

Evaluating An Estimated New Keynesian Small Open Economy Model

by Adolfson, Laséen, Lindé, Villani

Discussion
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Impressive paper! Here is my recap:

The goal is to build **the model** of the Swedish economy, i.e. a tool which is at the same time:

1. able to examine policy-related questions ...
2. ... with strong micro-foundations and theoretical underpinnings ...
3. ... and whose dynamics are rich enough to be data consistent and trustworthy in forecasting

Wow !!

To this end:

- A Small Open economy DSGE (foreign economy exogenous)
- Habits, Calvo prices & wages, adjustment costs in capital
- Indexation to current inflation target + past inflation rate
- 4 Phillips curves: domestic, imports C, import I, exports
- Modified UIP to better match the data (gets the hump-shaped response of RERs to MP shock and the negative correlation btw expected depreciation & RP)
- Explicit modelling of the MP switch in 1993

- 15 variables used in the Bayesian estimation stage (not pre-treated, in levels)

Results

Some modelling choices turn out to be correct ex-post :

- The modified UIP better matches the data
- The responses of RER to MP shock are more realistic (hump shaped)
- The data confirm the existence of a MP break

Plan of the discussion

- ❑ Another attempt to build “The Great Model”
- ❑ I want to highlight the main strengths of this particular enterprise, ...
- ❑ then raise some questions and suggestions for the authors ...
- ❑ and perhaps come back to “The Big Question” lurking in the back of all this literature:

Is there enough information in the data to draw inference about parameters of such big models?

What impressed me about the paper ?

Very serious model validation

- relative fit (marginal likelihoods)
- misspecification test (DSGE as prior for VAR)
- forecasting
- impulse responses (of RER to a MP shock)

QSN1: is this the right direction ?

- The UIP modification is not micro-founded
- This blurs the distinction between the dynamics that the model is able to capture via its “structural” (micro-founded ?) part and the one captured via its error (unexplained) terms
- Perhaps until we have a theoretical way to justify the modification we should not blur the Ireland distinction between structural dynamics and error dynamics

QSN2: was that enough validation ?

- Sometimes it is not clear what is really needed and what is not needed to achieve The Great Model
- Let me go through some modeling choices that perhaps deserve more investigation/clarification

Too many 'Calvinists' ?

- Calvo pricing with dynamic indexation:
 - cumbersome theoretically
 - empirically rejected in micro-data on pricing behavior
 - extremely difficult to estimate: at best only one parameter between the degree of indexation and the Calvo coefficient has some hope to be identified from the data
- Nonetheless, this modeling choice is pervasive (4 Phillips curves + a labor supply ↗ a total of 10 parameters)
- Why not dropping some indexation scheme, in wages or in domestic goods? Inertia in the remaining sector should suffice.
- How much we loose in term of fit ?

Imperfect pass-through

The LOOP holds at the border; then importing & exporting firms face nominal rigidities

- empirically rejected: these sectors are **very flexible** in the data but they come out to be **very rigid** in the model to match the low pass-through that is in the data
- theoretically troublesome: implies a unity correlation btw exchange rates and terms of trade (Obstfeld-Rogoff 2000)
- Other mechanisms can be invoked to induce a low pass-through: trade costs, distribution sector intensive in local inputs, active price discrimination etc...

Estimation with raw data

- A single stochastic trend in order to work with trending (i.e. unprocessed) data. This allow to extract information about parameters that affect the steady-state (and/or the great ratios)
- Not what they do. They calibrate the steady-state great ratios and then are forced to eliminate the “excessive trend” from some variables (imports & exports)
- Isn't this procedure picking up troubles from both approaches without any of the benefits?
- Why they decided to go this way?

Again on model misspecification

- Their complicated procedure (finding the **optimal** amount of relaxation of the **cross-equations restrictions** coming from the DSGE) does not tell the modeler where the misspecification lies. It is a global misspecification test
- Proposal: why not looking at the historical innovations in the structural shocks that were needed to retrace the sample data and check whether they pass a standard test of normality?
- Wouldn't that be a **direct test of misspecification** but “equation-by-equation”?

More generally (philosophical questions)

- To think seriously about **model validation** seems to raise more questions than it answers (since many more dimensions than the two considered in the paper are questionable)
- Bayesian techniques are dangerous in this respect: they seem to trade **rigorous** model validation for **dubious** parameter identification (model validation under different priors is a very tricky game)
- At the end a doubt remains: estimation of these big models using Bayesian techniques and tight priors isn't just **sophisticated calibration**? (Canova)

Conclusions

- Great empirical contribution
- Modification of the UIP fits better the data (and the particular IRF considered)
- Very thorough way to validate structural model
- Might be a first step toward addressing the big questions