

Referee report on

The New Keynesian Phillips curve tested on OECD panel data

by Roger Bjørnstad and Ragnar Nymoen.

The paper analyzes the role of the forward looking term in the open economy New Phillips curve (OE-NPC)

$$\Delta p_t = a^f \Delta^e p_{t+1} + a^b \Delta p_{t-1} + bws_t + cx_t \quad (1)$$

for a panel of 20 OECD countries; the conclusion is that the expected value of inflation plays no role when the error correction terms of the Imperfect Competition Model (ICM) of Sargan (1980) are included in the equation.

Major Comments

i) Balanced equations and cointegration. In line with the results in Table 1 you describe p_t , ulc_t and pi_t as being $I(1)$ variables and the ICM motivates the assumption that $p_t - \mu_1 ulc_t - (1 - \mu_1)pi_t$ in (8) is the stationary long run equilibrium of the model. Because $p_t - \mu_1 ulc_t - (1 - \mu_1)pi_t = p_t - pi_t - \mu_1(ulc_t - pi_t)$, if one assumes that $ulc_t - pi_t$ and $p_t - pi_t$ (or $ulc_t - p_t$) are stationary then it follows that (8) is stationary but the converse is not true, that is the ICM is not sufficient to claim that (9) is balanced. It seems to me that you have in mind the two stationary long run relations $ulc_t - p_t$ and $ulc_t - pi_t$ with (8) being a linear combination of them. If this is correct than one should motivate them in more detail.

ii) The role of (7). As far as I can see one can present (9) as a reparametrization of (2) through the identities in (3) and (4) and propose the same analysis as testing the restrictions on the coefficients of (9) implied by the NPC theory (as you say below (9)). This is motivated by the following observations: the NPC model already incorporates the idea that firms set prices as a mark up on marginal costs (Calvo pricing in monopolistic competition) and this seems to capture already the idea of (7). Secondly, it is not that clear to me why one should believe the dynamic part of the NPC is the true one, and thus include it ad hoc in the ICM, when the model by Clarida et al.(1999) is abandoned. The presence of expected inflation in (2) is a property of the economic model and not of the data. If one wants to analyze the data one could think of starting from a general specification and then test the economic model as a restriction of it.

iii) Encompassing. I have some doubts regarding the relevance of the concept of encompassing in the present paper and the consequent claims about the relation between NPC and ICM. The paper by Govaerts, Hendry and Richard (Journal of Econometrics 63, 1994) contains a detailed exposition of the encompassing methodology in dynamic models which contain variables which are believed exogenous by the investigator. At the top of page 253

it is stated that in order to derive the Wald Encompassing Test (WET) statistics for the model M_1 , an auxiliary process (the completing model M_c) is needed for the nonmodelled variables. At the bottom of the same page one finds that

Encompassing methodology in conditional dynamic models requires that statistics are explicitly derived under the joint $M_1^c = (M_1, M_c)$ and not just under M_1 . Because the completing model M_c is instrumental in the analysis, it influences the values of the WET statistics and hence the outcomes of the tests. Consequently, a careful choice of M_c is required. (Govaerts et al., JoE 1994(63), page 253-4.)

Is this analysis not relevant in your case? Why is it so?

iv) Same slope coefficients. I believe that the assumption of homogeneous responses in (10) should be motivated in more detail. It seems to me that the countries included in the dataset are heterogenous in many respects, size, inflation dynamics, unemployment dynamics, current account positions, etc, and it is not so clear why the response of the economy to some shock be the same. For example, one could think that the effect of a change in the price of oil affects a net seller differently from a net buyer.

Minor Comments

p.3, l.3 : am I correct in interpreting stringent theoretical derivation as a synonymous of micro-founded? How would you then reconcile the fact that the trade off between inflation and output gap in the model of Clarida et al.(1999) is absent if the ad hoc assumption of $AR(1)$ disturbances is deleted?

p.3, l.15 : by scientific inference do you mean reliable? that is, that the assumptions of the statistical model are to be checked before inference from the model can be discussed?

p.3, l.-18 : shows.

p.5, l.15 : please move the definition of ws_t below eq.(1).

p.7, (4) : is this an identity?

p.8, l.-6 : $H_0^b : \beta_4 = -\beta_2$.

p.8, l.-6 : the phrase *the only difference...* is unfinished.

p.9, l.-18 : could you please clarify the statement *There is a separate...*? Does it mean that each country has different variables in x_t ?

p.9, (10) : why the price of oil has subscript i ? Does the price differ among countries?

p.13, Table 3 : I would suggest to delete the subscript i from the names of the variables and display p-values instead of standard errors (same in Table 4).

p.13, l.-2 : significant.