

This PDF is a selection from a published volume from the National Bureau of Economic Research

Volume Title: NBER Macroeconomics Annual 2011, Volume 26

Volume Author/Editor: Daron Acemoglu and Michael Woodford, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 978-0-226-00214-9 (cloth); 978-0-226-00216-3, 0-226-00216-0 (paper)

Volume URL: <http://www.nber.org/books/acem11-1>

Conference Date: April 8-9, 2011

Publication Date: August 2012

Chapter Title: Comment on "Risk, Monetary Policy and the Exchange Rate"

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Chapter URL: <http://www.nber.org/chapters/c12422>

Chapter pages in book: (p. 315 - 324)

Comment

Martín Uribe, Columbia University and NBER

This paper studies the effects of time-varying volatility shocks in the open economy. More specifically, it analyzes the consequences of productivity volatility shocks, monetary-policy volatility shocks, and inflation-target volatility shocks for the dynamics of exchange rates, the yield curve, and cross-country interest-rate differentials.

Spurred in part by significant advances in quantitative economics and computational speed, interest in the macroeconomics of uncertainty shocks has experienced a revival in the past few years. Recent applications include an explanation of the great moderation based on a decline in the volatility of structural shocks (Fernández-Villaverde and Rubio-Ramírez 2007; Justiniano and Primiceri 2008), an evaluation of the role of country-spread uncertainty as a driver of business cycles in emerging countries (Fernández-Villaverde et al. 2011), and uncertainty shocks to productivity as determinants of the demand for factors of production at the firm level (Bloom 2009). The Benigno, Benigno, and Nisticò paper adds to this list by considering the role of uncertainty in a global context.

This is an ambitious project, for it attempts to accomplish three demanding tasks. The first one is to empirically identify the three aforementioned volatility shocks. The second one is to estimate the empirical impulse responses of a number of variables of interest to innovations in the identified volatility shocks. Finally, the paper assesses the ability of a two-country, New Keynesian model to account for the estimated impulse responses.

This paper represents a first pass at what I view as an important research agenda in open economy macroeconomics. As such, it suffers from a number of problems that I will spell out in what follows. Never-

theless, I believe that the present study is bound to become an important reference in this literature.

Identification Issues

The first contribution of the Benigno, Benigno, and Nisticò paper is to empirically identify three sources of time-varying volatility: monetary-policy volatility shocks, inflation-target volatility shocks, and total factor productivity (TFP) volatility shocks. To this end, the paper estimates a VAR system of the form

$$y_t = A(L)y_{t-1} + e_t,$$

where y_t includes ten variables that can be classified in two groups. The first group consists of three unobserved volatility shocks that form the focus of the empirical analysis. They are:

$u_{\xi,t}$ = monetary-policy volatility shock.

$u_{\pi,t}$ = inflation-target volatility shock.

$u_{a,t}$ = TFP volatility shock.

The second group of elements of y_t consists of seven observable variables typically included in open macro/finance empirical studies. They are:

i_t = Federal Funds rate.

$i_t - i_t^*$ = cross-country interest-rate differential.

$i_{sl,t}$ = slope of yield curve.

q_t = Real exchange rate ($s + p^* - p$).

p_t = CPI log level.

y_t = domestic industrial production.

y_t^* = foreign industrial production.

The authors orthogonalize the regression residual e_t using a Choleski decomposition with the order of the variables just given.

To make the previous VAR system operative, the authors must, of course, proceed to identify the three unobservable variables. I find this step of the exercise highly unconvincing. To see why, consider, for example, the identification of $u_{a,t}$, the TFP volatility shock. The authors proxy this variable with the volatility of the stock market. This is problematic because in principle the volatility of stock prices can be driven

by all of the shocks (e.g., preference volatility shocks, fiscal volatility shocks, monetary volatility shocks, animal spirits volatility shocks) buffeting the economy, not just TFP volatility shocks. Granted, under this identification approach, the VAR does deliver a measure of time-varying volatility. But it does not provide any basis to determine that this measure of time-varying volatility represents time-varying volatility in TFP. Instead, the time-varying volatility shock that comes out of the VAR is in principle a combination of a number of volatility shocks of different natures.

A similar problem arises with the identification of $u_{\pi,t}$, the inflation-target volatility shocks. In this case, the authors use as a proxy the MOVE index of implied volatilities in one-month Treasury options. Again, the volatility of bond option values can in principle be determined by multiple shocks, not just by inflation-target volatility shocks. As a result, the VAR will deliver a measure of time-varying volatility that cannot be reliably associated with innovations in the volatility of the inflation target.

These identification problems are serious for two main reasons. First, the authors use the identified VAR to plot empirical impulse responses to TFP volatility shocks and inflation-target volatility shocks. To the extent that these shocks are poorly identified, the information provided by these impulse responses may be highly misleading. Second, and equally important, the authors will build a DSGE model and will judge the ability of this model to explain the data by comparing the theoretical and empirical impulse responses to TFP volatility shocks and inflation-target volatility shocks. Because the empirical impulse responses correspond not to the desired shock but to an unknown combination of shocks, the conclusions derived from this evaluation exercise can, again, be highly misleading.

The third unobservable variable that requires identification is $u_{\zeta,t}$, the monetary-policy volatility shock. The authors proxy this variable with the (within-month mean square changes in) Federal Funds Futures Rate. In my opinion, this identification strategy is more fortunate than are the previous two. The reason is that the federal funds rate is to a large extent under the control of the monetary authority. It represents, after all, the Fed's central policy instrument. As a result, the measure of volatility constructed by the authors is likely to capture well the uncertainty involved in monetary policy. One caveat could be the fact that monetary policy has two parts, one systematic (which may depend upon variables such as inflation, output, and past interest

rates) and one nonsystematic. Ideally, the identification exercise should deliver a measure of innovations in the nonsystematic component of monetary-policy uncertainty. To the extent that the systematic component of monetary policy responds to past values of macroeconomic indicators, the VAR filter could succeed in purging a significant part of the systematic component.

Based on the aforementioned considerations, for the remainder of my discussion I will focus exclusively on the macroeconomic effects of monetary-policy volatility shocks.

Before moving on, I would like to close this section by suggesting an alternative identification approach. It consists of a direct estimation of a DSGE model. Indeed, the DSGE model that the authors build in a later section of the paper includes among its driving forces the three volatility shocks that the authors aim to identify and has precise predictions for the seven observable variables included in the empirical analysis. Admittedly, estimating DSGE models driven by time-varying volatility shocks is not a simple task. One difficulty has to do with the fact that linear approximations are not sufficient to capture the dynamic effects of disturbances in volatility. Therefore, higher-order approximations, which are technically and computationally more demanding, are called for. Another problem is the fact that the convenient Kalman filter cannot be used for constructing the likelihood function of nonlinear models. Instead, researchers have appealed to other methods, such as particle filtering. The good news is that recent significant advances in the formulation, computation, and estimation of nonlinear DSGE models coupled with ever-growing computational speed have made estimation feasible, at least at a small to medium scale. Some of the references cited at the beginning of this discussion represent examples of how this can be accomplished. In this regard, a key technical reference for macroeconomic applications is the recent survey by Fernández-Villaverde and Rubio-Ramírez (2010).

The Macroeconomic Effects of Monetary-Policy Volatility Shocks

Figure 1 represents the central empirical fact documented in this paper. It includes all three of the elements condensed in the title of the paper: “Risk, Monetary Policy, and the Exchange Rate.” It displays the response of the real exchange rate to an increase in the volatility of US monetary-policy implied by the estimated VAR system. The real exchange rate, $RER \equiv SP^*/P$, is defined as the number of US dollars re-

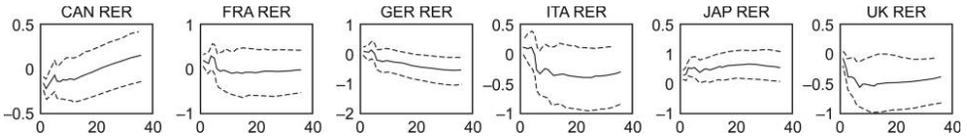


Fig. 1. The response of the real exchange rate to an increase in the volatility of US monetary-policy implied by the estimated VAR system.

Source: Benigno, Benigno, and Nisticò (chapter 5, this volume).

quired to buy a unit of foreign currency, S , adjusted by the ratio of the foreign consumer price index, P^* , to the US consumer price index, P . Therefore, when, for example, the real exchange rate goes down, it means that the US dollar is becoming stronger, or that the foreign country is becoming relatively cheaper. To understand the nature of the monetary-policy volatility shock, think of the monetary authority as following an interest rate rule that has two components. One component is systematic and may depend on variables such as output and inflation. The second component is purely random and is referred to as the monetary-policy shock. The variance of this shock is the monetary-policy volatility shock, and is itself random. Figure 1 shows the effect of an increase in this random volatility on the real exchange rate. Six foreign countries are considered: Canada, France, Germany, Italy, Japan, and the United Kingdom. I note in passing that the impulse response of the nominal exchange rate, S_t , would look very similar to that of the real exchange rate shown in the figure. The reason is that the post Bretton-Woods period, which is the sample period used for the estimation of the VAR, is characterized by much larger movements in the nominal exchange rate, S , than in consumer price indices P and P^* . As a consequence, movements in the real exchange rate are dominated by movements in the nominal exchange rate. Thus, in what follows, when I refer to the exchange rate, the reader can think either of the nominal or of the real exchange rate.

I find figure 1 highly thought-provoking in spite of the fact that, from a purely statistical viewpoint, its validity is questionable. For instance, the broken lines display one-standard-deviation error bands around the point estimates. In macroeconomics, however, the usual practice is to display two-standard-error confidence bands. Such a confidence interval would comfortably include zero for all countries, rendering the responses insignificant. And there are other statistical problems related to disparities in the signs and shapes of the responses across countries,

to which I will come back later. But from an economic point of view, the message of this figure is quite striking. To see this, I will ask the reader to do two things. First, forget about Japan. Second, erase from your minds the confidence bands. The picture that emerges is one in which an increase in monetary-policy uncertainty in the United States causes the US dollar to strengthen. This is a very counterintuitive stylized fact. For it states that if a new, more unpredictable Fed chair were to replace the current one, then the reaction of the US dollar would be to become stronger (not weaker)! This implication does not square well with the notion that the Fed is the primary guardian of the purchasing power of the US dollar. An immediate question is what kind of theoretical mechanism could explain this surprising result. I turn to this issue next.

Explaining the Surprising Empirical Relationship between Monetary-Policy Uncertainty and the Exchange Rate

What model could explain the empirical regularity that an increase in the volatility of domestic monetary-policy shocks causes the domestic currency to strengthen? It turns out that a standard two-country extension of the New Keynesian model captures this fact quite well, at least qualitatively. This is a significant finding of the paper under review which the authors do not highlight enough (a problem that hopefully will be fixed in the published version). The theoretical model presented in the paper has the following ingredients:

- Two countries.
- Two goods.
- Complete asset markets.
- Sticky prices.
- Epstein-Zin Preferences.
- Volatility and “level” shocks to the nominal interest rate, the inflation target, and productivity.

As it will become clear shortly, Epstein-Zin preferences are not essential for explaining the empirical link between monetary uncertainty and the exchange rate.

Figure 2 displays the impulse response of the real exchange rate to an increase in the monetary-policy volatility shock. Compare this figure with its empirical counterpart shown in figure 1. Quantitatively, the

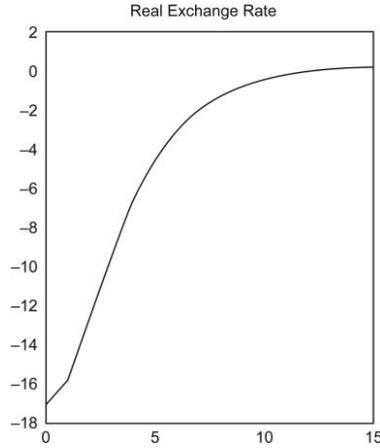


Fig. 2. The impulse response of the real exchange rate to an increase in the monetary-policy volatility shock.
 Source: Benigno, Benigno, and Nisticò (chapter 5, this volume).

model is far from matching the data. For example, the appreciation of the exchange rate predicted by the model is about ten times larger than its empirical counterpart. Also, for most countries, with the exception of Canada and Japan, the response of the exchange rate has a U shape, whereas the theoretical impulse response has a semi-inverted U shape. Finally, in the case of Japan, the empirical and theoretical responses have opposite signs.

But the theoretical response is qualitatively a success, for it captures the counterintuitive empirical finding that in response to an increase in monetary-policy uncertainty the US dollar appreciates. One cannot help but wonder what theoretical mechanism is responsible for this unexpected result. Unfortunately, the version of the present paper that I read does not present an intuitive argument that I find transparent (hopefully this will be not be an issue in the published version). So allow me to present my own intuition. Start with the following well-known Euler equation for pricing dollar-denominated, state-noncontingent bonds:

$$1 = (1 + i_t)\beta E_t \left\{ \frac{U'(C_{t+1})}{U'(C_t)} \frac{1}{\pi_{t+1}} \right\},$$

where i_t denotes the nominal interest rate, C_t denotes consumption, π_t denotes the gross rate of inflation, $\beta \in (0, 1)$ denotes a subjective discount factor, and U denotes the period utility function. This familiar

expression can be interpreted as Fisher's equation stating that the nominal interest rate must equal the sum of the expected rate of inflation and the expected real interest rate.

I will now introduce three simplifying assumptions. First, assume that

$$\frac{U'(C_{t+1})}{U'(C_t)} = 1.$$

This condition will hold in a flexible-price version of the model presented in the paper in which all real shocks (such as productivity shocks, preference shocks, etc.) are shut off. In such an environment, monetary shocks do not affect real quantities in general and consumption in particular. The second simplifying assumption I will introduce is

$$\pi_t = S_t/S_{t-1},$$

where, as mentioned earlier, S_t denotes the nominal exchange rate, defined as the number of US dollars needed to purchase a unit of foreign currency. This assumption essentially states that purchasing power parity (PPP) holds. It will be satisfied in a small open-economy version of the present model with flexible prices and no home bias. Here, the small open-economy feature allows us to ignore foreign inflation in stating the PPP condition, since the focus is on domestic shocks. Finally, the third assumption states that monetary policy is characterized by a Taylor-type interest-rate feedback rule of the form

$$1 + i_t = \phi(\pi_t), \quad \phi' > 1.$$

Combining these three assumptions with the above Euler equation yields

$$1 = \phi(S_t/S_{t-1})\beta E_t \left\{ \frac{1}{\pi_{t+1}} \right\}. \quad (1)$$

I will now conjecture that when monetary policy becomes conditionally more volatile (i.e., when the variance of i_{t+1} conditional on information available at t goes up), next period's inflation rate, π_{t+1} , also becomes conditionally more volatile. That is, $\text{var}_t(\pi_{t+1})$ rises. Further, I will assume, rather heroically, that this increase in inflation volatility occurs in a more or less mean preserving fashion. Then, by Jensen's inequality, we have that the conditional expectation of $1/\pi_{t+1}$ must also increase. Finally, by equation (1), the rise in $E_t\{1/\pi_{t+1}\}$ must be associated with an appreciation (a fall) in the nominal exchange rate S_t .

In words, what happens in this model is that the rate at which dollar-denominated assets gain value due to inflation $1/\pi_{t+1} - 1$ (a negative rate when inflation is positive) increases on average with the level of uncertainty. As a result, as the level of monetary uncertainty rises, holders of nominal assets (e.g., treasury bonds) demand a smaller compensation to maintain them in their portfolios. Thus, given the real interest rate, a rise in monetary uncertainty causes the nominal interest rate to fall. In turn, if the monetary authority follows a Taylor-type interest-rate rule, the fall in the interest rate must be associated with a fall in inflation. Finally, if PPP holds, the fall in domestic prices, given foreign prices, must be linked to an appreciation of the domestic currency.

Real Activity: The Disinvited Variable

Both the VAR and theoretical models feature measures of domestic and foreign output. Yet the predicted effects of uncertainty shocks on aggregate activity are reported neither for the empirical model nor for the theoretical model. Instead, the paper focuses exclusively on the effects of volatility shocks on financial variables. This choice is unfortunate because it might be interpreted by some readers as meaning that uncertainty shocks are strong enough to move financial variables but lack the traction to lift real variables such as output, consumption, investment, and employment. This is, of course, not the case, as documented by a number of recent related studies (see, e.g., Fernández-Villaverde et al. 2011; Bloom 2009). Hopefully, the published version of the present paper will remedy this important omission.

Conclusion

This is a promising project. It has the potential to deliver a first step toward understanding the international effects of uncertainty shocks both empirically and theoretically. But there remain a number of issues to be addressed. Among the most important ones are the identification of uncertainty shocks. Ideally, this issue will be tackled by a direct estimation of the proposed theoretical DSGE model. A second pending issue is a more satisfactory investigation of the theoretical model's ability to match the actual data, especially the observed effects of uncertainty shocks. A third priority is to put more effort into developing intuition for the many analytical results contained in the paper. Finally, a revised

version of this paper should provide texture to the exposition. The version I reviewed reads mechanical and monotone. All results, large and small, are given the same emphasis and space. A hierarchy of results is highly needed. Overall, I believe that, once finished, this paper has the potential to become an important contribution to the existing related literature.

Endnote

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