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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.

**E. Zehe et al.**

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First we want to thank the Reviewer for his thoughtful and constructive comments, which we addressed in detail below:

Reviewer: 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 be-

tween 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 ( 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. Response: This is correct. It is well known, that pressure patterns or weather types are suitable for forecasting weather conditions in Central Europe, but also Southern Europe such as Greece. Hence, the choice of objective circulation patterns as predictors for stochastic downscaling of precipitation in Europe appears to us as straight forward. In North Western India, which is closer to the Equator, this is not so obvious, as the Coriolis force is much weaker. However, it is still clear that pressure patterns and pressure gradients must affect circulation at the selected area. We agree that maybe the 700 hPA or even surface pressure level might be a better choice and we will check this in the revised manuscript. Please note, that the size of the selected window for pressure classification is does not mean that the pressure at any point is relevant. It is only the area where the algorithm searches for relevant locations. We will explain this better in the revise manuscript. The whole study is meant as test to explore the question whether the approach is transferable or not, and the answer is partly positive. Table 2 in the current manuscript presents the two essential quality criteria for the classifications schemes of pressure data into CP: The maximum and minimum values of the CP specific conditional daily rainfall probabilities divided be the unconditional average, we call this np, as well as the maximum and minimum values of the cp-specific conditional daily rainfall amount divided be the unconditional average,



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named nz. A reasonable classification scheme has minimum values close to zero and maximum values larger than one. This indicates that the CP-specific conditions differ strongly from the average. Especially the values for the classification scheme show clearly, that the related circulation patterns are suitable to explain deviations from average conditions! Also Figure 2 supports this fact (maybe not in the current format). If we had obtained something like a uniform distribution of rainfall probabilities/amounts between the different CP and maximum and minimum values of np nm close to one, the selected predictor would of course be bad. But that is clearly not the case. We think that this is a much stronger argument for selecting pressure patterns as predictors as simple seasonal correlations.

Reviewer: 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 available within the time allowed for revision, then I recommend rejecting the paper. Response: We agree with the reviewer, that a more thorough sensitivity study of the proposed method is necessary to fully evaluate the applicability to the monsoon system in North Western India. Currently the corresponding data analysis/simulations are under way.

Reviewer: Prediction” should be removed from the title; downscaling is more accurate. Response: We will omit the word prediction in the title of the revised manuscript.

Reviewer: p.2, ln.-5: Monsoon breaks are no longer believed to occur “quite randomly”,



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but to be related to intraseasonal oscillations, as argued in the Webster & Hoyos paper that you cite. Response: This is correct: the original statement is “Ěseem to occur quite randomly”. In the revised manuscript we will add a statement that addresses the reason for the outbursts.

Reviewer: 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.). Response: We will give more detailed information on the downscaling methodology, explain the choice of the objective functions and the optimisation procedure in the new manuscript.

Reviewer: 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. Response: In section 2.2 we explained, that only a ten years time series of rainfall is available at these ten stations. This is a PUB study, as our colleague Anupam Singh even had to digitize these data from paper! Of course ten years is short but it is all we have there (in spite of the long term time series at two stations). If we stop doing research in such an area, we can stop the PUB initiative at ounce! Rainfall probabilities and amounts are estimated non parametric based on frequency analysis.

Reviewer: P.9, ln.-10: Presumably the model is only applicable within the monsoon period. Response: No, the model is continuous in time. However, as no rainfall data were available outside the monsoon period, rainfall probabilities outside the monsoon period are currently estimated to zero.

Reviewer: 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. nz to small changes in the domain, which is worrying and needs investigation. This may reflect sampling variations. Table 2 must be cross-validated. Response: Table 2 currently presents the quality criteria



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for the calibration period. We agree that the sensitivity for a shift in the search window is interesting, and needs to be investigated in the revised manuscript. Of course it is important to check the model performance outside the calibration period, which is done in section 3.2.3. However, it is given practise first to optimise the model insight the calibration period and then do an extrapolation. Why should a model, that is worse in the calibration period, should be of similar quality outside the of calibration period? We disagree that this has to be cross validated especially when only ten years of data are available.

Reviewer: 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? Response: We will improve Figure 2, as it is an essential figure. Rainfall is linked to the CP, through conditional probabilities and amounts, within a transformed multivariate normal process. These conditional probabilities and amounts are shown in Fig.2. The optimisation defines the objective circulation patterns such, that CP conditional probabilities and amounts differ as much as possible from the averages! This is exactly what is needed, pressure patterns that either define dryer or wetter than average meteorological conditions at the Anas catchment.

Reviewer: Sect. 3.2.1: The simulation methodology needs to be described. How many were made? Response: We used 30 replicas/realisations of generated rainfall serious to compute the expectation of daily/ monthly rainfall totals. Please note that all the realisations are based on the same CP input time series (which is deterministic), within each generation each day has the same CP-specific rainfall probability and amount, as the CP does not change! We therefore think that 30 realisations give a reasonable estimate of the expectation of daily and especially monthly rainfall. Estimation variance of the mean is app 1/5 of the variance of the daily/monthly rainfall amounts. Nevertheless



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we will generate 100 realisations and compare the differences.

Reviewer: 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. Response: Address these differenced /biases, if they are significant in the revised manuscript. Please note again, that this Table cannot be cross validated, due to the short period of available data!

Reviewer: 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? Response: We agree that this has to be further investigated in the revised manuscript

Reviewer: Table 5: it should be clarified that these correlations are for the multi-year averaged seasonal cycle. Response: Will be clarified in the revised manuscript.

Reviewer: 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. Response: Will generate more realisations as recommended.

Reviewer: 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? Response: Fig. 9 also shows, that the observed series on seasonal monsoon total is appears to be quite different, also in the calibration period 1985 -94. Parts of these differences have to be reflected by the model of course. We will provide data on the interannual correlations between simulated rainfall totals at different stations in the revised manuscript.



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Reviewer: The interannual correlations are encouraging, but need to be better understood to be plausible. Sect. 4: Rather than speculate about SST influence, the paper would be better served by focusing on the meaning of the geopotential CPs. Reponse: We will focus stronger on the CP's, especially the interannual correlations and provide additional data such as CP frequencies for a better explanation in the revised manuscript.

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Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 1961, 2005.