Hydrology and Earth System Sciences Discussions

Interactive comment on "Prediction of monsoon rainfall for a mesoscale Indian catchment based on stochastical downscaling and objective circulation patterns" *by* E. Zehe et al.

Anonymous Referee #4

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General Comments:

An attempt is made to objectively identify daily large-scale 500-hPa geopotential height patterns over a very large spatial domain (40E–95E, 5N–35N), that are related to rainfall over a small ∼50-km square region in NW India. A stochastic downscaling model is constructed, based on these relationships, which is then evaluated in terms of (a) its ability to reproduce of daily station rainfall climatological statistics, and (b) the its ability to "predict" season rainfall totals, given the large-scale atmospheric circulation patterns.

Judging the paper in terms of its contribution to (a) and (b), I find some aspects of interest, but also serious shortcomings. Any reasonable stochastic "weather generator"

should be able to reproduce daily station rainfall climatological statistics adequately. The current model shows some substantial unexpected biases (see specific points below) that need to be explained. Cross-validation is essential to evaluating model performance, but I found no mention of it. This must be addressed.

From a meteorological perspective, the "predictive" aspect of the study is problematic. Statistical downscaling models must be based on plausible physical relationships between large-scale atmospheric predictors, and local daily rainfall. This paper is based on a model previously developed for Central Europe, in which 500-hPa geopotential height fields over a very large spatial domain (here 40E–95E, 5N–35N) are used as predictors. Large-scale 500-hPa ($\sim$5 km altitude) height fields are a natural choice to represent circulation patterns in middle latitudes, but this is much less clear for the Indian monsoon. Geopotential height is a much poorer indicator of circulation at low latitudes because of the Coriolis effect; mid-tropospheric levels tend not to be appropriate for monsoonal circulation patterns whose polarity reverses with height. Some supporting evidence, preferably both from the published literature, as well as from simple exploratory analysis is essential, to support of the choice of predictor variable and its domain. For example, correlation of seasonal averages of station-average rainfall would help.

The results of Sect. 3.2 are encouraging, but a much more thorough sensitivity study (esp. to domain choice) is needed; the present treatment is much too superficial for any meaningful scientific contribution. The sensitivity of monsoon rainfall to large-scale geopotential height is not well understood, but your results do suggest a link, and this should be discussed further. On the face of it, there is little reason to expect the very large spatial domain to be relevant to a small catchment in NW India.

In short, the "CP" scheme is a promising candidate for use over India, but the current implementation does not do it justice, nor sufficiently demonstrate its usefulness for India. The presentation is seriously lacking sufficient explanation of the methodology and its application. The paper requires a major revision. If there is insufficient time

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available within the time allowed for revision, then I recommend rejecting the paper.

Specific Comments:

1. Title: "Prediction" should be removed from the title; downscaling is more accurate.

2. p.2, ln.-5: Monsoon breaks are no longer believed to occur "quite randomly", but to be related to intraseasonal oscillations, as argued in the Webster & Hoyos paper that you cite.

3. Sect. 2.1.1 is very hard to follow, even at a conceptual level. A clear description high-level description of the optimization is needed (p. 7, 2nd paragr.).

4. Sect. 2.1.2: more details of the model are needed. It is not clear how conditional amounts are derived. Parametric or non-parametric? The 10-year calibration period seems rather short.

5. P.9, ln.-10: Presumably the model is only applicable within the monsoon period.

6. Sect. 3.1: Here it is not clear whether the results in Table 2 have been crossvalidated. Clearly the model with more CPs would be expected to give a better fit, within the training period. What matters is how it performs on independent data. There is also some quite large sensitivity of max. n_z to small changes in the domain, which is worrying and needs investigation. This may reflect sampling variations. Table 2 must be cross-validated.

7. Fig. 2 is hard to read and would be better plotted using maps. The stations are close together and clearly highly correlated. Maps for each CP would help understand geographical variations, and their relationship with eqs 1–3. For example, the optimization appears to select states that are associated with station differences from climatology. How is the character of model expressed in the results?

8. Sect. 3.2.1: The simulation methodology needs to be described. How many were made?

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9. Table 4: There are some substantial mean biases in the simulations, even within the calibration period. If simulated rainfall occurrence is based on conditional probabilities associated with each CP, where does the bias come from? Table 4 should also be cross-validated.

10. Sect. 3.2.2: Why is there a mismatch in the seasonality of occurrence at these two stations? Seasonality appears to be "built in" to the model (sect 2.1.2), so the origin of this bias needs to be explained. Is there a bias in the mean seasonality of the CP state sequence that could account for this?

11. Table 5: it should be clarified that these correlations are for the multi-year averaged seasonal cycle.

12. Sect. 3.2.3: The simulations in Fig. 9 don't appear to have enough spread. At least 100 simulations are needed to estimate the 95% conf interval.

13. Fig. 9: The simulations at the 2 stations look remarkably different, which is rather implausible, given the size of the CP domain; one would expect large-scale climatic influence to be felt more similarly between nearby stations. How well are correlations between stations reproduced in the simulations, on the interannual time scale?

14. The interannual correlations are encouraging, but need to be better understood to be plausible.

15. Sect. 4: Rather than speculate about SST influence, the paper would be better served by focusing on the meaning of the geopotential CPs.

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