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Discussions

Interactive comment on "Predicting East African spring droughts using Pacific and Indian Ocean sea surface temperature indices" *by* C. Funk et al.

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Summary:

This Discussion Paper builds on a series of important papers that the authors have written on patterns, drivers, and change in East African precipitation. In this paper the authors use this foundation to show that variability in March-May East African precipitation can largely be explained by variability in two key SST indices. They then propose that these indices can be used as tools for seasonal prediction. The paper is well-motivated and clearly written, and as far as I can judge it is methodologically sound. I do have one general comment about the purpose of the paper and a number of specific comments on methodology and presentation that I suggest the authors consider

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before the paper is accepted for final publication. These are listed below.

General Comment:

The core motivation for the paper is to identify indices that can be used in seasonal prediction. But the authors stop short of proposing a complete, vetted prediction system: there are no out-of-sample tests of predictive skill, predictors are not combined to generate a prediction of total precipitation anomalies, and despite the fact that the authors note nonlinearities and nonstationarities in their correlations they do not propose any way to incorporate this information into operational predictions.

Given these limitations, it seems that this paper is intended as an intermediate step rather than an end in itself. I have no objection to this, but I do think that the paper would stand on its own more effectively if the authors could frame its contributions in more specific terms. As it stands, Section 1 is mostly general background on the region and Section 4 is mostly general conclusions on statistical approaches to prediction. I would be interested to see these sections address questions like: What is the advantage of predicting PCs of precipitation rather than total precipitation? Is there any limitation to using PCs that explain only 38% of total variance as the prediction target? What do the proposed predictor indices offer that other predictors do not? What do these results suggest about the baseline period or the model structure of existing operational seasonal prediction systems in East Africa? Does the analysis offer any specific insights on how to deal with nonstationarity in this region, beyond just watching it as a general point of concern?

These are just examples. I would defer to the authors regarding the best emphasis, but I do think it could be more specific than the framing that is currently offered.

Specific Comments:

Section 2:

1. How was the geographic area of analysis selected? Is there evidence that the region

is coherent when it comes to large scale drivers of March-May precipitation? And can the authors demonstrate that their results are not overly sensitive to small changes in the region used to calculate PCs? If the region includes significant heterogeneity in Mar-May teleconnections and/or if the analysis is sensitive to small changes in the borders of the analysis region then the authors might want to target a more specific region defined on the basis of correlation strength with proposed SST drivers or on the basis of homogeneity in March-May precipitation variability.

2. The authors note that CHIRPS agrees well with other state-of-the-art precipitation datasets, but it is not clear whether they mean that the products agree well in this region or just that they agree well in general. Given the importance of CHIRPS to this analysis I think that it would make sense to include a timeseries of CHIRPS and other common precipitation datasets for the study region and/or for key precipitation zones within the study region.

Section 3:

3. Section 3.1: Were the PCs calculated from standardized precipitation fields or from the raw precipitation data? The decision to plot standardized precipitation in Figure 1 suggests the former, but reading the first paragraph of 3.1 I see no mention of standardizing precipitation anomalies prior to deriving the PCs, and the description of the calculation makes me think the data were not standardized. If this is the case then it would be useful to see versions of Fig 1a-c that use raw precipitation. As it stands there are some areas that receive relatively little Mar-May precipitation that feature very strongly in these maps. This would be an imperfect representation of the PC loadings if the PCs were in fact calculated without standardization.

4. Section 3.3: The term "skill" is used when presenting regression results, but I don't see any quantitative evaluation of the predictive skill of these models presented in the paper. The authors offer descriptive plots and in-sample correlations, but if the goal is to inform operational prediction systems then it really is necessary to present

some kind of evaluation of out-of-sample predictive skill. This is particularly the case considering that the authors have chosen to use a relatively short time period to fit their models and because trends over the time period of analysis contribute to observed model fit. I recognize that this might be asking for more of a prediction system than the authors want to attempt in this paper, but given the challenges of developing a statistical prediction model on a short baseline–which the authors seem to be recommending–some discussion of predictive skill evaluation would seem to be important.

Section 4:

5. This section offers an excellent summary of challenges facing seasonal prediction in a changing climate, but it offers relatively little in the way of conclusions and interpretation specific to this study. I would encourage the authors to use the Summary section to integrate the analyses presented in the paper–e.g., the relevance of the CMIP5 analysis to seasonal prediction using their proposed indices–and to tie their results more specifically to a way forward on seasonal climate prediction for East Africa. This could include questions I listed in my General Comment, but it doesn't have to.

Tables and Figures:

6. Table 1: It would be useful to provide the name of the model in addition to the name of the modeling group (e.g., which NCAR model was used)?

7. Figure 3d: The spatial variability in correlations suggests that the defined study region does not respond homogeneously to the selected large-scale forcers. This argues for a more systematic method for defining the region of analysis or, at the very least, a discussion of how and why correlation values differ within the region (e.g., why correlation in northern Tanzania is opposite that in surrounding regions).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 3111, 2014.