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Discussions

Interactive comment on "A two-stage Bayesian multi-model framework for improving multidimensional drought risk projections over China" *by* Boen Zhang et al.

Anonymous Referee #1

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In this paper, the authors present a Bayesian multi-model framework used for drought projections analysis. The method is presented, compared (regarding precipitation and potential evapotranspiration) to the ensemble mean when applied over an historic period, and then projections are realised and analysed. I will be honest: I am not familiar with Bayesian and copulas frameworks. Therefore, I will not comment neither the statistical aspects of these methods nor whether this work is really new and sound. I let that to other reviewers. Rather, I will concentrate my review on the climate change aspect as well as whether the presentation and analysis correspond to what is expected in a journal like HESS.

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Discussion paper

Major remarks:

Presentation of the performance of BMA historical projections: I have the feeling that BMA is slightly oversold. I agree that it performs better than AEM, but cases where it does not perform better are most of the time not mentioned. I spotted some of them in my miscellaneous remarks. I think this is a pity, as understanding and explaining why this is the case could be very interesting. I generally feel that the manuscript lacks a lot of analysis and discussion of the results.

Validity of the method under climate change: the authors analyse the performance of the method under historic data. However, the method is then applied to future climate projections. As the change of climate is rather important in the future, the validity of the method in terms of extrapolation must be questioned. Usually, especially in hydrology under change (see the many "split sample" papers and applications of this method), the historic data is split into two parts, one part being used for optimising the parameters of the model/relation and the second part being used as an independent evaluation. While this does not guarantee that the method might be performing well in the future (since the historic data might be less contrasted that future data), this is a condition that is necessary to satisfy. If the method does not perform well on historic data that is independent, then it is very likely that its behaviour will not be satisfying in the future.

One point that became evident to me when seeing the figures were even not mentioned by the authors. Namely, the two RCPs show very close results with BMA! Check for example Figure 9 (precipitation): if you compare RCP4.5 and RCP8.5 for a specific indicator, the spatial pattern is very similar and magnitudes are also very close. For PET, this is a bit less the case, but still, strongly similar spatial patterns can be observed. Later on, in Figure 14, several boxplots of the drought duration are identical for both RCPs. This is a bit worrying I would say, as I doubt that raw projections also show this behaviour. Could it be that BMA constraints too much projections? To understand that, similar plots without BMA could help.

A second point is seen in Figure 15: some RCPs boxplots (well many in fact) are flat. This means that all projections give the same number of drought episodes. While this could be for an indicator showing small number (e.g. <5), here we are often around 30! This is a bit strange too.

Miscellaneous remarks:

Abstract, line 11: that is not entirely true, as some adaptation strategies are dedicate to tackle flood issues. Line 22-23: which aspect of the risk is expected to double? Frequency? Duration? Intensity? Please be more specific here.

Line 34 and others: there are some surnames in the citations, please correct.

L. 36-38: using ensembles and not restraining the analysis to the mean is already standard in hydrology, including for droughts. Please check for example some examples in HESS: Vidal et al. (2016) and Parajka et al. (2016).

Line 56: while the statement is true, Ramos et al. (2013) is about forecasts, not projections. Extrapolating the conclusion from Ramos to projections is far from trivial I believe. I suggest removing this citation.

L 131: please define the pdf acronym.

L. 232: I quite disagree regarding precipitation. On Figure 4l, we see rather high differences on Eastern China.

L. 250: please define COR.

L. 252-254: this is not so clear for the other ones. See for example the winter PET.

L. 252-254 and lines 260-262 are contradictory

L. 264-265: I rather disagree; there is quite often a factor 2 between OBS and BMA. In addition, some divisions show a peak timing that is not adequately represented.

L. 267-268: nice to finally see some attempt of discussion of the results. However, I feel we need more: why is this the case? Explain! Please also provide a reference.

L. 269-270: again, I think that the presentation is unfair: the number of opposite behaviour is rather similar from what I see on the figure.

L. 306-313: this is methods, not results. Please move that part in Methods.

L. 312-313: there is no justification why these 3 copulas were chosen for these divisions.

L. 352-354: please remove, this is in the caption already

L. 362: occurrenceS

References: Chambers et al. Is it a book? Please specify the type of work.

Figure 1: we miss a legend in order to have the possibility to understand panels i and j.

Figure 2: while there is only one colour scale, it seems to me that the flat low lands in Eastern China are plotted in different greens. Can you please check?

From Figure 4 onwards: please specify the period of study used for these figures

Figure 4 and 5: we need the AEM plots in order to more easily compare the 3 datasets. That can help us understand the spatial differences, as here for example we have a very unclear idea of of far/close AEM and BMA are from each other.

Figure 6: why there are no line around OBS to help use assess the error, as in classical Taylor diagrams? That would help a lot.

References:

Parajka, J., Blaschke, A. P., Blöschl, G., Haslinger, K., Hepp, G., Laaha, G., Schöner, W., Trautvetter, H., Viglione, A., and Zessner, M.: Uncertainty contributions to low-flow projections in Austria, Hydrol. Earth Syst. Sci., 20, 2085–2101, https://doi.org/10.5194/hess-20-2085-2016, 2016.

Vidal, J.-P., Hingray, B., Magand, C., Sauquet, E., and Ducharne, A.: Hierarchy of climate and hydrological uncertainties in transient low-flow projections, Hydrol. Earth Syst. Sci., 20, 3651–3672, https://doi.org/10.5194/hess-20-3651-2016, 2016.

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