

The article “Global Shocks and their Impact on the Tanzanian Economy” (submission MS 2030) applies the cointegrated VAR (CVAR) methodology in order to investigate the exposure of the Tanzanian economy to (i) spill over effects from the Chinese economy and commodity price fluctuations, (ii) volatility in capital flows, and (iii) exchange rate fluctuations. With respect to (i) the Chinese economy and commodity prices, the study identifies substantial spill over effects from the Chinese economy, as well as a strong dependency on commodity prices. With respect to (ii) volatility in capital flows, the study only finds a small impact on economic growth in Tanzania. (iii) Exchange rate fluctuations are found to have a substantial impact on price levels, with a pass-through of about 60%.

Methodologically, the study is a relatively conventional implementation of the CVAR approach as described, for instance, by Juselius (2006). In order to address issues (i)-(iii) listed above, the author applies this methodology in three distinct exercises: (i) is investigated in a system of equations that includes yearly data on Tanzanian exports, Chinese domestic investment, commodity prices, and world GDP between 1990 and 2014; (ii) in a system that includes Tanzanian GDP, capital inflows, domestic investment and exports in Tanzania, with annual data spanning from 1980 to 2014; and (iii) in a system that includes domestic CPI, the nominal effective exchange rate, M2, and the world prices of oil and food, with *monthly* data covering January 2013 to January 2015.

The article is generally clear, concisely written, and well-structured. The author sticks closely to the statistical procedure described by Juselius (2006). However, I have a serious doubts about the reliability of the result. As elaborated below, these are based on concerns about *methodological choices and the transparency* of the latter, concerns about the *adequacy of the data*, the *plausibility of the results*, as well as *conceptual issues*. In what follows, I will first briefly clarify these general concerns, and then comment on more minor and specific issues in the article.

#### *Methodological choices / transparency:*

- Throughout the article, crucial test statistics are not reported; instead, they “can be provided upon request”. Especially in the case of choices of cointegration rank, one is unlikely not to run into any issues of non-normal residuals, undermining the validity of the trace test. Indeed, the author reports that normal errors were only “borderline accepted” (p. 11), without specifying any level of significance. Nor is it specified in which of the three models this is the case. *I suggest a simple table providing this information for all models combined. This which could also include values for excess kurtosis and skewness, which may be relevant in order to assess the impact on the trace test.*
- According to the article, Model 1 is estimated using data starting from 1990. However, it also says the model includes a dummy variable for 1989. This needs clarification at least. More generally, it is unclear from the text what dummies are included: The years are indicated and (in most cases) related to historical events or episodes, but not the type of dummy this is accounted for with.
- Using the dataset provided by the author on the journal’s website, I failed to replicate the results of model 1 (and did not proceed to the remaining two models). *I suggest a systematic listing of the three models’ features. From the text, this is clear for the lag length and cointegration rank (as they are the same throughout models), but not for the deterministic components.*
- Section 6 reports impulse response functions, the computation of which requires the models to be identified, but the assumptions underlying this (at least for the short-term analysis) do not seem to be explicitly stated.

#### *Adequacy of the data:*

- The sample size varies between 25 and 35 observations, which is extremely short given the large number of parameters to be estimated; this often leaves about 10 degrees of freedom, where many authors deem 50 or 100 to be the sensible minimum requirement. While I would not *categorically* dismiss the evidence on these grounds, I believe the author should be much more upfront about the resulting caveats. Generally, the results should be treated as *weak* indications of the phenomena they describe; these can then complement a strong narrative or theoretical argument. Instead, the author puts all the weight on the data, of which there simply is too little.

*Plausibility of the results:*

- A core result of the study is that “a one percentage contraction in domestic investment in China would lead to a drop in Tanzania’s exports of about 0.57 percent.” (p. 12; Note: also cited in abstract and introduction but with *not* trivially different wording. *Percentage points or percentages, levels or growth rates?* My understanding is that p. 12 captures the meaning of the results best.) This number seems implausibly high: China, while growing in importance, is only the third biggest export destination and accounts for roughly 10% of Tanzanian exports (see Observatory of Economic Complexity, 2016). At the beginning of the sample period, according to the study, this share was negligible. In my opinion, it is much more likely that Chinese investment captures features of the world economy that are not captured by world GDP (also included, but offering almost no variation; as a side note, it may be desirable to weight this by trade shares). As an experiment, what would happen if one was to include India’s investment / GDP (India being the first export destination)? Besides that, the previously mentioned small sample issues may be at play here.

*Conceptual issues:*

- Related to the previous point, the study puts a lot of emphasis on the importance of the Chinese economy for Tanzanian exports. Its claims in this respect are based on an investigation of the period between 1990 and 2014, but the method it employs crucially assumes parameter constancy across time. It is unlikely, however, that the effect of a change in Chinese investment in 1990 – when trade between the two countries was “negligible” for another decade (p. 3) – on the Tanzanian economy was the same as it would be in 2014, after trade took up. If the reported effect is meant to represent some average over the period, see the previous point for the plausibility of the estimated amplitude of the effect.

Besides these more general points, I note the following issues in the article:

- Starting with the abstract, the tone of the article seems overly dramatic. Example: “China’s economic *malaise* [...] *wreaked havoc* on African economies.” (p. 1) I do not believe this describes the situation adequately. Further instances of this are highlighted in the annotated scan of the article.
- p. 2: Saying that the study “generally [uses] data spanning the period 1980-2015” is misleading; in fact, none of the exercises uses this sample period. According to the main body, only the one investigating the effect of volatility in capital flows *almost* does so, using data from 1980 to 2014.
- Some claims are made without sufficient underpinning. For instance, the recent evolution of the Chinese economy from an investment-driven to a consumption-driven economy and the effects of this on commodity prices are simply stated as if they were long-established facts (p.2,3). A reference to a study substantiating such claims would be useful. Other instances

include (paraphrasing): “Africa is a prime destination for Chinese FDI because of limited competition, cheap and plentiful labour” (p. 3); “China’s slower growth is more balanced” (p. 5).

- p. 6: It is unclear to me what exactly the measure of volatility is (conditional standard deviation – but conditional on what?). Further, it is not obvious why volatility does not enter equation 1, but equation 2 in levels as opposed to all other variables.
- p. 8: The data sources are not precisely stated: It would be desirable to know the versions of the databases, as well as which variables are taken from which of the mentioned databases.
- p. 9: The notation in the explanation of the dummy-types is inaccurate (although conform with older articles, where it made sense): “19yy” does not work when parts of the sample are in the 21<sup>st</sup> century (“20yy”).
- p. 9 also includes a long footnote on stationarity and cointegration which does not appear necessary to me. Instead, it would be more informative to see the corresponding test results.
- P. 10: The choice of lag length is motivated by some findings of no autocorrelation etc., but again the corresponding tests are not reported, nor are the usual information criteria. Given the short sample, a lag length any longer seems unrealistic in the first place. The author could be upfront about that.
- p. 12:  $\alpha$  and  $\beta$  may give clues about sequentiality (or Granger-causality), but not necessarily about *causality* without further qualifications.
- P. 13, footnote 18: it should not be “surprising” that an estimate is consistent with a previous study.
- p. 14: “Impulse response analysis” is described as describing the effects of a 1 SE shock to a variable; 1 SE is just a frequent choice of impulse, but in the context of this study a 1% shock seems to be more sensible to discuss, as this is also what the author refers to throughout the article when discussing marginal effects. Note that 1SE shocks are often simply a convenient choice when computing IRFs, as many software packages have this as a default setting. It may be worth putting in the extra effort to obtain comparable measures.
- p. 17: When summarising the effect of a change in NEER on the *inflation rate*, I think one should really be talking about *prices* (last paragraph). Relatedly (and this applies to other instances in the article), percentage points (ppts) are given as a unit where percentage would be correct.

## References

Juselius, K. (2006). The Cointegrated VAR Model: Methodology and Applications. Oxford University Press.

Observatory of Economic Complexity, (2016). Available online at <http://atlas.media.mit.edu/en/profile/country/tza/#Exports>,