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)

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. Instead, see below a copy of my review for Int.J. Climatology. Note that all page and line number **refer to that year 2009 version** of the manuscript, **not** to the present one.

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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.

1 Main comments and recommendations

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.

2. 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 R^2 is not very high for most of the models. See also main comment 5.

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

   (a) Determination of the precipitation intensity indices (P_{99}, RP_{20}, RPD) from the model output data

(b) The pattern-scaling methodology to calculate the regression coefficients ($\Delta V'$) 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

4. 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.)

5. 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.

6. Section 4 mainly contains well-formulated self-criticism that is of great utility for the reader. However, I do not entirely agree with the 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.

7. 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?).

8. 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 $6\%\mathrm{K}^{-1}$ 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.

9. Are figures with colours allowed in this journal at present? If not, you should redraw all figures without colours and ensure that the essential information is still discernible.

2 Minor points

1. Should the country name 'Australia' be included in the title of the paper?
2. P. 2, l. 13: precipitation intensity indices change -> change in precipitation intensity indices?
3. P. 2, l. 14: global mean temperature change at the national level. Ambiguous, please reformulate.
4. P. 3: the first sentence is long and could be divided into two parts. On l. 2, should 'result from' -> 'resulting from'?
5. P.3, l. 15: It has -> Various versions of the technique have?
6. P. 3, l. 18: global responses -> global-mean responses?
7. P.3, l. 27: disasters these may -> disasters that these may?
8. P. 3, l. 29 - p. 4, l. 1: The main reason for the more intense (frequent?) extreme rainfall events is an increase in the atmospheric water vapour content rather than the enhancing hydrological cycle. See the discussion presented on p. 1439 in Kharin et al. (2007), for instance.
9. P. 4, l. 1-2: the climate change impact -> the impact of climate change?
10. P. 4, l. 5: much less -> much lower (much weaker)?
11. P. 5, l. 3: some meteorological phenomena producing excessive rainfall are of mesoscale rather than of synoptic scale.
12. P. 6, l. 3: 'The reason is partly due to our limited understanding of the climate system'. In my opinion, this is not generally true. Remember the possibility that some local or regional changes may be nonlinearly dependent on the global temperature change, by their nature.
13. P. 6, l. 18: change pattern -> pattern of change?
14. Eq. (3): Explain more explicitly, how the standard deviation has been calculated.
15. P. 7, l. 5, p. 8, l. 5, etc.: change rate -> rate of change?
16. P. 9, l. 7: 0.86 -> 0.80 (after shortening these sections, check all the numerical values presented.)
17. P. 10, l. 1-3: How can you be sure that **all** sample values were distributed randomly? What does the randomness exactly indicate here? (The same statement is expressed later in the paper.)
18. P. 10, l. 3: ISPL -> IPSL?
19. P. 11, l. 24: the largest among all -> larger than any of?

20. P. 14, l. 1: Why such an extremely dense (0.25 x 0.25°) grid? How have you interpolated the model data onto this grid?
21. P. 15, l. 4-5: Why GCM-internal variability **from pattern scaling?** I find that the method by itself is not the reason for variability.
22. P. 15, l. 8-12: This is documentation of the methodology. Could it be included in section 2?
23. P. 15, l. 14: 'change rates of the 3 indices all increased under global warming'. What is meant by 'rates increased'?
24. P. 15, l. 23-24: Is it possible to perform this in a reverse order: first to calculate regional averages and only thereafter the percentiles?
25. P. 16, l. 2: not significant -> not positive?
26. P. 18, l. 20-21: all GCMs equally plausible, no weighting. This important information should be given in the introduction and/or in section 2.
27. P. 19, l. 4-8: For most of the GCMs, dynamical downscaling by a regional model is not publicly available. Therefore, one is justified to use global models in this study, even though the resolution is lower.
28. P. 19, l. 14: 7.21 -> 7.19?
29. Tables 1-3: Is all this detailed information necessary? If these tables or some of them are retained, note the following suggestions:
    (a) The title of the table should be complete. Give the unit of the variable ($\%K^{-1}$), the full names of the 8 regions etc.
    (b) Instead of numbers 1-12, use the model acronyms to facilitate reading.
    (c) Only those values that are statistically significant with boldface font.
30. Figures 1, 3 and 5: Too much information in a single figure. It is very hard to see what points and regression lines refer to a certain model.
31. Figures 2, 4 and 6: If the shaded bars indeed represent the intervals between the 5th and the 95th percentiles, asymptotically only 10% of all points should be located outside the bars. However, there seem to be much more such outliers. Please check that the calculations have been done correctly and then explain the reason for this discrepancy. No unit has been given for the y axis variable. On the x axis, model acronyms would be more informative than the numbers (if this is technically feasible in your plot program).
32. Figure 4, caption: 'annual and seasonal' (?)
33. Figure 7: Why there is a different colour scale in the panels? Use colours that make the zero contour clearly discernible. Is it possible to show the coefficient of variation (standard deviation divided by the mean) on the right-hand panels; this would give information about the significance of the means or medians. If printed without colours, the information presented in these figures would become unreadable.
34. Supplementary figures: are these necessary?