Report on
Global factors, Unemployment Adjustment and the Natural Rate

The present paper starts by estimating and identifying the common factor that drives OECD unemployment rates over the period 1960-2002. Then this measure is used in order to estimate and to study subsequently a model of unemployment adjustment and a reduced-form (Phillips-based) inflation dynamics. In the former, the authors study whether or not labor market institutions can influence unemployment through different channels: (i) the speed of adjustment, (ii) the domestic component of the equilibrium level of unemployment, (iii) the long-run effect of the global factor on the equilibrium level of unemployment, and (iv) the impact of shocks to global factor on the change in unemployment. Overall, institutions seem to influence adjustment to the global factor. However, there is no influence on the natural rate—only the global factor shifts the equilibrium level. In the latter, the authors provide evidence that the common factor performs well as a measure of the natural rate in a Phillips curve.

Overall, the paper is interesting—the questions are of interest and may deserve some more future research. At the same time, three points are of main concern. First, the key point of this paper is to estimate and interpret one common (global) factor for OECD unemployment. In doing so, authors take into account cross-sectional dependence and outline the role of the global dimension. However, is this factor enough to explain the natural (potential) unemployment in each OECD country? or How does it explain country specific unemployment? How can it compare with standard measures of potential unemployment? In a policy perspective (given usual caveats), what does it mean? The reported evidence suggests that institutional or country specific characteristics may only alter the speed of adjustment but not the equilibrium level. This is rather challenging, especially given the results of the unemployment literature. In other words, we would like to be better convinced that this is the correct approach to follow. Moreover, there are some caveats in the estimation and the identification of this common factor, which may limit the interpretation of the results (see next section).

Second, too many questions are addressed in the same paper, especially regarding the implications of the common factor in Sections 3 and 4. To some extent, the fact that institutional variables matter in section 3 is really interesting per se and there is already a large number of challenging issues that have to be addressed. Instead of doing so, the authors pursue the analysis in section 4 with a somehow "orthogonal" application (institutional variables do not matter). However, we would like to see more in Sections 2 and 3. Moreover, results are not enough explained and documented. Third, the paper is sometimes silent on important methodological issues. All in all, either the authors should better describe the techniques and the results in both applications (hereafter the model of unemployment adjustment and the model of inflation dynamics) or they should only concentrate on one model.

In the sequel, I discuss (1) the determination of the common factor, (2) the model of unemployment adjustment and the corresponding empirical results, and (3) the model of inflation.

1 The determination of the common factor

In the first section, the authors proceed in two steps. First, they estimate the common(s) factor(s) of OECD unemployment rate (and the investment rate) using the PANIC approach developed by Bai and Ng (2004). In particular, the authors retain the first principal component which explains 70% of the total variation in unemployment and 58% of the total variation in investment. In a second step, the authors identify this common factor as being a measure of global expected returns. Obviously their first section is crucial for the rest of the analysis since this measure is used in both applications. Regarding the first step, my main comments are:

1. The PANIC approach needs to be clearly presented and detailed.

2. The authors do not test whether the factors (respectively the idiosyncratic component) is I(1) (respectively I(0)). Panel unit root tests can be implemented within the PANIC approach.

3. The authors report four principal components. However, there are neither explanations nor information criteria (ICP, PCP, etc) to justify this number (see Bai and Ng, 2002).

4. Latent factors, which are estimated from a small number of indicators (as in the paper), are generally imprecise. In theory, consistent estimation of the latent factors cannot be achieved under the traditional assumption that T is large and N is fixed, or vice versa. This is really an issue in the present paper since the dataset has a small number of cross-sections (21) and the time dimension is not so large. Consequently, one may wonder whether the first principal component is consistently estimated and is not a noisy measure of the "true" latent factor. If the latent factor is not enough precise, it may affect subsequent results.

5. The consistency issue is important at least when the authors compare the first PCs for unemployment and investment. Cointegration tests are conducted on the two constructed variables whose construction depends on estimation in a previous step. In general, the use of consistent estimates from a previous round will not cause a problem with consistency in later stages. However, it will generally add noise to the problem that appears in the asymptotic covariance matrix of the later-stage estimators. Moreover, there is a small sample issue ($T = 41$) and the power of the Johansen's tests can be questioned. In that respect, it is difficult to have a firm conclusion regarding the existence of cointegration relationship (or the existence of the (1,-1) restriction on the cointegrating vector).

6. If the main goal is to provide that the first PCs for investment and unemployment are identical, the authors should not calculate the factors for unemployment and investment independently. It would be interesting at least to "compare" the first PCs reported in the paper and the first principal component of an extended statistical model (e.g. when imposing a shared factor structure).

7. To what extent (ceteris paribus), this common factor component of unemployment rate is different from or compare with individual natural unemployment rate (e.g. a filtered measure of unemployment rate or other measures used in the literature)?

8. It would be interesting to report a figure that represents (for selected OECD countries) the unemployment rate and the common factor.

Regarding the identification of the first principal component, the authors only focus on "global expected returns" and especially a proxy of this unobserved variable (or a related variable), namely the world real rate of interest. However

1. The world real rate of interest is still "unobserved" and should be constructed as the weight average of the real rate of interest in the G7 countries. This methodology is questionable since it aims at explaining a latent factor (the first principal component) with an imperfect and partially unobserved variable.

2. Assuming that this measure is a good proxy of global expected returns (and that results are robust to other definitions of the world real interest rate), there is no formal testing procedure(s). In that respect, the authors could use the methodology developed by Bai and Ng (2006), e.g. to consider statistics in order to determine if the observed and the latent factors are roughly the same. Moreover, they could use and test a larger set of variables, which may explain this common factor.

3. Consequently, there is not enough convincing arguments to justify why the common factor of unemployment is mostly driven or is a measure of global expected returns.

All in all, this section deserves more work and careful analysis of the empirical results. As a final remark, the theoretical discussion (last paragraph) goes far beyond the statistical model used in this section.

2 A model of unemployment adjustment

In sections 2 and 3, starting from the model of Nickell (1985), the authors propose a model of unemployment adjustment in which labor market institutions can enter the specification through four channels. The main idea is that the short-, long-run coefficients and the speed of adjustment coefficient might be time- and country-varying through the role of labor market institutions. While Eq. 12 is somehow ad hoc, these channels might be operative and there is some interest to study them. Overall, the reported empirical evidence suggests that these channels are not all "significant".

My main comments concern the data, the specification, and the empirical results.[1]

1. The authors use the database of Nickell and Nunziata and extrapolate the final values. Some "summary statistics" should be reported and the extrapolation method

[1] Regarding the model (Eq. 12), higher-order dynamics can be considered by using the contribution of Pesaran (1991).

should be explained. It would be useful to have an appendix that describe the variables used and their definition (even if the reader has still the opportunity to read the aforementioned paper!).

2. One main concern is that these institutional variables often display not enough variability. As a result, they might be insignificant and multicolinearity could be an issue. Does it matter here? My own experience (with the OECD database of institutional indicators) is that it matters!

3. As pointed by the authors, there is the obvious problem that labor market institutions are likely to be endogenous. In order to circumvent this issue, the authors propose to use a random effects panel estimator. However, the results are briefly discussed and the choice of the specification (lagged institutional variables, global factor and unemployment) is not clear (results are not reported). Moreover, the presence of both the global factor and unemployment is troublesome, especially given that the first is a generated regressor. It is difficult to be convinced that endogeneity is not a problem.

4. Regarding the specification, the main question is to know whether there are strong reasons to believe that certain coefficients are time- and country-varying. Note that this interpretation can be debated since the empirical strategy seems to be more a model with interaction variables than a formal variable coefficients model (see further). Two dimensions are examined in Section 3. On the one hand, individual estimates (Table A2) provide evidence that there is heterogeneity across OECD countries regarding the speed of adjustment, etc. However, there is not a one-to-one relationship between individual heterogeneity and institutional variables. On the other hand, Tables 2 and 3 report weak evidence that there is substantial variation over time (using the random coefficient estimator of Swamy or a fixed effects estimator). Given these two sets of results, I don't see enough convincing evidence that these coefficients are time- and country-varying. This is a critical issue since this is one the main result of this paper. In that respect, I would suggest the following (imperfect) procedure. First, using individual estimates, I would regress (cross-section regression) in the second step the individual coefficients on the average of institutional variables (after controlling for standard deviation estimates).

This is rather a "crude" exercise but it could give a rough (if any) idea about the relationship between individual estimates and institutional variables. Second, assuming there is no heterogeneity (strong requirement!), I would estimate a model with coefficients that evolve over time (Zellner, Hong, and Min, 1991) in a panel context (Hsiao, 2003). However, this is more complicated here in the sense that the authors use a dynamic model.

5. Regarding Table 4, the model is estimated using a nonlinear least squares procedures. From my own understanding, the authors use interaction variables (as for instance $x_{it}f_{t-1}$) and then estimate non-linearly the coefficients of interest. Is that correct? Against this procedure, an alternative would be to use a general formulation of stochastic-parameter models with systematic components or to write explicitly (in separate equations) coefficients as a function of other exogenous variables. Again, this is much more complicated here given the dynamic nature of the model. In any case, it would be interesting to have more explanations regarding the estimation technique.

6. The random coefficient model should be better explained. At the end of the day, the model is non-linear, which is not the standard setting of the Swamy's methodology.

7. Finally, I would suggest to separate the different parts of this section (and to explain the general methodology at the beginning of the section): (i) individual estimates, (ii) panel estimates, and (iii) estimates with institutional factors. In doing so, the section would be much easier to read than the current version and further details and comments could be added. Overall, results should be more discussed and tables are not self-content.[2] It would be nice to have a table in the appendix, which reports all estimation results of Eq. (14).

3 The Phillips curve

The last section aims at evaluating the information content of the natural rate (common factor) in a Phillips curve. The analysis proceed in two steps. First, Eq. 16 is estimated

[2]Table A2b is not explained.

individually and then in a panel context. Second, the authors proceed with a system estimation.

1. The addressed question is of interest and this type of system estimation is often used in the (reduced-form) unemployment literature. However, the question is somehow "orthogonal" to the previous section (again even if there is an interest to estimate simultaneously both equations). While the key ingredient is the same (the role of the common factor), the institutional variables are no more discussed (except in footnote 16) and it is difficult to compare the results with those of the previous sections as well as to follow the streamline of the paper.

2. The measure of global inflation is a crude proxy of the underlying concept. It should be discussed.

3. Individual estimates are not discussed. However, results suggest weak evidence for the common factor.

4. Table 5 is not self-content. The mapping between the reduced-form parameters and the "structural" ones is not explicit.

5. Eq. (20) should be explained. There is no information regarding x_{it-1}. There is a typo (last equation page 17).

6. Results need to be clearly stated and interpreted. More information is needed before making an overall judgment about this section.