Interactive comment on "Spatially explicit seasonal forecasting using fuzzy spatiotemporal clustering of long-term daily rainfall and temperature data" *by* M. B. Plain et al.

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Editor overview after final responses and revised document

Manuscript: Plain et al. 'Spatially explicit seasonal forecasting using fuzzy spatiotemporal clustering of long-term daily rainfall and temperature data'

Overview:

I thank the reviewers and the authors for their participation in this review process. This is now my evaluation of the final manuscript submitted and the responses to the reviews to put forward my recommendation for publication in HESS.

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Interactive Discussion

Discussion Paper

Both reviewers suggest major revisions (one suggested rejection based primarily on the fact the paper did not assess seasonal anomalies - more later on this). The question is have the authors submitted a paper that has undergone major revisions and addressed all the reviewers comments. Sadly I have to conclude that the authors have not developed the paper enough in this regard. I would not, therefore, recommend it for publication in HESS. Overall when I assess the changes made between manuscripts there has been very little additional work done here (some text changes (mainly clarifying sentences) and one considerable change to Table 3), this is not good enough considering the reviewers comments. I think this is a pity that the authors did not fully engage in more detail with some good reviewer comments and therefore re-evaluated their manuscript in detail to improve the paper. My main reasons for rejecting the paper for publication to HESS are identified below:

Reviewer 1:

- Main point: Dismissing 'seasonal forecast models/large amount of literature on downscaling' - The authors have added in total two 'sentence sections' to cover these comments in what they suggest is 'word the section somewhat more careful and presenting the method as an alternative'. I do not think this is a good enough job and still this has not even clarified some of the comments made about statistical methods in the authors response. A better job needs to be done here and that relates the results to these 'alternative' methods where appropriate to develop useful discussion - Diez et al. 2005 should also appear in the references.

- Zero values for missing values - I think as a minimum the authors should clarify for there 'extremes' what the higher likelihood of a dry over a wet day is for the reader to make their own conclusions

- Section 3.2 and Table 1 - The title is not adequate to allow the reader to understand what is being compared. I think it's fair to say there are a very few 'improvements', some limited improvements and some 'artefacts' of improvement

(and poorer results) and this is not brought out correctly in the discussion. Also the reviewers concern about such differences being an 'artefact' of the sample population is not discussed. This could be statistically evaluated to test the 'improvements' but this has not been done, it should be and be in Table 1...

- Quality of patterns in Figures 7 and 8. I too agree with the reviewer that these predicted patterns are generally poor (and certainly worse than the authors discuss - they seem to concentrate on artefacts). Certainly for temperature things are poor (note the modelled 6month predictions do not have the same colour scale and they should to compare effectively). But they cannot also on the one hand suggest they are developing an 'operational model for agriculture' and then be happy with temperature predictions that are way too high - what use would this be for practical applications? (note no such discussion is entered into in this manuscript - see further comments below). In fact the authors must appreciate in their discussions of model prediction accuracy (section 3.3) that high correlations (temperature) do not take into account the overall magnitude differences in prediction that relative error does. Anyhow I think a better more reflective assessment of the models capability needs to be developed and this is what was being suggested by the reviewer. The authors have not tackled this effectively in the revised manuscript.

- Rainman as a reference system - This response could have somehow been directly noted in the paper

Reviewer 2:

- Use of anomalies (a major issue of model evaluation and in fact context of model application) - I am in agreement with reviewer 2 on this point. If the authors are not assessing what is the most sensitive assessment then this needs to be

clarified in the text (it really isn't discussed) Again there is a justification of using correlation, but the predictions could be significantly in error and still have a 'correlation' of some kind. But importantly the anomalies are poor, in this regard this should be related to this being a usual benchmark for such results and that perhaps the model has failed in fact as a useful forecasting model of seasonal rainfall and temperature... Surely this comparison of the anomalies shows the model to be extremely poor in a forecasting mode. I agree with the reviewer that predicting the general seasonal cycle will have a high likelihood of producing stronger correlation statistics. Note the title for this table is not good enough (no dates of comparison for example). Note again (and this comes up later too) the authors note there model is for applications. Then the discussion, conclusions and objectives need to make this very clear, and in that case if the predictions and forecasts mean that it has operational usefulness. I would say that I don't believe the authors have made this distinction of what the model will be used for and reflected this in their intro etc. at all like they suggest they have done.

- Are Rainman predictions a valid comparison - It's not clear to me in the text that this has been 'fixed' and needs further work to ensure comparisons are the same.

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