sensitive to special features of this 14-year period. The authors make this choice because "many of the EMEs [emerging market economies] in our sample are characterized by different monetary and fiscal regimes before 2000."

The authors use monetary shocks constructed by Bauer and Swanson (2023) from high-frequency data around Federal Reserve Board (Fed) announcements. They run Bayesian vector autoregression (VAR) with "Minnesota" priors, that is, priors that shrink coefficients of their (vector) autoregressive system toward a random walk. They first run a VAR with

NBER Macroeconomics Annual, volume 39, 2025.
 https://doi.org/10.1086/735261

9 variables and 12 lags for the United States and then a panel VAR with 11 variables (3 US and 8 local) but only 2 lags separately for 10 AEs and 14 EMEs.

Figure 1 of the paper presents the results of the US VAR. The results are—for the most part—relatively standard. The main ways in which the results are unusual are (1) gross domestic product (GDP) drops on impact rather than falling gradually, which is more common in this type of analysis; and (2) the response of imports and exports is very large. The peak response of imports and exports is almost a 20% fall (about 10 times larger than the proportional response of GDP).

Figure 3 of the paper reports the responses of AEs to a US monetary tightening. Output and prices fall. Imports and exports fall by very large amounts. These foreign AEs ease monetary policy in response to the Fed tightening. Initially, the exchange rate depreciates, but after a few months, it begins appreciating and appreciates rapidly for over 2 years.

Figure 4 reports responses for EMEs. Overall, these responses are similar to those for AEs. One notable difference is that the fall in output is larger than for AEs. The fall in imports and exports is estimated to be very large. In contrast to AEs, the EMEs are estimated to tighten monetary policy even though output and prices are falling. These estimates, thus, indicate that the EMEs do not typically adopt countercyclical monetary policy in response to economic softening caused by Fed tightening. As with AEs, the exchange rate first depreciates and then appreciates rapidly.

The authors are not the first researchers to estimate the international transmission of US monetary policy shocks. In fact, there is a robust recent literature on this topic using similar methods to the ones the authors use. A notable recent paper in this literature is Degasperi, Hong, and Ricco (2023). Figure 2 of that paper presents results that are broadly similar to those of the present paper. In response to a US monetary tightening, output and prices fall in AEs and EMEs. The fall in output in EMEs is smaller than the headline estimates in the present paper (more similar to some of the robustness analysis in the appendix of the present paper). Trade volumes respond strongly (but not as strongly as in the present paper). Monetary policy eases for both AEs and EMEs. This is more in line with Leo, Gopinath, and Kalemli-Özcan (2024) than with the results of this paper. Another notable difference is that the estimated response of the exchange rate is quite a bit smaller.

Although there are some differences, the strong overall similarity of the results of Degasperi et al. (2023) to the results of the present paper is reassuring. Degasperi et al. (2023) make different choices regarding a number

of important aspects of the analysis. For example, they use about twice as much data in the time series (their sample period is 1990–2018). They also use data from more AEs (15 rather than 10). They make the more standard assumption for a monthly VAR to have 12 lags as opposed to 2 lags. And they use a different monetary shock series—the one from Miranda-Agrippino and Ricco (2021) rather than the one from Bauer and Swanson (2023). These differences in methodological choices suggest that the core empirical findings of this paper are relatively robust to changes in various choices made by the authors.

II. Empirical Methods: VAR versus LP

The empirical exercise performed in the paper is an exercise in dynamic causal inference. The authors are interested in estimating the dynamic effects of US monetary policy on various macroeconomic outcomes in other countries. They have a series of monetary shocks, which they take from an earlier paper (Bauer and Swanson 2023). Their goal is to estimate how these shocks affect the various macroeconomic outcomes at different horizons.

As I mentioned above, the method the authors use to perform this exercise is a Bayesian VAR with Minnesota priors. An alternative method would be to use LPs, or "local projections." An LP is simply fancy macro jargon for directly regressing the variable of interest on the exogenous shock at the researchers' disposal. The researcher is interested in the effect of ϵ_t on Y_{t+h}. Why not simply regress Y_{t+h} on ϵ_t? In other words: Why not use an LP?

This question is closely related to another question: Why use a VAR? Conceptually, there are two quite distinct reasons why a researcher may choose to use a VAR, and it is important to keep the distinction between these two reasons clear. One reason for using a VAR is to identify exogenous shocks. A number of different strategies for identifying exogenous shocks in macro rely on VAR methods. This includes Cholesky decompositions, long-run restrictions, sign restrictions, and so on. This is not what the present paper is doing. The present paper has an exogenous shock series constructed by other researchers, which was identified using high-frequency methods. The rationale for using a VAR in the present paper must therefore be something else.

The other rationale for employing a VAR (rather than an LP) is to enhance statistical power when constructing impulse responses. This is what the authors are using the VAR for in this paper. The authors have a relatively modest data set—14 years, a little more than one business

cycle—yet their empirical procedure yields a considerable amount of statistical power (modestly sized confidence intervals). They are leaning on the VAR and Minnesota priors to shrink their standard errors and smooth out the impulse responses.

The variance reduction of a VAR comes at a potential cost in terms of bias. LPs make minimal assumptions and are therefore unbiased. But they can be very noisy if the data are not very informative. VARs, on the other hand, make stronger assumptions. This reduces variance, but if the assumptions embedded in the VAR are not valid, it introduces bias. This raises a question: Are the empirical conclusions in the paper driven by the data or by the assumptions in the VAR?

VARs are a bit of an odd tool to reduce variance because they are extremely highly parameterized. The US VAR estimated by the authors has 108 parameters and 168 data points. Estimating so many parameters raises the issue of overfitting. Recent advances in econometrics have focused very strongly on this overfitting problem. In particular, controlling overfitting is a core goal of machine learning methods. For example, Lasso and Ridge regressions shrink parameters for the purpose of limiting overfitting. The authors' approach to limiting overfitting is to use Minnesota priors. These shrink the impulse responses toward unit roots. This will work well if the true data-generating process is close to a unit root but less well if it is not.

A second issue is that VARs contain lagged dependent variables. Regressions with lagged dependent variables are biased. In particular, the coefficient on the lagged dependent variable is biased downward. (The direction of bias of other coefficients is more complex.) With only 14 years of data, this type of bias may be substantial. The Minnesota prior pushes against this bias. But it is not clear which way the net bias will go in the authors' setting.

It is useful to consider a few specific impulse response estimates in the paper with the issues discussed above in mind. First, consider the response of GDP in the US VAR. As I mentioned before, GDP jumps down on impact. This is an unusual result. The whole impulse response looks like a unit root response. Are the authors estimating this because the data call for it or because of their prior? Because this VAR has 108 parameters and only 168 data points, the overfitting concern is very severe. My guess is that the Minnesota priors are rather tight. Or in other words, the empirical results are likely being shrunk toward a unit root pretty aggressively.

Contrast this with the response of the nominal exchange rate in the EM VAR. Exchange rates are usually considered to be very close to a unit

root. But in this case, the authors estimate an impulse response that is quite far from a unit root. The exchange rate is estimated to appreciate very rapidly after it stops depreciating. One worries that this might be due to downward bias in the largest root of the estimated autoregressive system.

The authors do present results from an LP in the appendix (fig. 23). These results are useful in helping to assess what is really coming from the data and what is coming from the VAR assumptions. A notable difference in results is that the nominal exchange rate response in the LP looks much more like a unit root. In their LPs, the authors take averages of the variables that they are interested in across countries within AEs and EMs. They include 24 lagged controls in these regressions. With only 168 data points, even this raises some potential concerns about overfitting. One noticeable aspect of the LP impulse responses is that the confidence bands are quite jagged, implying that one can reject a smooth response. This suggests that these confidence intervals are narrower than they should be. Nevertheless, it is valuable to have these LP estimates as they come closer to showing the reader what is coming purely from the data before stronger assumptions are layered on.

III. The Trade Channel and Financial Channels

Let me now turn to the theoretical part of the paper. The recent literature has emphasized the financial channel of the transmission of US monetary policy to other countries. Most prominently, Hélène Rey and coauthors have emphasized the notion of a Global Financial Cycle (Rey 2013; Miranda-Agrippino and Rey 2022). This theme is prominent in a large body of recent work, including the papers by Şebnem Kalemli-Özcan and Riccardo Degasperi and coauthors cited above. The present paper uses a structural model to assess the mechanisms through which US monetary policy affects other countries. The authors conclude that it is the trade channel that explains most of the transmission.

In a vanilla open economy model, a US monetary tightening has two effects: First, the US dollar appreciates, which implies that the currencies of other countries depreciate. This results in expenditure switching away from US goods and toward foreign goods. This should result in positive transmission toward other countries, that is, an expansion of output in other countries. The other basic channel in a vanilla model is that a US monetary tightening causes demand in the United States to fall. Because the United States is a big country, this lowers demand in other countries as

well. This causes negative transmission toward other countries, that is, a slowdown of output in other countries.

If the second of these channels is stronger than the first, then a US monetary tightening will result in foreign output falling. It seems to me that this is basically what is going on in the present paper. The authors assume dollar pricing (as opposed to producer currency pricing), which helps mute the expenditure switching effect. There is strong evidence for this assumption in the literature. But then there is the question of the strength of the demand channel. In the present paper, it is estimated to be quite strong. Might it be too strong?

One of the points that I made in my discussion of the paper at the conference was that I worried about this issue. The authors have since revised their paper addressing this issue. The estimation of the model in the draft of the paper presented at the conference implied that the small open economy was very exposed to US demand. In the model, the "foreign" region is equated with the United States and the estimation in the earlier draft implied that the weight of US goods in the home consumption basket was 46% for EMs and the weight of US goods in the home investment basket was 71%. I remarked that these seemed much too large, and I worried that this made the trade channel too strong in the model. The revised draft of the paper adjusts the priors in the Bayesian estimation exercise to reduce the weight of US goods in the home consumption and investment baskets. For EMs these are now 2% and 31%, respectively. Because this large change has not materially affected the results, my worries about this are mitigated.

The authors' model incorporates a wide range of different financial frictions. But the authors demonstrate convincingly that to the extent that these frictions affect the response of AEs and EMEs to a US monetary shock in their model, it is mostly through the trade channel. This is clearly demonstrated in figures 7 and 9, which present the fit of the model to the estimated impulse responses for AEs and EMEs, respectively. These figures show that the authors' model is able to generate a substantial fall in output, investment, and exports in response to a US monetary policy tightening. However, the ability of the model to match the response of the nominal exchange rate, the response of domestic monetary policy, and the response of the price level are rather poor for AEs (better for EMEs). In particular, the model generates a substantially smaller response of the nominal exchange rate than the data do (and no delayed overshooting).

Figures 7 and 9 also present a counterfactual where the authors only hit the AEs and EMEs with the change in US interest rates, not the response of US GDP and prices to the US monetary shock. The response of output

and investment is much smaller in this counterfactual, and the response of exports has the opposite sign relative to the data. This shows that the model is not generating much action in foreign countries from pure changes in US interest rates independent of their effect on US demand.

Why does the model come to this conclusion? One potential reason is that there may be no other mechanism in the model to generate a substantial fall in foreign output. But the model does incorporate a large range of financial frictions. After all, the idea is to give financial frictions a fair shot at generating the foreign bust.

One potential difficulty is that the response of the nominal exchange rate implies large excess returns on the foreign country currencies after a few months. Initially, the US dollar keeps appreciating (delayed overshooting). This generates excess returns on US dollars. But after a few months, the US dollar begins to depreciate and does so quite rapidly for several years. This generates high returns on the currencies of the foreign countries.

High returns on the foreign currencies should generate capital inflows into the foreign countries and therefore should push in the direction of a boom in the foreign countries. This is, of course, exactly the opposite of what the authors "need" to match the fall in foreign output they estimate in the data. I worry that this issue is part of why the financial channels are not playing a bigger role in the authors' model. But I have to admit that I do not think I fully understand how all the different features of the authors' model interact to generate their results. This implies that I do not feel that I have a great grasp on why they come to the conclusion they come to in the theoretical part of the paper.

Let me conclude by saying that I enjoyed reading this thought-provoking paper. I learned a lot thinking about it over the past few months. It is really striking how large the effects of US monetary policy seem to be on other countries. The authors' conclusions are provocative in terms of the breakdown of these effects into a trade channel and a financial channel. But I think we need a lot more work on international financial frictions before we can generate a consensus on these issues.

Endnote

Author email address: Jón Steinsson (jsteinsson@berkeley.edu). For acknowledgments, sources of research support, and disclosure of the author's material financial relationships, if any, please see https://www.nber.org/books-and-chapters/nber-macroeconomics-annual-2024-volume-39/comment-international-monetary-transmission-mechanism-2-steinsson.

References

Bauer, M. D., and E. T. Swanson. 2023. "A Reassessment of Monetary Policy Surprises and High-Frequency Identification." *NBER Macroeconomics Annual* 37:87–155.

Degasperi, R., S. S. Hong, and G. Ricco. 2023. "The Global Transmission of U.S. Monetary Policy." Working paper, Sciences Po OFCE no. 09/2021.

Leo, P. D., G. Gopinath, and Ş. Kalemli-Özcan. 2024. "Monetary Policy and the Short-Rate Disconnect in Emerging Economies." Working Paper, University of Maryland.

Miranda-Agrippino, S., and H. Rey. 2022. "The Global Financial Cycle." In *Handbook of International Economics, Volume 6*, ed. G. Gopinath, E. Helpman, and K. Rogoff, 1–43. Amsterdam: Elsevier.

Miranda-Agrippino, S., and G. Ricco. 2021. "The Transmission of Monetary Policy Shocks." *American Economic Journal: Macroeconomics* 13:74.

Rey, H. 2013. "Dilemma not Trilemma: The Global Cycle and Monetary Policy Independence." Proceedings—Economic Policy Symposium—Jackson Hole, Federal Reserve of Kansas City, 285–333.