

Dear Referee,

First, I would like to thank you for taking the time to read the paper and provide very useful comments. I believe the Referee's comments and suggestions will improve the paper. Please find below my point-by-point responses to the major issues raised. In particular, the Referee has raised interesting methodological issues, which necessitated detailed discussion. Therefore, I have provided below somewhat extended discussion on some of the methodological issues, which may also be of interest to readers. In case I have not understood any of your questions correctly, please let me know. Also let me know if any of the points discussed here happen to be unclear or if you have any further questions. Please note that I discuss here only the major comments while the relatively minor, but useful, comments will be taken into account in revising the paper. There seems to be some misunderstanding surrounding the theoretical model in Section 3 and the empirical model/methodology in Section 5, which I discuss below. I will add a paragraph or two in the revised version discussing the relevance of the theoretical models and how they map into the empirical model in Section 5. It is my hope this would avoid any confusion and provide a clearer picture.

Response to major comments

1. Questions on theoretical models

It should be noted upfront that the whole purpose of the broadly defined theoretical relationships in Section 3 is to facilitate understanding of the empirical analysis and extract as much information as possible from the data at the background of them. The objective of Section 3 is to discuss a number of hypothetical relationships to be empirically tested in subsequent sections. It should also be noted that I pursue a system-of-equations cointegrated vector autoregressive (CVAR) approach and thus the analysis in the paper does not correspond to single equation cointegration analysis as might be implied by the theoretical relations in Section 3.

(a) I do agree that the theoretical models in Section 3 should in principle include proper error terms in the spirit of Equation (1a) (see Note by the Referee). But, as indicated above, the theoretical relationships have been formulated with a view to facilitate understanding of and serve as a background for the empirical analysis. In a way, the models serve illustrative purposes. **It should, however, be underscored that adding error terms in Equations (1) to (5) will in no way affect the empirical analysis and thus the subsequent results.** The empirical analysis is grounded on the CVAR model in Section 5.1, which allows for proper error terms in the context of system-of-equations modeling.

(b) This question has already been addressed (see **footnote 11**). But I am reproducing it here to help readers understand the concepts of integration and cointegration in the context of system-of-equations analysis. The general requirement to use the CVAR model in Section 5.1 is that the vector process x_t is at most integrated of order 1, $I(1)$. As such the model formulation does not require that all variables in x_t be unit-root nonstationary. In other words, the aforementioned VAR model can be used for the empirical analysis if at least some of the variables in x_t are unit-root nonstationary. As a first step in determining the order of integration of x_t we applied univariate augmented Dickey-Fuller test for all variables in the system. The relevant alternative hypothesis in most cases is trend-stationarity because most of the variables are trending. In addition, we performed univariate unit-root test also allowing for a change in trend slope at some known dates identified based on the methodology in Doornik *et al.* (2013). The null of unit-root was not rejected

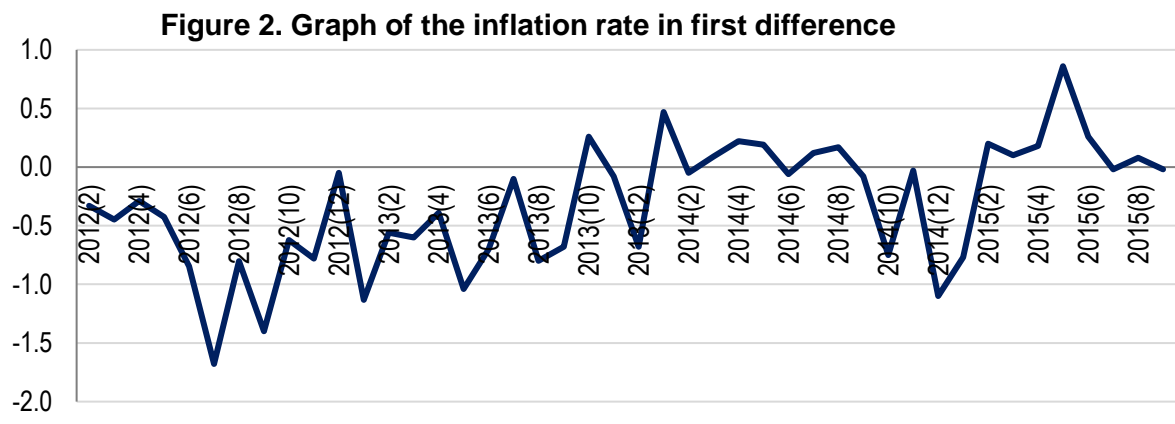
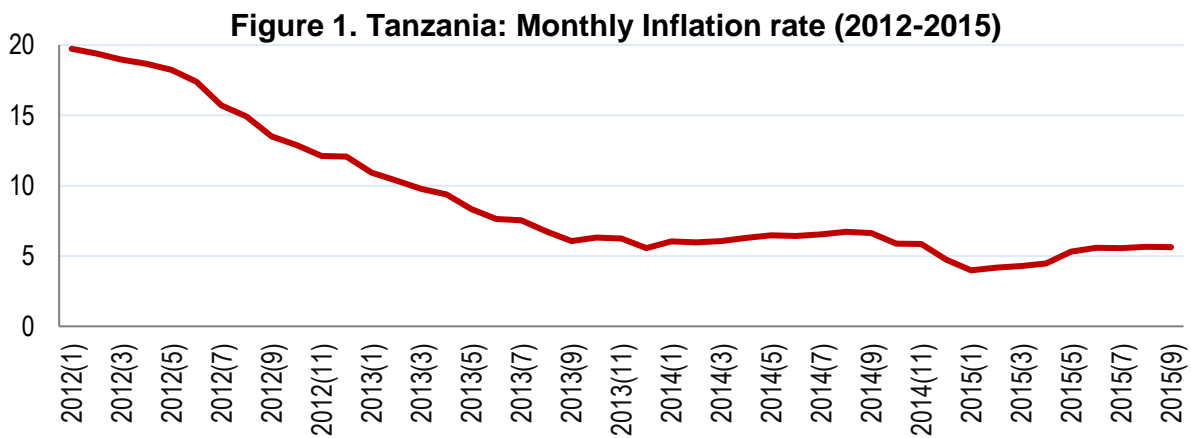
in all cases, although it was rejected for some of the series when the test was based on a regression that included a trend term. Next, the order of integration and cointegration was determined based on multivariate VAR based unit-root test. Neither the choice of full rank (data in levels are stationary) nor zero rank (data are non-stationary but not cointegrated) was supported by the statistical tests in all cases, suggesting that $x_t \sim I(1)$. We also tested the order of integration of the individual series within the multivariate model. Because most of the variables are subject to deterministic trends we test the hypotheses that the variables as well as their trend-adjusted counterparts are $I(1)$. As all three models were specified to allow for a change in trend slope we formulate the test to allow for this possibility. All variables were found to be unit-root nonstationary; however, the $I(1)$ test was rejected for some of them when we allowed for a change in trend slope.

(c) First, I would like to clarify that I do not consider Equations (1) as cointegration model and Equation (2) as the error-correction model. As indicated above, Equations (1) and (2) simply serve illustrative purposes concerning the impacts of the ‘explanatory’ variables on export level and export growth, respectively. The cointegration and error-correction coefficients are clearly defined in the equation for CVAR model in Section 5.1. As the Referee pointed out, the error correction coefficients are reported in Table 1 only for the long-run model. In the CVAR model, the error-correction terms enter the short-run model as an exogenous variables (see Juselius (2006: Chapter 13)). In other words, the reduced-form (short-run) VAR model is conventionally estimated conditional on the identified long-run relationships. The revised version of the paper will report the coefficients on the error-correction terms in the short-run model.

(d) The first part of this question is addressed in my answer to question 1(c). Because the additional variable vol_t (capturing the volatility of capital flows) is included in the CVAR model, the variable appears in the long-run model in *level* while it appears in the short-run model in *first difference*. Table 2 reports only the estimate corresponding to the short-run model (i.e. the equation for GDP growth Δy_t) because it is of particular interest to see the impact of volatility in capital flows on economic growth. In other words, the variable is included both in the long- and short-run models but, in Table 2, I am just zooming in on the short-run growth model. So the approach is econometrically plausible like those for the rest of the analysis. It is just that I am discussing only some of the results, namely the short-run growth estimates. If need be, I can report the long-run estimates, although the impact of volatility in capital flows on GDP (in levels) is of no particular interest. One of the key questions the paper set out to address is whether volatility in global financial markets would significantly impact on Tanzania's economic growth— the key reason we focused on the equation for Δy_t .

(e) As regards the question surrounding integration and cointegration, I will argue in favor of two things: (i) Statistical tests clearly show that Δp_t is $I(1)$, not $I(0)$; (ii) Even if Δp_t is $I(0)$ this would not *necessarily* be a problem. First, Δp_t in Equation (5) is $I(1)$. Figures 1 and 2 show the inflation rate and its first difference, respectively. The figure indicates that Δp_t exhibits clear non-stationary movements (with a break in trend slopes toward the end of 2013). Statistical tests confirm that the inflation rate is indeed unit-root nonstationary. The null hypothesis that Δp_t is $I(0)$ was clearly rejected with a very high p -value. So, Δp_t is definitely not $I(0)$. This does not come as much of a surprise in light of many prominent studies which show that prices are normally $I(2)$ while their first differences (inflation rates) are $I(1)$ (see, among many others, Juselius (2006: Chapter 16), Johansen *et al.* (2010), Juselius and Toro (1999), Juselius and Ordonez (2009), Juselius and McDonald (2004)). Second, even if Δp_t was $I(1)$ it would not necessarily follow that the analysis is econometrically implausible. In a model with only two variables, both of them need to be at least

$I(1)$ for cointegration analysis to be plausible. However, in a model with several variables, not all variables have to be $I(1)$. As noted above, the general requirement to use the CVAR model in Section 5.1 is that the variables should be *at most* $I(1)$ (Johansen, 1996; Juselius, 2006). In other words, the analysis would be sound even if some of them are $I(0)$. Even if the left-hand-side (LHS), in our case Δp_t , was $I(0)$ (which is not as mentioned above), the nonstationarity in the right-hand-side (RHS) variables could cancel out each other, leaving both sides of the equation stationary. By contrast, in a world with only two variables, if the LHS variable is $I(0)$ and the RHS variable $I(1)$ the analysis would not be econometrically acceptable as an $I(0)$ variable cannot be equivalent to an $I(1)$. But this is not necessarily the case when several variables are at play.



2. (i) Noted with thanks. I will add a clarification in the revised version of the paper. By constant market prices, I mean all variables except exports are adjusted for price (or inflation) effects. All data are given in US Dollars (see data uploaded on the *Economics* Dataverse).
- (ii) Noted with thanks. I will use another character instead of p for $D_{p,t}$. As for the second question about testing whether the “chosen” breakpoints are appropriate and whether there exist more than one breakpoints, the third paragraph on page 10 and footnote 11 mention how the breakpoints were identified. However, these may not be sufficient and I will make sure that the revised version of the paper sheds more light on the determination of the existence, timing, and significance of the breaks in trend slopes (or shifts in mean growth rates).

3. Determination of break points

(i) **Methodological choice:** In modelling structural breaks, the paper draws on the conventional (multivariate) cointegration approach in Johansen *et al.* (2000) and Hungnes (2010), which accommodates different types of structural breaks. Specifically, using such a multivariate framework, hypothesis testing on breaks in trend slopes (or shifts in growth rates) can be formulated and properly tested. A potential drawback of a system-of-equations approach is that the trend breaks are assumed to occur at the same date for all series. An alternative would be to use a univariate approach and apply some variant of the method proposed by Perron (1989). However, in our case, the use of a single equation model would be more restrictive and hard to justify in the face of overwhelming evidence for the existence of more than one cointegration relations in all three models. In addition, Bai *et al.* (1998) show that there are substantial gains in precision from using multivariate models in which several variables are modelled as cointegrated system. The use of multiple series sharpens inference about the existence and dates of shifts in the mean levels (Bai *et al.*, 1998, pp. 420). In other words, a break in mean growth rates might be more readily detected and estimated in a multivariate setting including variables that are purportedly co-moving.

(ii) **Empirical application:** As a starting point, we identified the break dates based on *a priori* knowledge on the timing of special events in the data, a graphical inspection of the data, as well as a statistical test for the presence of trend and level shifts in the data. The broken trend possesses the most significant coefficient at the break dates mentioned in Section 5.2 and accordingly the models are specified with a change in trend slope at these dates. The hypotheses that Tanzania has had no statistically significant shift in the mean growth rates of the series at the specified dates are strongly rejected (p -value: 0.00). Turning to formal tools, I identified and test for trend breaks using univariate as well as multivariate statistical procedures. In particular, an algorithm searching for breaks developed by Doornik *et al.* (2013) and the procedure in Hungnes (2005) were used to determine the existence, timing, as well as the significance of breaks in mean macroeconomic growth rates. Sustained shifts in growth rates are defined following Hausmann *et al.* (2005) (i) For a shift in mean growth rate to be categorized as a growth turnaround it should be sustained for at least 8 years and the change in growth rate has to be at least 2 percentage points; (ii) A variables can experience more than one instance of growth turnaround as long as the dates are more than 5 years apart; (iii) Trend breaks were selected at 1% 'target size' (i.e. $\alpha = 0.01$) in the *Autometrics* options in OxMetrics 7 (see Doornik *et al.* (2013)).¹ Note that we perform a sensitivity analysis to examine if the estimates based on the statistically as well as economically most credible break date are fairly robust to alternative candidate break points in the vicinity of the first-best break point. We find that the main conclusions of this paper are sufficiently robust to changes in the break dates.

4. Dummy specification

(a) I completely agree that the spotted observations do not necessarily correspond to outliers. However, as discussed below, these observations were closely scrutinized and accounted for using different types of dummy variables as well as through allowing for changes in the deterministic components of the VAR model. The VAR model is derived under the assumption of multivariate normality and constant parameters. In a first tentatively estimated VAR model, these assumptions are often rejected due to, *inter alia*, omission of important variables, interventions, inadequate

¹The target size determines the significance level below which a break is not kept in the model. Note that for Burkina Faso the trend break was significant only at the 5% level.

measurements, and policy reforms. Macroeconomic data are generally riddled with extraordinary events and changes in institutional settings, which are likely to distort statistical inference unless they are appropriately accounted for. Specifically, the dates mentioned on page 10 (1998 and 2009)² have been found to be aberrant (or extraordinary) observations, which may cause structural breaks or extraordinary effects on the variables by changing the data generating mechanisms. Therefore, I test whether these events have permanently changed the equilibrium relationship between the variables in the models and econometrically account for the most significant ones. Accordingly, the analysis controls for the most dramatic events using different types of dummy variables. For example, a shift in the equilibrium mean can be captured by a step dummy, $D_s yy_t$, defined as $(0, \dots, 0, 0, 1, 1, 1, \dots, 1)$, while a one-period shock effect can be accounted for by an impulse dummy, $D_p yy_t$, defined as $(0, \dots, 0, 0, 0, 1, 0, 0, 0, \dots, 0)$. In addition, in models with changes in trend slopes in the long-run relations, there is a need to additionally account for the change in underlying trends (and thus the corresponding shift in long-run growth rates). Such events were modelled using a broken linear trends (t_{yy}) in the long-run relations, Δx_t , and a step dummy ($D_s yy$) in the equations, x_t . These is perfectly in line with the tradition in the CVAR literature. See Nielsen (2008), Juselius *et al.* (2014a,b), Johansen *et al.* (2010), Juselius and McDonald (2004), Gebregziabher (2014, 2015).

(b) In fact, the specification of dummies and deterministic trends (as well as changes in trends slopes) are based on proper diagnostic tests. I understand that changes in trend slopes and specification of dummy variables could be complicated and are not always amenable to straightforward interpretation. *In principle*, the dummies, linear trends, and broken linear trends may not able to capture the changes. That is why we need model misspecification tests (see, for instance, Juselius (Chapter 4)). If the deterministic trends and dummies failed to capture the extraordinary changes in the data, then the model would not pass the misspecification tests. The question of whether the models describe the data reasonably well is inherently an empirical issue and needs to be settled based on through diagnostic tests. In other words, **the question whether the linear VAR model is misspecified or not is testable.** If the linear VAR model failed the misspecification tests, this would potentially call for nonlinear models, among others. Accordingly, I followed the standard approach to linear CVAR modelling (Johansen, 1996; and Juselius, 2006) and performed several misspecification tests. As mentioned in the paper, all linear CVAR models pass the misspecification tests with high p -values, suggesting the models are well-specified and capture the variation in the data fairly well. As pointed out by Referee 1, the details of the specification test results were not reported in the paper. But as I promised in my response to the first Referee, the misspecification test results will be reported in the revised version. Therefore, I did not arrive at the conclusion that the linear CVAR model captures well the key features of the data without conducting proper diagnostic tests. The question of how to model changes in the underlying trends of macrovariables (deterministically versus stochastically) and how to specify (as and interpret) the deterministic components in the VAR model is not straightforward and has no easy answer. However, as suggested in Juselius (2006: pp. 298), because the extraordinary events in our models systematically changed (mean) growth rates I modelled the shifts deterministically.

5. Two things are worth mentioning here. First, as mentioned above, I have conducted several model misspecification tests (the detailed test results will be reported in the revised version as I

²As pointed out by Referee 1, the year 1989 was included among the outlying observations by mistake and thus I have removed it from the discussion here.

already mentioned in my response to the first Referee). So the conclusions are well grounded on a models that passed standard misspecification results. Second, I have already explained above how the breakpoints were identified.

6. I do not understand the statement “I do not see any seasonal dummy in the model”. Of course, seasonal dummies were incorporated in the model that used monthly data, namely Model 3. Seasonal dummies were not included in Models 1 and 2 as they entered the models insignificantly. However, estimates of seasonal dummies are not often reported in empirical applications of the CVAR model as they are not of particular interest and not of utmost significance. Otherwise, I can report these estimates in the revised version of the paper.

7. Noted with thanks. This point has also been raised by the first Referee. Accordingly, I am currently revising the paper with a view to incorporating results from the several misspecification tests conducted.

8. In my opinion, this is not the way to go about the moderate ARCH effects (see Juselius (2006: Chapter 4)). In the first place, only residuals from the equations for some of the variables exhibited borderline significant ARCH effects. However, as already mentioned in the paper, Rahbek *et al.* (2002) and Gonzalo (1994) have shown that cointegrated VAR results are robust against moderate residual ARCH effects. This is a well-established result in the literature. So there is no need to resort to ARCH/GARCH models. The ARCH effects are not even significant enough to be modeled by ARCH/GARCH models.

9. My response to question 1(c) above also answers this question.

10. (a) Please note that the models have been estimated using systems-of-equations approach. So the appropriate yardstick to look after are p -values, which are already reported in the paper. For instance, the structure of identified long-run relations in Model 1 has been accepted with a p -value of 0.74 (see Table 1). Similarly, Model is accepted with a p -value of 0.97 (see Table 2). The rest of the Tables in the paper include p -values with which the corresponding models have been accepted. Note that a high p -value means that the identified long-run relations are almost exactly in the space spanned by the unrestricted long-run structure. The p -values measure the degree to which the identifying restrictions are data consistent: the higher the p -value, the more strongly the restrictions are accepted.

(b) I have run several sensitivity tests to check the robustness the main conclusion of the paper that a drop in China’s investment growth is associated with a decline in Tanzania’s export growth. This includes using China’s GDP as an indicator for economic activity in China instead of China’s domestic fixed asset investment. The key results were found to be sufficiently robust. *See Appendix Tables 1-3.* One of the main questions the paper set out to address is whether China’s economic slowdown would potentially have significant negative effects on the Tanzanian economy and particularly on the country’s export sector. As I explained in my response to the first Referee, I would say the paper’s key results are not implausible in light of the findings of other studies. Our results are consistent with, for instance, the finding in Drummond and Liu (2013), who show that a 1 percentage increase in China’s domestic investment growth is correlated with an average 0.60 percentage point (ppts) increase in Sub-Saharan Africa (SSA) countries’ export growth. In

particular, focusing on the top five resource-rich SSA countries (Angola, South Africa, the Republic of Congo, Equatorial Guinea, and the Democratic Republic of Congo), they show that a 1 percentage point slower investment growth in China is associated with 0.80 (ppts) lower export growth: the more resource rich a country is, the larger the impact on export growth. Note that the majority of SSA countries in Drummond and Liu (2013) exported only less than 15 percent of their exports during 2001-2011 to China. It should also be kept in mind that, for many of these countries, the shares of exports to China were much smaller in the 1990s than in the 2000s. Given that the above-mentioned study also looks at the impact of China's domestic investment on the exports of SSA countries and focuses on more or less similar period, I would think our results are on the plausible side. Having said that, looking at the impacts of fluctuations in the economies' of Tanzania's other trading partners on Tanzania's exports would be an important topic for further research.

11. To my knowledge, the standard CVAR methodology applied in the current study does not allow us to **reliably** use the available recursive procedures to test parameter stability. See Juselius *et al.* (2014a,b), Gebregziabher (2014, 2015), among many others, who make similar assertions. This is why I noted upfront the limitations of the conventional VAR model. However, since both parameter non-constancy and non-normal errors are often associated with periods of political and economic turbulence, such as supply shocks, war, severe droughts, civil unrest, and policy interventions, we improve parameter stability and mitigate non-normality by controlling for the most dramatic events using several dummy variables. The aforementioned papers, for instance, attempt to circumvent the problem using a similar approach.

12. My response to this question would be similar to 4(b).

References

1. Bai J., Lumsdaine R. L., and Stock J. H. (1998). Testing for and dating common breaks in multivariate time series. *Review of Economic Studies*, 65, 395-432.
2. Doornik, J. A., Hendry, D. F., and Pretis, F. (2013). Step-indicator saturation. Discussion paper 658, Department of Economics, University of Oxford.
3. Gebregziabher, F. H. (2014). The long-run macroeconomic effects of aid and disaggregated aid in Ethiopia. *Journal of International Development*, 26(4): 520-540.
4. Gebregziabher, F. H. (2015). Adjustment and Long-Run Economic Performance in 18 African Countries. *Journal of International Development*, 27(2): 170-196.
5. Hausmann, R., Pritchett, L., and Rodrik, D. (2005). Growth accelerations. *Journal of Economic Growth*, 10(4): 303-329.
6. Hungnes, H. (2005). Identifying the deterministic components in GRaM for Ox Professional: User manual and documentation, mimeo. Available at <http://www.hungnes.net/GRaM>.
7. Hungnes, H. (2010). Identifying structural breaks in cointegrated vector autoregressive models. *Oxford Bulletin of Economics and Statistics*, 72 (4): 551-565.
8. Johansen, S., Mosconi, R., and Nielsen, B. (2000). Cointegration analysis in the presence of structural breaks in the deterministic trend. *The Econometrics Journal*, 3(2): 216-249.
9. Johansen, S, K. Juselius, R. Frydman and M. D. Goldberg (2010). Testing Hypotheses in an I(2) Model with Piecewise Linear Trends. An Analysis of the Persistent Long Swings in the Dmk/\$Rate. *Journal of Econometrics* 158, 117-129.

10. Juselius, K. (2006). *The cointegrated VAR model: Methodology and applications*. Oxford: Oxford University Press. Juselius, K. and MacDonald, R. (2004). International parity relationships between the USA and Japan. *Japan and the World Economy*, 16: 17-34.
11. Juselius, K., Møller N., and Tarp, F. (2014a). The long-run impact of foreign aid in 36 African countries: Insights from multivariate time series analysis. *Oxford Bulletin of Economics and Statistics*, 76(2): 153-184.
12. Juselius, K., Reshid, A., and Tarp, F. (2014b). The real exchange rate, foreign aid and macroeconomic transmission mechanisms in Tanzania and Ghana. Discussion Papers, University of Copenhagen.
13. Juselius, K., Reshid, A., and Tarp, F. (2013). The real exchange rate, foreign aid and macroeconomic transmission mechanisms in Tanzania and Ghana. Working paper no. 90, UNU-WIDER.
14. Juselius, K. and J. Toro (1999). The effect of joining the EMS: Monetary Transmission mechanisms in Spain", Working Paper, 22, Institute of Economics, University of Copenhagen.
15. Juselius, K. and J. Ordonez (2009). Balassa-Samuelson and Wage, Price and Unemployment Dynamics in the Spanish Transition to EMU Membership. *The Open-Access, Open-Assessment E-Journal*, vol. 3.
16. Perron, P. (1989). The great crash, the oil price shock, and the unit root hypothesis. *Econometrica*, 57, 1361-1401. Erratum (1993) *Econometrica*, 61: 248-249.