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Interactive comment on "Seasonal forecasting of hydrological drought in the Limpopo basin: A comparison of statistical methods." *by* M. Seibert et al.

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Author's response to Anonymous Referee #1

Mathias Seibert

May 24, 2016

Dear referee,

we would like to thank you very much indeed for your comments on our manuscript.

Reply to the original comments:

1. Page 7, line 3: "The standardised streamflow indices (SSI) are calculated for each station at the scale of 6 months. SSI_6^May of May at that scale covers the desired main runoff period from December to May, henceforth named SSI_{DJFMAM} (Figure 2)". When discussing the SSI, it is not clear if the SSI is a single value (averaged or summation?) for the months Dec-May or each month has its own SSI value. I would assume that there is only one SSI value for Dec-May, in that case Figure 2 is showing the Box-Plots of Monthly streamflow and not the SSI. I don't see the use of Figure 2 in this manuscript in relation to SSI.

   - Agreed. The figure does not directly help the reader to better understand the SSI. However, it was meant to help the reader to understand what we defined as the "desired main runoff period". While the complete removal of the figure could benefit the overall length of the paper we would like the

promote its use as a way to inform the reader about the region's seasonal regime, helping those not familiar with Southern Africa's climate. Therefore, we moved the figure reference to the description of the study area (section 2.1, page 4, line 16).

Regarding your critique that it is not clear whether "SSI is a single value...or each month has its own SSI value", we added the following two sentenses at the beginning of the section, hoping to clarify this: "In streamflow standardisation a time series is transformed to a normally distributed time series, which can be applied at different temporal scales. At the chosen scale, the respective period (for example January-February) is averaged annually and then standardised based on all annual values present in the time series."

2. Page 13, line 2: " Therefore, the RFOR predictor importance was modified for comparison ." How was it modified?

   • To be very clear and avoid confusion for the reader, we have deleted the first two sentences of this paragraph, so that we start directly with collinearity and then explain, how predictor importance is calculated: "Collinearity of predictors can affect the importance estimation, since predictors might easily replace each other in the regression trees if they have a similar predictive strength. This can cause several effects. On the one hand, the importance per single predictor might be underestimated, if it is not located at an important position in all regression tree models. On the other hand, in presence of collinearity there would be multiple predictors with underestimated predictor importance. Therefore, the results of RFOR predictor importance are summarised for comparison with the MLM partial coefficient of determination. Closely related predictors are merged as relative group importance, calculated as ..." (page 13, lines 10-15)

3. Suggestion: As ANN is not bound by any linear assumptions (as opposed to

MLM), the use of the MLM predictors which were selected based on Pearson correlation (a linear technique) and relying on MLM stepwise predictor selection has limited the performance of ANN in this study. I suggest that in the future studies, the authors do not bound ANN to limited linear selection of inputs (predictors) and investigate a wider range of inputs using either a simple method of trial and error with ANN or more complicated methods such as mutual information or genetic algorithm to select ANN's inputs.

- Indeed, it is likely that the ANNs performance might have been reduced by the chosen predictor selection. In future studies we will prefer to keep MLM and ANN predictor selections completely independent.

The authors would like to express their appreciation for the received revisions and suggestions. Thank you very much.