

## **Taking a DSGE Model to the Data Meaningfully**

The paper challenges the current practice in empirical macro to fit highly stylized theoretical DSGE models almost one-to-one to the data. Using the well-known model of Ireland (2004), the authors show that almost all hypotheses that can be derived from the theoretical model are strongly rejected by the data once they are allowed to speak freely. Moreover, the authors propose to carefully analyze the cointegration and adjustment properties of the data set at hand in order to derive stylized fact that have to be met by empirically successful theoretical DSGE models.

### *General comments:*

I am highly sympathetic to the view that empirical modeling should carefully take the properties of the data into account and should not thoughtlessly impose a theoretical model structure on the data that is much too stylized to fit observed (lagged) adjustment processes or even long-run relationships. On the other hand, the current trend in the literature towards (applied) DSGE modeling instead of VAR and, in particular, cointegration modeling did not start without reason. In my view, it is simply a reaction to the problems of this approach, and especially the latter part of the paper illustrates two of them:

1. Empirical modeling without theory is probably as bad as empirical modeling without data. This concerns all modeling stages like the choice of the cointegration rank, the identification of the cointegration vectors and the driving forces (common trend), and the tests for exogeneity and stationarity. Exclusively relying on successive tests with (often) unknown size and low power for the small sample sizes at hand appears to be not very attractive either. While such an approach delivers a well-specified model by means of all criteria employed, the empirical results are often so difficult to reconcile with our theoretical and empirical priors that I cannot see how to use them as stylized facts for theoretical model building as claimed by the authors. Just a few examples:
  - a. If we believe in a Cobb-Douglas function and assume it is stationary, then we expect parameter estimates that reflect the labor and capital share in the economy, which are typically around  $2/3$  and  $1/3$ , respectively. Hence, an estimate of the labor share of 0.39 in the second cointegration relationship is not believable unless there are very good reasons in favor of this estimate.
  - b. The difficulty to identify the second cointegration vector stems from the lack of a good economic prior once all the simple cointegration relationships predicted by the PI model are rejected. The interpretation of the vector as it emerges from the analysis is tentative at best.
  - c. The exclusion of the linear trend from the cointegration space in the second sub-sample is difficult to interpret. Why should there be trends in the 60s and 70s but not afterwards? What has changed?
  - d. Similarly, the estimation and interpretation of the common trends is again tentative. In particular, based on the weak exogeneity of consumption the authors suggest that the business cycle is mainly demand driven. However, the theoretical model does not preclude productivity shocks to affect consumption in the long run. In addition, the

authors admit that they did not orthogonalize the common trends and give good reasons for not doing this. But then I do not see what we can really learn from section 7.

2. My personal experience with cointegrated VAR models is that they are very sensitive to all kinds of things (sample period, lag order, deterministic components). This is particularly true for the trace tests and, hence, the determination of the cointegration rank which is not an easy task anyway as the discussion related to Table 3 shows. I guess this is one of the reasons why many researchers are very cautious with these tests and impose the rank predicted by their model if they consider cointegration restrictions at all. Therefore, one should be very careful when interpreting the results and report some robustness analysis. This is particularly important when the remainder of the analysis depends on the cointegration restriction being made correctly.

What does all this mean for the paper in general? I very much like the (cointegrated) VAR approach to test the implications of theoretical (DSGE) models. In the paper, it is demonstrated convincingly that the PI model is strongly rejected by the data in virtually all dimensions. However, I see much less value added in theory-free empirical modeling to produce stylized facts that are both highly debatable and sensitive. Therefore, I would like to suggest that the authors defend their sections 6 and 7, reconsider it (e.g. base it on a more general theoretical model to guide the specification and interpretation), or cut it at all.

*Specific comments:*

1. Is it essential for the PI model that the labor-augmenting technical progress be a linear trend? I guess the model goes through almost unchanged when it is specified as a random walk with drift. This would correspond to the later finding of cointegration rank 2. (p. 5/6)
2. I could not access the website stated in footnotes 2. (p. 6)
3. Should (11) not be an expectational difference equation? Or is it the solution to it?
4. Your measure of the capital stock seems flawed. Clearly, the capital stock is not observable. Sometimes people take some derived measures, like the one based on the perpetual inventory method, and buy all their implicit assumptions, which may not be compatible with the theoretical model at hand. But it appears that you use capital formation (=investment) which is a flow variable.
5. Apart from modeling convenience, why do you want to avoid broken linear trends in the data? There is considerable literature claiming that there are trend breaks in US GDP. (p. 13)
6. You exclude the linear trend from the second subsample. How sensitive are the results to this restriction? Perhaps the cointegration parameters of the Cobb-Douglas function become more in line with theory when leaving the trend in. (p. 19)