

RESPONSES TO REVIEWER R. K'S COMMENTS

COMMENT: 1. 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 $(1; 0; : : : ; 0)$, $(0; 1; 0; : : : ; 0)$ 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.

RESPONSE: I agree with the reviewer that there is a discrepancy between the cointegrating results and FMOLS results reported in Tables 4 and 5 respectively. I have checked and found that the presence of nominal exchange rate variable is responsible for the seven cointegrating relationships. Consequently, I have replaced it with an equally acceptable variable: openness of the South African economy to the rest

of the world. Doing this, only four cointegrating relationships are reported and the subsequent FMOLS results reported in Table 5 are in alignment with these results. Cognizance must be taken of the fact that I have introduced $LSINFL_{t-1}$ into the equation. I do agree with the reviewer that estimation of cointegrating vectors from single-equation methods such as FMOLS only works reliably if there is just one cointegrating vector. However, my understanding is that the identified number of cointegrating relationships must reflect in the results of the equation estimated with the cointegrating variables under consideration.

OTHER POINTS

COMMENT 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.

RESPONSE: I do share the concern of the reviewer that the transformation of the inflation data may yield extremely negative values in low inflation periods. Ideally, inflation should be $\Delta \log P$ (*i.e. the growth rate of prices*). However, World Bank data used for the study report inflation as **annual percentage change in the consumer price index**. Fortunately, the transformation of the data has not produced extremely negative values as the number of observations ($N=41$) corresponds with the data points (1965–2006).

COMMENT2: 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 .

RESPONSE: This has been corrected. Nominal interest rate is now defined as $1+r$

COMMENT 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. Equation (4) represents a model with error correction. The coefficient α_1 for the error-correction term is confusing and does not correspond to the formula given beneath $\alpha =$

$$1 \quad \alpha_1 \quad \alpha_2 \quad \dots \quad \alpha_k.$$

RESPONSE: The two equations have been modified.

COMMENT 4: I wonder what the deterministic specification is for the DF and PP tests reported in Table 2. Even with trend (DF- t), 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.

RESPONSE: I do share your concern. However, that is what is obtained.

COMMENT 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.

RESPONSE: Well noted.

COMMENT 6: The null hypothesis $R \leq 0$ does not make sense, as a rank cannot be negative. This should read $R = 0$.

RESPONSE: This has been corrected. Please, see Table 4.

COMMENT 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.

RESPONSE: This has been corrected. Please, see Table 4.

COMMENT 8: The correct terminology for the marginal significance level is ‘p-value’, the words ‘prob value’ and ‘prob.’ should be avoided.

RESPONSE: This correction has been made. Please, see, for example, Table 4.

COMMENT: Table 8 is likely related to Equation 4, not to Equation 1.

RESPONSE: Correction has been made.