Comments on *Foreign Effects of Higher US Interest Rates* by Iacoviello and Navarro

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Introduction

This paper asks an important and under-studied question, namely what are the effects on foreign output of shocks to American interest rates? A good research paper provides a plausible answer to an interesting question, so the good motivation for this topic means that half the battle is already won. The question I focus on for the remaining of my comments is whether the answer provided by the authors is plausible.

I have no issues at all with theory in the paper, since there is none; the analysis is wholly empirical, and the paper should be judged accordingly. The empirics are divided into two parts. The first measures the exogenous part of American monetary policy shocks, and I have no substantive issues with it. So the entire focus of my comments is on the second part of the empirics, which links foreign

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output to the (American) monetary policy shock. I have two sets of issues with that; the data, and the methodology.

Data Issues

The authors employ a panel of data covering 50 Countries, 1965Q1 through 2014Q4. Now a long, broad panel like this seems ideal to answer the question of interest. However, there are a couple of aspects that make me a little queasy. First, a non-trivial amount of the data set has been converted from the annual to the quarterly frequency, which is intrinsically problematic. Second, a lot of the data set has been extrapolated backwards. For instance, footnote 10 in the current draft of the paper reads (highlights added):

> "To avoid dropping observations relative to our benchmark analysis, we fill in the missing observations using backward extrapolation. ***For instance, we assume that the current account position of a country in 1965-1969 is equal to its 1970 value…"***

This might not worry one so much if it did not affect 26 of the 50 countries in the sample. And the problem is not only widespread but non-random; most of the missing data is early in sample, which is disproportionately during the fixed exchange rate regime periods. This leads me to believe that there may be selection bias in the sample.[2]

[2] There are also lesser problems with the data, stemming from the fact that the authors use the Reinhart-Rogoff data set on exchange rate regimes. One that jars me personally is the authors' statement that "Canada, for instance, was closely pegged to the dollar until 2002, kept a managed floating regime between 2002 and 2010 …" when the Bank of Canada clearly stated in 2011 "The last time the Bank intervened in foreign exchange markets to affect movements in the Canadian dollar was in September 1998." https://www.bankofcanada.ca/wp-content/uploads/2010/11/intervention_foreign_exchange.pdf .

A completely different issue is the American-focused nature of the analysis. A minor aspect of this is that a number of European countries in the sample (Czech Republic, Hungary, Poland …) have made the transition, during the sample, from 2nd world to 3rd world to 1st world. It seems odd to estimate time-invariant functions for such countries. More important are European issues *per se*. I think of the Eurozone as a large open economy that sets interest rates for its members as well as for a substantial periphery that is more affected by Eurozone interest rates than American rates.[3] The dozen countries in EMU are mostly unaffected by America interest rates after 1971, certainly compared with the importance of German interest rates. Another eight European countries outside EMU are affected more by EMU/Germany more than by America (Czech Republic, Denmark, Iceland, Norway, Poland, Sweden, Switzerland, and UK). My suggestion would be to re-center the 19 smaller European countries on Germany rather than the United States, drop EMU observations after the creation of the Euro, and drop Germany from the sample altogether, just like the USA. That would also reduce the excessively large number of rich floating exchange rate observations (80%) in a reasonable way; after all, France, Germany, Spain and Italy all float in precisely the same way vis-à-vis the United States, since they are all in EMU.

So, to summarize, I see the following problems with the data base. The conversion of annual to quarterly and the extrapolation backwards in time are both disagreeable aspects of the data set. More importantly, the European nature of the data set – some 40% of the observations – has been inappropriately ignored.

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[3] Before EMU, Germany was surely the central large open economy in Europe, affecting financial conditions both inside and outside the EMS.

Methodological Issues

The authors include a number of mechanisms to model the varying linkages between the United States and the peripheral countries. There are three: 1) the exchange rate regime against US$; 2) trade openness vis-à-vis the United States; and 3) an index of "Financial Conditions" or *Vulnerability Index*. This itself is a principal component of inflation, output gap, and the current account deficit. The last is the one that is most interesting and works best. But it is far from obvious what is going on. First, the list of three variables don't seem mostly financial, so the name seems inappropriate. Second, it is unclear to me why this list of variables is chosen. Third, a large number of potential alternatives are excluded, each potentially linking center country monetary policy to foreign output. At least seven come to mind: 1) international reserves (as emphasized by East Asians since the Asian crisis and Jeff Frankel); 2) external debt, especially if denominated in foreign currency (as emphasized by Calvo); 3) capital controls (as most academics emphasize, given the importance of Mundell's trilemma); 4) domestic credit growth (as emphasized by Claudio Borio); 5) government debt (a particular obsession with German policy-makers); 6) asset price bubbles (as emphasized by policymakers who think that MacroPru is the silver bullet); and 7) capital flows and local measures of financial uncertainty analogous to the VIX (as emphasized by Helene Rey in her work on the global financial cycle).

So the list of potential mechanisms linking center-country monetary shocks to output performance in the periphery seems excessively short to me. But there's a completely different problem affecting the Financial Conditions index. In particular, it is unclear to me why the authors use a principal component to combine three variables (inflation, output gap and current account deficit) into a single index, when they do nothing comparable for either foreign exchange or trade linkages. More generally, I don't understand why any data shrinkage or economy is needed; why use one principal component and two variables rather than a dozen measures separately?

More generally, the methodology is both at least a little opaque and somewhat contrived. Consider the status of foreign output. Foreign GDP is modeled as a cause of US monetary shocks (eqn 1). Even ignoring the legality of this given the Fed's dual mandate, foreign output is also *caused* by American monetary shocks – indeed, that is the point of the paper. But it is also part of the transmission mechanism, since the foreign output gap also enters in the index of financial conditions. One has to have a lot of faith to believe that the three different roles of foreign output can be clearly disentangled through the econometrics. Even ignoring this opacity, the methodology seems overly intricate to me. For example, consider the interactions of section 5.1, which consist of 5 (!) steps: 1) standardization; 2) logistic transformation; 3) re-centering; 4) interacting; and 5) recursively orthogonalizing. Again: the index of financial conditions is the principal component of a three-year moving average of three fundamentals (inflation, the output gap and the current account deficit) truncated at 5%. In both cases, there seem to be a number of unnecessary steps, and this complexity comes at a cost. Unless considerable sensitivity analysis is conducted to reassure the reader that none of these steps is critical, the credibility of the analysis suffers. I personally would find a more straightforward approach more plausible. For instance, I'd begin by simply adding interactions directly or splitting the sample when it comes to modeling the mechanism linking central monetary shocks to peripheral output.

My Bottom Line

Iacoviello and Navarro arrive at a number of conclusions. First, u*sing a panel* (with both cross-section and time-series variation) seems best to address the issue of measuring the foreign effects of American monetary policy. Second, there is typically a l*arge* response of foreign output to tighter American monetary policy, often comparable to that within the United States. Third, there is l*ots of heterogeneity* in these foreign responses. In particular, the response is larger for advanced economies with tighter trade and FX links (through fixed exchange rates) to the United States, consistent with the classic Mundellian small open economy. For emerging markets, more financial vulnerability makes for a bigger response.

All of these conclusions are completely sensible, and indeed they are remarkably well-aligned with my priors. However, I have a large number of issues with the empirics they present for the reasons I lay out above. So, I believe their conclusions, but not their evidence; I personally have experienced little Bayesian updating.