

**Interactive comment on “Forecasting droughts in East Africa” by E. Mwangi et al  
Anonymous Referee #2**

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The paper explores the use of ECMWF in forecasting droughts in Africa; the topic is quite interesting and very relevant for the journal. The abstract is presented in a simple way but lacks of one or two line details on how the error was assessed. This would appear that have spatial and temporal components from the abstract but they are not clearly developed or explained. The methodology used for the scientific procedure was not explained in overall and due to the high number of abbreviations it becomes a bit complex. Vital statistical information from the SPI (used) are not presented. The results and conclusion are mixed with what was done in this study and with conclusions from other works, so conclusion might not clearly come from the presented results. The skill scores used are not explained well. In my opinion still needs some improvement in the descriptions and to not be so sharp in the conclusion that might mislead the extension of the results.

Important to highlight that the abstract seems to point to develop analysis in terms of drought duration, magnitude and spatial extent, this somehow is not clear along the paper

I have two overall comments and a list of small concerns,

Based on these I believe a number of changes and a second review are required before the paper is published.

**We thank the reviewer for the comments on the paper. See our comments in red below.**

This are some comments related to the paper readability and structure.

1. The abstract present unclear link between drought with dynamic models and drought index. It is important to clarify that SPI is meant for offline analysis and normally average along period. In the same way it might not be suitable for a dynamic system as it is stated. Please check the variability of your SPI if you change the starting point, or the range of time of the SPI time series analysis.

**In the paper, we did not provide an extensive explanation of the SPI calculations using the dynamical forecasts. The detailed methodology used to calculate the SPI from the ECMWF seasonal forecasts and a general evaluation of the forecast skill is currently being prepared and will be submitted to HESS (Dutra et al. 2013b). These followed the methodology initially described by Dutra et al. (2013a) and applied on a basin scale. Similar approaches have been also described by Mo et al. (2012) and Yuan and Wood (2013). Considering the above mentioned works, we are confident of the suitability of using seasonal dynamical forecasts of precipitation for the SPI calculations when these seasonal forecasts provide long hindcasts, as is the case of the ECMWF system.**

**Ref:**

Dutra, E., Di Giuseppe, F., Wetterhall, F., and Pappenberger, F.: Seasonal forecasts of droughts in African basins using the Standardized Precipitation Index, *Hydrol. Earth Syst. Sci.*, 17, 2359-2373, doi: 10.5194/hess-17-2359-2013, 2013a.

Dutra, E., F. Wetterhall, F. Di Giuseppe, G. Naumann, P. Barbosa, J. Vogt, W. Pozzi, and F. Pappenberger,: Global meteorological drought: Part II - Seasonal forecasts, to be submitted to HESS, 2013b.

Mo, K. C., Shukla, S., Lettenmaier, D. P., and Chen, L.-C.: Do Climate Forecast System (CFSv2) forecasts improve seasonal soil moisture prediction?, *Geophys. Res. Lett.*, 39, L23703, doi: 10.1029/2012gl053598, 2012

Yuan, X., and Wood, E. F.: Multimodel seasonal forecasting of global drought onset, *Geophys. Res. Lett.*, 2013GL057540, doi: 10.1002/grl.50949, 2013

Line 10 page 10213, it is not clear why observed interpolated precipitation is given as percentage of long term information. What is the benefit, what is the area interpolated, which methods have been used. At this stage this statement makes vague de description of the observed data. May be a figure and a short description of the region taken for this interpolation and the period taken for the averaging. At this part of the paper it is not clear what is going to be the methodology along the paper. Just models and data are being presented.

The observed maps for the Greater Horn of Africa were obtained from ICPAC as images because we could not access the raw data for the whole region. For a particular season, seasonal totals from representative stations from all the countries in the region are taken and divided by the seasonal long term mean (1961-1990) and multiplied by a hundred to see the percentage of the long term mean that was received in that season. These values are at points so to produce the maps interpolation.

Line 15, page 15213, missing references to most of these products

#### References added

Line 20, it appears that the hindcast is initialised using ERA Interim reanalysis for the period 1981–2010 and using 15 ensemble members, but somehow I missed the link with the synoptic, this in terms of time and the spatial relating (grid size and locations). It will be good to have a map at this stage of the paper and a table will also help.

We do not understand the reviewer's question on the need for a figure and table. A detailed description of the seasonal forecasts system, including the initial conditions (taken from ERA-Interim) is given by Molteni et al (2011). This is similar to initializing a medium-range weather forecast from analyzed 3D- atmospheric fields (in this case from ERA-Interim).

lines 5-20 page 15214, The information provided is written as reference of what other people use to do for assessing the forecasting skills, but I believe it is more clear if it is mentioned directly what was used from all this skill score measures. A table with names, formulas and reference to the skill scores

analysis used should be provided. Main measures of assessment should be explained in a very short way, including the formula used. ACC is used widely but not explained how it was implemented (In short). The continuous ranked probability score formula is also not presented but results of its calculation are crucial for the understanding of the graphical results presented.

The formulas will be added in the revised version. We could potentially add a table with this information, but we think that it is better explained in the text with the mathematical formulas carefully explained.

In the section 2.3, qualitative assessment does not clearly state what is talking about. It seems that quantitative information is not available, however, previous sections talks about previous quantitative information. So how is this smoothing and manually tuning information linked with the previous section? Please try to update or explain better the link, may be also related to a methodology or structural description of the research or experimental procedure would help a lot.

In this section we only provide a qualitative assessment, as opposite to section 2.2 as we do not have the “grid-point” information of the outlook forums forecasts and observations used, only the maps. Therefore we cannot quantify the skill of the outlook forum and compare with ECMWF forecasts, but only provide a qualitative evaluation, based visual inspection of the forecasts and observations maps. This has been further explained in the text.

From line 7 to 15, page ... please add this into a table; it will improve the readability of the paper.

This information will be put in a table.

Page 10215 It is important to highlight that the SPI is sensitive to the quantity and reliability of the data used to fit the distribution. The distribution used to fit the data is not mentioned. Also please take into account that studies recommend using at least 30 years of high-quality data (McKee et al. (1993), may be nice to comment on this. Applicability of the SPI depends on a suitable theoretical probability distribution being found to model the raw precipitation data prior to standardization. Line 26 again talks about an example, but it is not clear what is used in this study.

Please see the reply to the first question.

### Section 3 Results and Discussion

It is not well explained

Page 10216 line 1, It is not mentioned from where the Anomaly correlation coefficients. Also Figures mentioned in the paragraph talk about statistical significance but the caption should clearly state of what?

The first sentence of results was deleted. The statistical significance in this context means that the correlation is significantly different from zero with 95% confidence.

Page 10216 line 1-7, it is not well explained, from what is shown in graphs is more a isolated case and not a clear overall mathematical analysis of what we can see in the graphs. I suggest a time series of the changes in correlation on the overall region or by a combination of sub-regions. As it is now, I don't feel that the forecasting abilities represent what is said to represent.

Figure 4 show the time series (as a function of lead time) of the CRPSS for the different lead times, as the reviewer mentioned, and similar results are found for the anomaly correlation.

Page 10216 line 7-12, it is not clear the discussion and as it is not somehow mixes a bit the conclusion with the work presented by other authors. The statement clear says that the reason of the phenomena is linked to the SST and El NINO, but this is not a result of this work and I believe is part of a discussion. So the English might be stating that this relation was conclusive based on this paper, which I clearly do not see it anywhere.

The sentence was clarified.

ROC presented figure 5 was never explained before, and clearly these variability of ROC ensembles needs to have some information, it is necessary to present what you mean in this box plots.

A brief description of the ROC scores was added to the paper.