

Interactive comment on “Long-term precipitation forecast for drought relief using atmospheric circulation factors: a study on the Maharloo Basin in Iran” by S. K. Sigaroodi et al.

S. K. Sigaroodi et al.

qchen@rcees.ac.cn

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1. General comments:

(1) This paper presents results of long-term forecasts (10 months ahead) of precipitation using data-driven techniques. This is an interesting topic, but the current version of the paper has a number of shortcomings.

Reply: We thank the reviewer for the general comments on the paper. We also think it is interesting and important to make long-term prediction of precipitation for drought relief, especially for the arid region such as the Maharloo Basin in Iran. We agree there

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are still several aspects to be improved in the study; however, the current results are already quite attractive.

2. Detailed responses to each comment

(1) Forecasting precipitation 10 months ahead is obviously a very ambitious objective and the results show that the accuracy of the forecasts is very low. The paper claims that the precipitation over the Maharloo basin can be forecast about 10 months ahead. Of course the forecasting is possible technically, but the question is how good are the forecasts to use them reliably for a practical purpose, e.g. to support drought risk management – which was the motivation of the paper. In my view, this aspect is not sufficiently argued in the paper.

Reply: We do not agree with this argument. We need to realize that sometimes the R2 is low, but it is significant (P value). If we look at the Table 9, most of the predictions by ANN model are good enough, and can provide information to support drought relief.

(2) Moreover, the paper does not show any attempt to analyze the uncertainty of the results and how the uncertainty may influence any pro-active decision to be made for drought management.

Reply: We agree this is a very good comment. Uncertainty analysis is a very hot topic nowadays, in particular when model is involved. We of course release this importance, and have carried out the work. However, one paper may not cover all aspects, and the part of uncertainty analysis is presented in another separate paper, which is submitted to JEI.

(3) The number of observed rainfall stations (four stations) used for the size of the basin (31,500 km²) is also very low.

Reply: The heart of the research is to try to do something useful basing on the limited monitoring stations. If we have enough monitoring stations, why should we take such an efforts to make a model basing on the circulation factors? This comments is really

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strange.

(4) The relatively better performance of the non-linear ANN model compared to the linear regression is not new.

Reply: Yes, we agree the method is not new. However, this is not the interest of the paper. The innovation of this study is the use of circulation factors to predict long-term precipitation in data-limited region for possible drought relief.

(5) Why 80/20 splitting of the data for calibration/validation is not discussed? The validation using only 20% of the data is not convincing.

Reply: In most textbook about ANN model, data splitting is either 80%/20% or 70%/30%. We do not think there is much problem with the use of the data for ANN model construction and validation. However, we are happy to test different data splitting and check the effects on the results.

(6) The forecasting is done treating each month separately, so the temporal correlation is not considered. Is 10-month lag always gives the best result? It would have been more interesting if the results were presented to show how the uncertainty or the forecast accuracy changes with the lag-time.

Reply: First of all, although forecasting is finally done treating each month separately, auto-correlation analysis has been conducted before this, which did not show significant effect. Therefore, the model has considered the aspect of temporal correlation. The results show that in most cases (10 out 12), 10-month lag gives the best result.

(7) The link of the study to drought risk management is also poorly presented, and even the widely used drought characterization index, the Standardized Precipitation Index (SPI) is defined differently in this paper (Eq. C1).

Reply: Many thanks indeed for this essential comment. The link to drought risk management has been largely enhanced in the revised manuscript, based also on the comments from anonymous reviewer 1. Please refer to the revised version 2, which

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