

Review of ‘Is inflation in South Africa a structural or monetary phenomenon?’

The paper has not improved in its revision. Its main problems persist, and they even surface more clearly now. In his comments, the author has failed to address my previous points. These comments have little to do with my points. For this reason, I have to elaborate on them in more detail.

The main point, numbered ‘3’ in my comments to the first version, has not been addressed. Nothing has been ‘corrected’. According to Table 4, the author finds 7 cointegrating relationships, in his notation $R = 7$. Then, the entire system would be stationary, which is in clear contradiction to the univariate results reported in Table 2. It is the author’s responsibility to investigate and explain this discrepancy.

If the result from multivariate analysis in Table 4 is correct, all arbitrary vectors will cointegrate. All vectors will be basis vectors, and any estimated vector is as good as $(1, 0, \dots, 0)$, $(0, 1, 0, \dots, 0)$ etc. It is not possible to retrieve something special from the single-equation FM-OLS and TSLS attempts. The author should study thoroughly the relevant source literature (for example, JOHANSEN, 1995, HAMILTON, 1994, LUTKEPOHL, 2005), references to software such as EViews do not help here.

If the TSLS and FM-OLS estimation is flawed, all conclusions drawn from such estimates are invalid.

Estimation of cointegrating vectors from single-equation methods such as FM-OLS only works reliably if there is just one cointegrating vector, in the author’s notation $R = 1$. If really $R = 7$, all estimation is futile. If $1 < R < 7$ —which is a possibility, as for example the analysis reported in Table 4 may use an inadequate deterministic specification—a triangular scheme must be set up, with a first variable regressed on the other variables, then the second variable regressed on variables #3 to #k, etc. Kindly consult the source literature. Even then, the first estimate will not permit a unique representation of cointegrating equilibrium relationships.

Other points:

1. The author has misunderstood my remark on the transformation of inflation. I would never criticize the choice of a variable, e.g. whether U.S. or Canadian inflation. My only concern is technical correctness. Consider prices denoted by P , usually assumed to be integrated and non-stationary. Also $\log P$ will be non-stationary. $\Delta \log P$ (or a variant with subannual data) is often used as ‘inflation’ and approximates $(P_t - P_{t-1})/P_{t-1}$. The author’s transformation $\log(P_t - P_{t-1})/P_{t-1}$ is unusual and fails for episodes of deflation. It is close to $\log \Delta \log P$. For low inflation, this transformation yields extremely negative values. The common and traditional transformations $\Delta \log P$ for inflation and $\Delta^2 \log P$ for changes in inflation do not share this problem. Many current authors find $I(0)$ inflation for the U.S., in the 1980s there was a preference for $I(1)$ inflation, so

this issue can be regarded as unsettled. The author's version differs from current practice, and I feel it is not an improvement.

2. With regard to the transformation $\log r$ for an interest rate r , my remark was misunderstood in a comparable way. I am not responsible for the choice of the interest rate. $\log r$, however, is an unusual specification in the literature, as it implies an extremely strong reaction for low interest rates, where $\log r$ tends to $-\infty$. Rather, $\log 1 + r$ is often used, which combines the two advantages of being on the logarithmic level and of a nearly linear reaction for small r .
3. Equations (3) and (4), although modified, are still incorrect. Equation (3) represents a model without error correction. Note that subscripts are missing from the coefficient matrix ψ . Equation (4) represents a model with error correction. The coefficient δ_1 for the error-correction term is confusing and does not correspond to the formula given beneath $\delta = 1 - \delta_1 - \dots - \delta_k$.
4. I wonder what the deterministic specification is for the DF and PP tests reported in Table 2. Even with trend (DF- τ), values such as -3.44 should be significant at least at the 10% level. This does play a role, as this would indicate that U.S. inflation is stationary.
5. FM-OLS is not the 'most advanced' technique for estimating cointegrating relations. It is, just like Johansen's ML method, an asymptotically efficient method, which means that it achieves the minimum variance in large samples. Whether FM-OLS or Johansen's method deliver better estimates in smaller samples is an unsettled issue. There is no justification for using Johansen's ML for the identification of the cointegrating rank—a decision that is used incorrectly in this paper anyway—and using FM-OLS for coefficient estimation.
6. The null hypothesis $R \leq 0$ does not make sense, as a rank cannot be negative. This should read $R = 0$.
7. Table 4 does not show a 'maximum likelihood test' but the maximum-eigenvalue statistics, one of the two versions of Johansen's likelihood-ratio test for the cointegrating rank.
8. The correct terminology for the marginal significance level is 'p-value', the words 'prob value' and 'prob.' should be avoided.
9. Table 8 is likely related to Equation 4, not to Equation 1.

References

- [1] HAMILTON, J.D. (1994) *Time Series Analysis*. Princeton University Press.
- [2] JOHANSEN, S. (1995) *Likelihood-Based Inference in Cointegrated Vector Autoregressive Models*. Oxford University Press.
- [3] LÜTKEPOHL, H. (2005) *New Introduction to Multiple Time Series Analysis*. Springer-Verlag.