A Note on Global Shocks and their Impact on the Tanzanian Economy

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Haile (2016) investigates whether plummeting commodity prices, China's economic malaise, and global financial market turbulence have recently wreaked havoc on African economies. He finds that a 1 percentage point (ppts) drop in China's investment growth is associated with a decline in Tanzania's export growth of roughly 0.60 ppts and a 1 percent fall in commodity prices leads to 0.65 percent lower exports value. He also concludes that a hard landing of the Chinese economy to its new normal would doubtless send shock waves through the Tanzanian economy by further driving down commodity demand and prices as well as lowering development finance. In contrast, financial market volatility has a fairly negligible impact on economic growth. This note gives some comments to the author to improve his paper. This note could also be helpful to other academics and practitioners to improve their use of cointegration and error correction models in the applications.

Haile (2016) uses the following models in the study:

$$export_t = \theta_0 + \theta_1 cdi_t + \theta_2 price_t + \theta_3 y_t^{world} , \tag{1}$$

$$\Delta export_t = \theta_0^* + \theta_1^* \Delta cdi_t + \theta_2^* \Delta price_t + \theta_3^* \Delta y_t^{world} , \tag{2}$$

$$y_t = \delta_0 + \delta_1 cap_t + \delta_2 inv_t + \delta_3 ex_t , \tag{3}$$

$$\Delta y_t = \delta_0^* + \delta_1^* \Delta cap_t + \delta_2^* \Delta inv_t + \delta_3^* \Delta ex_t + \delta_4 vol_t , \tag{4}$$

$$\Delta p_t = \phi_0 + \phi_1 neer_t + \phi_2 \Delta p_t^{oil} + \phi_3 m_t + \phi_4 \Delta p_t^{food} . \tag{5}$$

1. In Section 3, I have the following comments to the author to improve his paper:

(a) I suggest the author to include proper error terms in the above models, e.g.

$$export_t = \theta_0 + \theta_1 cdi_i + \theta_2 price_i + \theta_3 y_i^{world} + \varepsilon_t \ . \tag{1a}$$

If he does not want to include the error terms in the above models, he should use something like the following equation:

$$E(export_t) = \theta_0 + \theta_1 cdi_i + \theta_2 price_i + \theta_3 y_i^{world} \ . \tag{1b}$$

Over here, I suggest the author to use Equation (1a) instead of Equation (1b) because the error terms could be useful for further investigation.

(b) The author should test whether all the above level variables are I(1) and all the difference variables are I(0).

(c) The author considers Equation (1) is the cointegration model while Equation (2) is the error-correction model for Equation (1). The author did include the result of the error-correction term in Table 1 but he has not included it in Equation (2. I suggest he should include the error-correction term in Equation (2)

(d) Similarly, the author considers Equation (5) is the cointegration model while Equation (6) is the error-correction model for Equation (5). The author did include the result of the error-correction term in Table 2 but he has not included it in Equation (6). I suggest he should include the error-correction term in Equation (6) In addition, the author introduces an additional variable vol_t in Equation (6) but not in Equation (5). He should show that including an additional variable vol_t only in the error-correction model but not the cointegration model is appropriate in the modeling setting.

(e) There is a problem of Equation (5) because in Equation (5) Δp_t is expected to be I(0) while some of the explanatory variables, e.g. m_t are I(1). Regressing I(0) variable on I(1) variables could be problematic, see, for example, Engle and Granger (1987) and Hamilton (1994) for more discussion.

2. In the data section, the author suggests that all variables except export are at constant market prices. I suggest the author to explain "constant market prices" clearly. I also recommend the author adjusts all data to be in terms of a particular currency, say, the US dollar.

3. In Section 5.1, the author employs the following cointegrated VAR model:

$$\Delta x_t = \alpha \beta^* x^*_{t-1} + \Gamma_1 \Delta x_{t-1} + \Phi D_{s,t} + \phi D_{p,t} + \mu_0 + \varepsilon_t \tag{6}$$

where $x^*_{t-1} = (x_{t-1}, t, t_{yy})$ is a p-dimensional vector of variables in which t_{yy} is a broken linear trend $(\cdots 0, 0, 0, 1, 2, 3, \cdots)$ starting in the year 19yy and restricted to the long-run relations, $D_{s,t}$ is an unrestricted step dummy $(0,0,0,1,1,1)$, and $D_{p,t}$ is $(\cdots, 0, 0, 0, 1, 0, 0, 0, \cdots)$ is a permanent impulse dummy and accounts for an unanticipated one-period shock effects in 19yy.

I suggest the author to use another character p for $D_{p,t}$ to avoid the confusion from p-dimensional vector of variables. Readers may refer to Haile (2016) for the definitions of the variables.

I also suggest him to test whether the breakpoints chosen in t_{yy}, $D_{s,t}$, and $D_{p,t}$ are appropriate and whether there are more than one breakpoint in each of t_{yy}, $D_{s,t}$, and $D_{p,t}$, see, for example, Bai and Perron (2003) and Bai, *et al.* (2008) for more information.

4. In p10, the author states that *in Model 1, the following dates were classified as outlying observations: 1989, 1998, and 2009. These outliers correspond to observations with standardized residuals larger than 3.0.*

The author also finds that *there exists change in trend slope in export price in 2002 and he addresses the change by using a broken linear trend in the long-run relations and a step dummy in the equations in 2002.*

I have the following comments to the author:

   (a) kindly note that the test suggests that in year 1989, 1998, and 2009, the model could have changed and thus they are not outliers.

   (b) kindly also note that the changes in the breakpoints (1989, 1998, and 2009) and slopes could be very complicated, dummy variables like a step dummy in the equations in 2002 and a broken linear trend may not be able to capture the changes.

   If the author believes that a step dummy in the equations in 2002 and a broken linear trend could capture the changes very well, I suggest the author to conduct some proper diagnostic tests, including a nonlinearity test, see, for example, Brock,

et al. (1996) and Hui, *et al.* (2017), and a nonlinear causality test, see, for example, Bai, *et al.* (2010, 2011) to test whether there is any nonlinearity or nonlinear causality that has not been captured in the model. If the tests show that there is no nonlinearity and no nonlinear causality in the residuals, then the author's claim is correct. Nonetheless, if the tests show that there exist nonlinearity or nonlinear causality in the residuals, then we could conclude that there exist nonlinearity or nonlinear causality in the model and thus there may exist nonlinear form(s) of the existing variables including the step dummy in the equations in 2002 and the broken linear trend and/or additional explanatory variables to capture the change.

5. The author suggests that *Model 3 became well-specified when he allowed for a broken linear trend in 2013(7) (i.e. the seventh month of 2013) and 2014(8), and the impulse dummies.*

   I do not see the author has conducted any proper test to draw the conclusion that "Model 3 became well-specified". I suggest the author to conduct a proper test for this purpose. There could be more than one break point. The author should conduct a proper test to determine how many breakpoints to be used and where are the best locations of the breakpoints.

6. In p11, the author suggests that there is evidence of considerable seasonality in the monthly data, which the author accounted for using seasonal dummies.

   However, I do not see any seasonal dummy in the model. The author could consider to improve the models by including some seasonality variables in all the models.

7. In p11, the author mentions that the models discussed above pass most of the specification tests and describe the data reasonably well and no serious deviations from the assumptions of residual independence and normality was detected.

   I cannot find any result from any specification tests in the paper. I suggest the author to include the results of the specification tests so that readers could know whether no serious deviations from the assumptions of residual independence and normality.

8. In p11, the author mentions that there are some signs of moderate ARCH effects and excess kurtosis. I suggest the author to include ARCH or GARCH in all his models to

capture all the ARCH/GARCH effects.

9. In Section 6, the author discusses the identified structures of long-run equilibrium relationships for Models 1 - 3. Kindly note that Models 1 and 3 are cointegration models that could be used to identify structures of long-run equilibrium relationships. However, Model 2 is an error-correction model that helps to examine the presence of equilibrium or disequilibrium between short run dynamics and long run equilibrium. Nonetheless, the author has not included the error correction term in Model 2 and other error-correction models in his paper.

10. The author draws strong conclusion that a 1 percentage point (ppts) drop in China's investment growth is associated with a decline in Tanzanias export growth of roughly 0.60 ppts, a 1 percent fall in commodity prices leads to 0.65 percent lower exports value. He concludes that a hard landing of the Chinese economy to its 'new normal' would doubtless send shock waves through the Tanzanian economy by further driving down commodity demand and prices as well as lowering development finance.

    I have the following comments to the author:

    (a) The author should report adjusted R^2 in all the tables. The results with $R^2 = 0.99$ is much different from those with $R^2 = 0.01$.

    (b) The author draws strong conclusion that the drop in China's investment growth is associated with the decline in Tanzania's export growth, etc. because, for example, the main explanatory variable used is China's domestic fixed asset investment in Equations (1) and (2). Could Tanzania's other major trading partners have the same or even higher impact on Tanzania's export growth? To address the issue, I suggest the author to include, for example, Tanzania's other trading partners' domestic fixed asset investments in Equations (1) and (2). If only China has big impact on Tanzania's economy but not any other major trading partners, then the claim from the author is correct.

11. The author comments that the small number of observations at his disposal circumscribes the power of available recursive procedures. He could consider to modify the approach used by Abeysinghe (1998) and others to improve the power of his study.

12. In this paper the author employs only cointegration and linear causality to study the impact of global shocks on the Tanzanian Economy. The author may consider to examine whether there is any nonlinear causality relationship between global shocks and the Tanzanian Economy, see, for example, Qiao, *et al.* (2009) and Chow, *et al.* (2017) and the references therein for more information.

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