

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.

Anonymous Referee #1

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Title: Spatially explicit seasonal forecasting using fuzzy spatiotemporal clustering of long-term daily rainfall and temperature data.

Authors: M.B. Plain, B. Minasny, A.B. McBratney and R.W. Vervoort.

The paper describes a methodology to produce statistical seasonal forecasts using a fuzzy classification of long term daily temperature and rainfall data. The paper needs to go through major revisions in order to be published.

—Major comments —

The authors seem to mix up climate and seasonal forecasts. These are two quite

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different forecast ranges whose relevant physical processes may be quite different. For example, for seasonal scales the detailed description of the atmospheric chemistry may not be so relevant. The chemical processes in the atmosphere become very relevant on climate scales.

The authors dismiss quite rapidly the use of seasonal forecast models because their resolution is coarse. They are obviously not aware of the large amount of literature describing downscaling of seasonal models or literature on regional climate models, run using boundary conditions from coarser grid seasonal forecasting systems. A couple of references for the downscaling of seasonal forecasting models are: Coelho et al (2006) Meteorological Applications vol.13 pg 73-82 and Diez et al (2005) Tellus vol. 57 pg 409-423

In section 2 it is said that the stations are 107 but only 75 can be used. This is quite a small number of stations to be able to characterise a part of Australia, which from Fig. 8 seems to be at least 900x900 Km.

Line 15 on page 1163- here it is said that if the rainfall is missing, a value of zero is assigned. This will lead to an increase of the non-event. How are the authors going to cope with this when scoring?

In section 3.2 the authors describe the results in table 1. They claim that the incorporation of SOI and elevation significantly improves the results but they do not show it. Instead they show the difference between current SOI and SOI lag periods in table 1. I wonder whether the differences we see in table 1 are 'significantly' different or just an artefact of the small sample of stations to verified against (I assume the 75 stations are used).

In the discussion (section 4), the sentence on line 20 pg 1172 'The added temperaturebetween the models', is a bit confusing: does it mean that because you have longer series of data you get better results? If yes, then why at the beginning of the paper the authors want to reduce the data size (line 23 pg 1163)? Perhaps a sensitivity study is

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needed to see how results are affected by the decrease of sample size.

The match between the patterns in figure 7 and figure 8 is rather poor. Moreover Figure 7 top right panel shows a strange behaviour in the top part of the area (colder temperature shading stops abruptly).

—- Minor comments —-

In the introduction (section 1) there is no mention of how the paper will be structured.

Figure 1 is not needed, as this is a paper and not a technical note

In section 2 the details on the version number of programs and various pieces of software are not needed as this is not a technical note.

On page 1165 line 12: why is Compositional Kriging used as a reference system?

On page 1167 line 17: why is Rainman used as a reference system?

Figure 8: The 75 stations could perhaps be put on the figure?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1159, 2008.

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