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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A statistically based seasonal precipitation forecast model with automatic predictor selection and its application to Central and South Asian headwater catchments" *by* Lars Gerlitz et al.

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This is a very interesting approach and I enjoyed reading the paper. I do have some questions relating to data quality, statistical significance if the method is automated to a large number of regions, the very high forecast correlations apparently obtained, and the SST predictor regions for the Central Asia locations. Based on these questions, my recommendation is for major revisions. I do not have sufficient expertise to provide any detailed comments on the technical aspects of the cell-forest forecasting methodology.

Major comments:

1. Data quality. If the methodology is applied in an automated way to a number of different regions, how can data quality issues, which can vary considerably from location to location, be dealt with? And specifically for the case of Central Asia, the number of reporting stations varies dramatically over the 1948-2014 period considered here. I think the authors need to comment on both the general issue and provide some more information for the specific cases of Central and South Asia (e.g., plot the number of reporting stations as a function of time and assess the sensitivity of their results to the number of stations).

2. False positives if automated. Additionally, if the method is run for a large number of locations, some regions will get high prediction skill purely by chance. (If, say, a 95% significance criterion is applied for the validation period for each location, approximately 5% of the locations will appear significant by chance.) How would this issue be dealt with?

3. Forecast correlation magnitude. I'm somewhat confused by Table 2. Are the correlations for the training period or for the evaluation period? And is the seasonal cycle included when calculating the correlation or is it removed first? If not removed, then numbers for when it has been removed should also be shown. If I'm reading the table correctly, there are several forecast correlations between 0.7 and 0.86 – I'm not aware of any forecast correlations for precipitation (with seasonal cycle removed) that are anywhere near that high for any region using any forecast method. As an example, it appears that the forecast correlation for Naryn is 0.86 for JFM forecast from Dec. As far as I know, that's also considerably higher than any potential predictor for the region (SSTs, lagged precipitation, etc.). If I've read that correctly, that's a rather extraordinary result that will require extra evidence to be considered plausible – perhaps by identifying a few individual high-correlation predictors and showing that they are linearly independent. It would also be useful to put those numbers into the context of other reported forecast skill for the regions, especially from the usual seasonal forecast centers, and of the skill of a pure persistence forecast.

4. SST relationship for Central Asia. For the March SST correlations shown in Fig. 2, I don't understand why there is no signal at the equator in the central Pacific – I was expecting an ENSO pattern (and that is also what I get if I do a quick correlation based on GPCP data).

Minor comments:

1. I found the use of "exemplarily" to be somewhat distracting. I would suggest something more like "the model was applied to two test cases" or "two example cases." If the two regions really are exemplars, what makes them particularly useful or representative of the approach? Were other regions considered and, if so, why were they not included?

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