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Hydrology and Earth System Sciences Discussions

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

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Thank you very much for your valuable comments. Please see our responses below.

Review comments on the manuscript Applicability of ensemble pattern scaling method on precipitation intensity indices at regional scale by Y. Li and W. Ye (Hess-2011-149)

Anonymous Referee #1

Comments: This manuscript had been submitted to International Journal of Climatology in 2009. I acted as a referee. I wrote a four page review, recommending accept with major revisions. This manuscript seems to be an exact carbon copy of the version that I have read two years ago. Even simple spelling errors have not been corrected.

Therefore, I do not find any reason to start to study the text in detail again.

Response: We could not get any response from the Int. J. Climatology after we submitted to the journal two years ago. We made numeric request to the journal for the feedbacks but heard nothing back, hence we did not have chance to see your comments. We finally made decision to withdraw the MS from the journal and submitted to HESSD.

Recommendation: Accept with major revisions This paper introduces a version of the pattern-scaling method that is an useful extension to the previously-used versions. This method utilizes the GCM data effectively, both the simulations performed with several SRES scenarios and model data for different time slices. The extreme precipitation indices to which the methodology is applied are relevant. Moreover, I agree with the authors on the usage of a large ensemble of models in compiling the projections rather than a single or a few models (the latter approach is fairly common in literature, unfortunately). In this study, the need for a large ensemble rules out the possibility of utilizing regional climate models, although their higher spatial resolution would be beneficial in simulating small-scale phenomena that typically produce the largest precipitation intensities; for most of the GCMs, this kind of dynamical downscalings are not available. Accordingly, I recommend publication of this paper. However, the manuscript requires shortening and significant revisions.

Response: We have made major revision based on 3 reviewers' comments, including shortened the MS significantly.

1 Main comments and recommendations Comment: 1. Evidently, the main issue investigated in this paper can be expressed: "How many percent the regional value of a certain precipitation index changes per a 1_C increase in the global mean temperature?". I find that this idea should be stated explicitly in the beginning of the manuscript, perhaps both in the abstract and in the introduction.

Response: We inserted your comments to the abstract and the introduction

Comments:On the other hand, I find that the paper does not give any answer to the question whether the relationship between the global warming and projected changes in the indices are exactly linear or nonlinear (p. 2, l. 4-7; p.2, l. 13-15; p. 5, l. 8-10). The statistical significance of the linear regression coefficient does not tell anything about this. The value of R2 is not very high for most of the models. See also main comment 5.

Response: In this MS, the linear correlation was examined with the t-test. The 0.05 of the p-value of the t-test was used for the significance level of linear correlation.

Comment: Section 2. The methodology should be documented more systematically. Divide this section into three subsections:

(a) Determination of the precipitation intensity indices (P99, RP20, RPD) from the model output data (b) The pattern-scaling methodology to calculate the regression coefficients (_V 0) for individual GCMs (this part of the text is already rather good) (c) Determination the "final" coefficients (including the uncertainty analysis) from the inter-model ensemble data

Response: Methodology part was rearranged according to the comments

Comment: 2. Section 3. The discussion is very detailed. For example, is it indeed necessary to list the regression coefficients for all the 8 regions, all the 12 models and all the three indices (Tables 1-3, sections 3.1-3.3)? I recommend that you should focus on changes for the entire area (Australia) since these findings are the most robust ones. Results for the smaller regions might be presented on your www page, merely including a brief discussion in this paper. This would shorten sections 3.1-3.3 substantially (accordingly, I have not presented many detailed comments on these subsections). Consider shortening of subsection 3.4 as well. (Of course, it is the author of the journal that ultimately decides how much space will be given to this article.)

Response: The discussion was shortened with focus being mainly on national level.

Comment: 3. Statements like "showing that almost no linearity could be detected" (p. 8, l. 2-3);"showing strong linearity signals", "almost no linearity could be detected" (p. 10, l. 15-16); "the high linearity", "good linearity", "good linear performance" "linearity weakens quickly" (p. 18, l. 10-14), etc., are confusing. It would be more informative to state that the linear regression coefficient was found to be (not to be) statistically significant. Note that the significance of the coefficient does not indicate that the actual relationship is linear. Even a nonlinear dependence may produce a high level of significance, if the sample size is large and/or the scatter of the data around the regression curve is small. Moreover, even if the relationship indeed were linear, the significance of the regression coefficient does not tell anything about the slope of the regression line.

Response: The confusing and words were replaced A t-test was used to examine the statistical significance of the linear correlations.

Comment:

4. Section 4 mainly contains well-formulated self-criticism that is of great utility for the reader. However, I do not entirely agree with the statements presented on l. 6-8 of p. 17: It is too optimistic to claim that 'most of the GCMs' pass the 95% level at national level (e.g., in Fig. 3, only 6 models out of 12) and 'perform reasonably' at the regional level. The uncertainty range of the projections is fairly large for impact assessment studies.

Response: Agree and was rephrased as per the comments.

Comment: 5. P. 18, l. 21-24: I find that this methodology does not offer any opportunity to evaluate the model performances, select models or to assess the weighting coefficients. For example, it is possible that the actual future change in some precipitation index will prove to be close to zero. In that case, just those models that produce the strongest and statistically most significant signals would be the most erroneous ones (perhaps I misunderstood your idea?).

Response: The statement was deleted. This research proposed a method which could make the best use of the available GCM daily data for climate change impact on precipitation intensity assessment while more accurate and high resolution data are still pending. This research attempts to highlight and examine the fundamental assumptions of the pattern scaling technique, as well as uncertainty, and contributes to the practical application of GCM-derived climate projections. This study is not aiming to evaluation the performance of GCM in precipitation simulation. Comment: 6. I suggest an addition to the conclusions: The application of your results might be based on a delta change method. For example, if the regression coefficient for an index were 6%K-1 and the global mean temperature increased by 2_C, the resulting response would be 12%. For adaptation studies, one could then multiply the observational value of the index by 1.12 in order to have the quantitative projection.

Response: Added

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