

Comments for authors

“The New Keynesian Phillips curve tested on OECD panel data”

The ‘hybrid’ version of the New Keynesian Phillips Curve (NKPC) popularized by Galì and Gertler (1999) and Galì et al. (2001), is at the center of vast empirical scrutiny, with a twofold objective: first, to understand whether forward-looking pricing models can account for the observed persistence in inflation; second, to assess the role of marginal costs as the single cause driving force of inflation.

This paper belongs to the class of contributions that casts doubts on the reliability of the NKPC as a model of inflation dynamics which features both properties. It proposes the empirical investigation of the NKPC on a panel data set of OECD countries, keeping an eye on the encompassing principle as a tool for evaluating whether many typical findings obtained from the GMM estimation of the NKPC can be explained in terms of a rival model. Estimation is based on the hypothesis of homogeneous slope coefficients across countries.

The authors show that the typical findings that characterize the GMM estimation of the NKPC at the single country or macro area level, can be replicated in the panel of OECD countries when the pooled estimation is applied. In particular, the pooled estimates apparently suggest that the model fits the data well, and with forward-looking component dominating the backward-looking component (albeit with insignificant slope coefficient). However, when the NKPC is evaluated against a rival model, which is in this case represented by the Imperfect Competition Model (ICM), and the underlying hypotheses are opportunely tested, the previous results reveal to be fragile. The typical findings obtained from the GMM estimation of the NKPC can be explained in terms of the GMM estimation of the ICM, and in particular it is shown that the inflation forward-looking component of the NKPC can be replaced with error-correcting terms which are well motivated under the ICM paradigm.

I am quite sympathetic with this paper, in particular with its message that the typical results characterizing the ‘limited-information’ estimation of

the NKPC at the single country or macro area level, are to be expected also when a (pooled) panel approach is pursued, if the model is seriously flawed. In a sense, this is an expected result. Another worthy point is the explicit recognition that the time-series involved in the analysis can be approximated as nonstationary cointegrated processes, which is uncommon in the literature.

There are some points that the authors should clarify in the paper. For instance, I am perplexed about the premise through which estimation in the paper is carried out, i.e. the claim that since the microfoundations of the NKPC abstract from the historical idiosyncrasies experienced by the countries in the panel, the econometric implications of the model relative to the implications of a rival model can be investigated through a pooled estimation approach assuming homogeneity. Actually, the pooled estimation approach presented in the paper can be interpreted as a sort of counterfactual experiment: what happens if countries are homogeneous, and how do we interpret the econometric evidence in this case? But as I explain in detail below, I would not dismiss the hypothesis of heterogeneous (across countries) coefficients in the NKPC as at odds with the theoretical microfoundations of the model; some recent literature recognizes that heterogeneity is an issue that must be properly addressed. In principle, if it is reasonable to assume a common discount factor across OECD countries, there is no reason that the elasticity of substitution among differentiated goods, the elasticity of firms marginal costs to their own output, and the percentage of prices which are not reset optimally, should be the same across countries with very different market institutions. If one accepts the hypothesis of heterogeneity, the econometric estimation and evaluation of the NKPC becomes more problematic than presented in the paper.

Moreover, I believe that although the encompassing principle is a valuable and powerful tool for comparative model evaluation, the way it has been applied in this paper may be misleading. More precisely, it seems that the encompassing analysis presented by the authors has been carried out in a ‘limited-information’ framework, whereas by its very nature, encompassing works in a ‘full-information’ context. I’m not claiming that the analysis presented in the paper is wrong, but a note of caution would be necessary.

Below I have reported a list of detailed comments where I expand my remarks.

Detailed comments

1 - The authors motivate the idea of imposing coefficient homogeneity in their panel estimation as a consequence of the lack of any historical/institutional consideration upon which the theory behind the NKPC is built. The implicit assumption is that the NKPC reads as a ‘representative’ model of inflation dynamics, which should hold irrespective of the historical and/or institutional context experienced by the single countries in the panel. (Actually, as far as I know, almost all theories - especially those involving expectations - do the same ... it is the econometrician that should enrich, when motivated by facts, and when technically possible, the original specification in order to account for possible country specific factors and/or relevant episodes).

A recent work by Imbs, Jondeau and Pelgrin (2007) (hereafter IJP), investigates the issue of aggregation of NKPC-type equations, and shows that if pricing is heterogenous (across sectors), any estimation that ignores the issue is flawed. Moreover, they show that under certain conditions, imposing homogeneity results in overestimating the backward-looking component, and in underestimating the importance of marginal costs. If the results of IJP can be extended from the case of sectors to the case of countries, they would be consistent with the basic findings of the paper. At the same time, however, they would also suggest that if heterogeneity is an issue, then the empirical assessment of the NKPC relative to potential rival models in a panel data framework should be thought of differently.

Monacelli (2005) recently shows that when there is incomplete pass through of foreign prices in domestic currency to domestic currencies, extra terms representing deviations from the law of price and differences between the domestic and foreign consumption basket enter the NKPC, and this might induce heterogeneity across countries. Likewise, adopting a global perspective, also Dees et al. (2008) argue that heterogeneity in the NKPC is an issue and address it empirically.

The authors clearly state that their pooled panel data regression is valid only under the assumption of homogeneity (in slope coefficients but also in the other coefficients), and argue that the potential bias can be believed to be small, as the estimated coefficients display about the same magnitudes found in other studies (based on single countries). This is a good argument in favour of the approach followed in the paper, nonetheless, the authors

might relate their analysis and results to the findings of other authors based on heterogeneity, stressing analogies and differences.

2 - Part of the conclusions of the paper are based on the following idea: on the one hand, the NKPC can potentially parsimoniously encompass the rival ICM, but on the other hand it is also possible that the ICM class of models (Nymoer, 1991) can successfully explain the salient features characterizing the GMM estimation of the NKPC. In particular, the paper shows that the forward-looking component of the NKPC can be replaced with the equilibrium correction terms which are consistent with the ICM class of models.

That the NKPC can potentially parsimoniously encompass the rival ICM, means that (a) the NKPC is nested within the ICM, and that (b) the NKPC encompasses the ICM (Hendry, 1995, Chapter 14). In my view, (a) is derived in the wrong way.

Let me start from Eq. (6). Eq. (6) is an open-economy version of the NKPC (OE-NKPC hereafter), and is obtained from the ‘standard’ hybrid NKPC by adding a set of open-economy driving variables (x_t), and by re-writing the model through the following steps: (i) it is exploited a simple identity through which the wage share is replaced with real unit labour costs $ulc_t - pd_t$, where pd_t is the domestic price level; (ii) it is used a relation (identity ?) which defines the price level, p_t , as a weighted average of the domestic price level, and the import price level

$$p_t = \gamma pd_t + (1 - \gamma)pi_t \quad (1)$$

with time invariant weights $0 < \gamma < 1$ and $(1 - \gamma)$. Actually, the exact relation (identity ?) (1), carries relevant consequences, as it introduces a new parameter in the model, which plays a non-negligible role in the analysis of the OE-NKPC presented by the authors. For instance, in comparing Eq. (6) with the rival ‘expectations-augmented’ ICM of Eq. (9) (henceforth I will denote this model with the acronym E-ICM), the authors claim that the rejection of the hypotheses H_0^a and/or H_0^b is inconsistent with the OE-NKPC, but is consistent with the E-ICM (provided that $\beta_1 > 0$, and $0 > \beta_2 > -\beta_1$). I question the point that the hypotheses H_0^a and H_0^b really capture the implied OE-NKPC restrictions. I have two arguments.

First, the relation (1), and the implied additional (structural?) parameter γ , are not direct consequences of the microfoundations: the hypotheses H_0^a

and H_0^b (based on γ) arise from the combination of the definition (1) with the definition $ws_t = ulc_t - pd_t$, and with the structure of the OE-NKPC equation. In my view, a statistical test for H_0^a and/or H_0^b is not a test for the OE-NKPC against the E-ICM, but a test for detecting whether the original formulation of the OE-NKPC in Eq. (2) embodies the additional restrictions implied by the ‘exogenously given’ relation (1).

Second, the E-ICM in Eq. (9) is treated as the constructive alternative to both the OE-NKPC and the backward-looking ICM class of models. Eq. (9) is obtained assuming that the dynamic part of the OE-NKPC is the true one, and then augmenting the model with the same variables entering the ICM. Assuming that Eq. (9) is the constructive alternative to both the OE-NKPC and the backward-looking ICM class of models, the analysis should be based on the comparison between the implications arising from the (determinate, if any) reduced form solution associated with Eq. (9), with the implications arising from the (determinate, if any) reduced-form solution associated with Eq. (6). In principle, this would require the full specification of the stochastic processes generating the variables in the vector of forcing variables $Z_t = (ulc_t - p_t, ulc_t - pi_t, x_t)'$ (Pesaran, 1987).

To sum up, my remarks are the following: (r1) Eq. (6), which hinges on a set of identities, and Eq. (9) seem to represent the same model; (r2) when models with forward-looking behaviour are involved, it is extremely difficult to carry out an encompassing exercise without accounting for the full specification of the system, and ignoring the implied set of cross-equation restrictions.

3 - The results in Table 3 show fairly well that the error correcting terms $(ulc_{i,t-1} - p_{i,t-1})$ and $(ulc_{i,t-1} - pi_{i,t-1})$ serve as proxy of $\Delta p_{i,t+1}$ (model M2). A simple argument which might help to explain the results of Table 3 could be following: according to the ICM class of models

$$\begin{aligned}
 E_t \Delta p_{i,t+1} &= \alpha_1 (ulc_{i,t} - p_{i,t}) + \alpha_2 (ulc_{i,t} - pi_{i,t}) \quad (+\text{other lags and variables}) \\
 &\equiv \alpha_1 (ulc_{i,t-1} - p_{i,t-1}) + (\alpha_1 + \alpha_2) \Delta ulc_{i,t} + \alpha_1 \Delta p_{i,t} \\
 &\quad + \alpha_2 (ulc_{i,t-1} - pi_{i,t-1}) + \alpha_2 \Delta pi_{i,t} \quad (+\text{other...}).
 \end{aligned} \tag{2}$$

Assuming that (2) is the correct model through which one-step ahead forecasts of inflation are formed in the economy, and using this expression in Eq. (2) (of the paper) and rearranging terms, the resulting (estimable) equation can potentially account for almost all results summarized in Table 3.

4 - As robustness check, the authors could also test in Table 1 whether a unit root is in the variable ws_t . Indeed, from the definitions given in the paper, if $ulc_t - p_t$ and $ulc_t - pi_t$ are $I(0)$, by construction ws_t should be expected to be $I(1)$.

Some authors have recently found that Δp_t and ws_t can be approximated as $I(1)$ cointegrated processes at the macro area level: formally such relation would read as a polynomially cointegrated relation. Could a potential cointegration relation between Δp_t and ws_t be re-cast and interpreted within the ICM paradigm?

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