

Interactive comment on “Applicability of ensemble pattern scaling method on precipitation intensity indices at regional scale” by Y. Li and W. Ye

Anonymous Referee #3

Received and published: 21 July 2011

The paper presents an application of the pattern scaling method to evaluate changes in extreme precipitation indices from global mean temperature change of different GCMs projections over Australia. The variability of changes is evaluated within each GCM and considering all GCMs as an ensemble. Different levels of spatial scale are considered: grid cell level, regional scale (5 administrative units in Australia) and the whole Australia. The authors concluded that results show stronger evidence for changes when spatial scales are increase. It is not clear however if this is an expectable effect of averaging.

The introduction presents relatively well the approaches in the literature, although it is not clear why "skill based weighted ensemble methods" are not appropriate to their

C2950

case-study (as stated on page 5231). The investigation of such approaches for the combination of projections would bring additional value to the results of the paper. The methodology section is confusing and the results are, at the end, a lot of numbers and percentages, presented in a repetitive way that makes the reading of the paper very boring. The discussion section basically focuses on the fact that a small sample of GCMs projections is insufficient for robust conclusions, which raises some doubts about the validity of paper's results themselves. It also lacks a comparison of the authors' achievements with other studies in the literature: do they converge/diverge, point out to the same limitations, etc.?

In my opinion, this paper is not suitable for publication in HESS. Furthermore, I agree with all the comments of the other two reviewers of this paper to encourage the authors to revise it and, eventually, submit it again. Additionally, I also think that the study needs more investigation to make the results innovative and interesting to the hydrologic community.

Finally, I must say that I was surprised by the fact that the same paper was already submitted to another journal in 2009 (as indicated by Reviewer 1) and that the authors have ignored the reviews provided at that time (they probably received more than one review), even for simple language mistakes. It definitely does not encourage reviewers to do a hard work in their reviews to help the authors to improve their manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5227, 2011.

C2951