Hydrol. Earth Syst. Sci. Discuss., 2, S954–S959, 2005
www.copernicus.org/EGU/hess/hessd/2/S954/
European Geosciences Union

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.

Received and published: 16 November 2005

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

Reviewer: 'Stochastical' may not be a proper English word - have not come across this before please check. Response: Will be corrected into stochastic

Reviewer: I am not sure what was the basis for the authors choosing the predictor variables they did. Given that the downscaling approach is most sensitive to the choice of predictors, some discussion on this would be helpful. The way the paper reads I was coming to an impression that they were using the same predictors they identified in a study in Germany to their study on the Monsoonal system in India - which would make sense only if they have tested this hypothesis thoroughly - hence clarifications would

be helpful here. 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. 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, 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 highly suitable to explain deviations from average conditions! Also Figure 2 supports this fact. 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.

Reviewer: Section 2.1.1 - is the standardisation done on a daily time step or using the full time series (irrespective of season)? If yes - then you are distorting the persistence in your predictor variables from one day to the next - which may pose problems in representing temporal dependence in the downscaled series. If no - then your classification of 'low' 'high' etc is seasonally insensitive. Response: The standardisation is done using the long term monthly average pressure and standard deviation at a grid point. The reviewer is of course correct, that a pressure anomaly of e.g. 0.3 in June and in December is not necessarily the same absolute pressure difference. Nevertheless, the classified circulation patterns are suitable to explain the deviation from the average

HESSD
2, S954–S959, 2005

rainfall conditions as underpinned by Fig. 2 and Table 2 in the old manuscript. Of course there might small changes in the rainfall conditions related to a certain CP, but the nature of the CP does not change: a wet CP in Summer does not become a dry on in Winter. Furthermore these seasonal changes are accounted for as the CP specific rainfall probability and amounts are estimated as function of the day in the year.

Reviewer: It would be nice if the authors could express their model that relates the atmospheric variables to rainfall through a system of equations which indicate clearly what needs to be optimised based on equations 1 and 2. The objective functions should really be coming after the model structure has been outlined. Response: In the revised manuscript we will start the methods section with a short outline of the stochastical model, that the reader can better understand the selection of the objective function.

Reviewer: Section 2.1.2 - I am afraid omitting the details of the conditional downscaling model totally is not helping. A small explanation is needed to give readers enough information to try and make sense of the results. I am assuming the structure should be along the lines of the rainfall being conditional to current CP, and the current CP being conditional to the CP on the previous day - the common CP on a given day enforcing spatial dependence in the daily rainfall, and the temporal persistence of the CP from one day to the next enforcing a semblance of temporal persistence in rainfall at each location. If this is true, the model will suffer from the use of discrete weather states (CPs), something that does not occur in reality. Consequently, the model will also suffer from a misrepresentation of the temporal dependence attributes of the resulting rainfall time series, a serious issue of results of the downscaling model are to be used for agricultural decision making and planning. Please include some minimal details on the model in your revision. Response: We will add a more detailed description of the stochastic rainfall model in the revised manuscript. The model accounts for space time continuity in rainfall through a) accounting for the autocorrelation of the rainfall time series, conditional to the CP and the day of the year and b) by accounting for the

HESSD

2, S954–S959, 2005

spatial covariance at a given day also conditional to the day of the year and the CP. The second source for temporal continuity is, of course, the persistence of the CP's which is of course usually longer than one day.

Reviewer: Is table 4 presenting results in cross-validation? If not, are these results really representative of what the model might do in the future? Response: Table 4 currently presents only results for the "calibration period. We will add results for the validation period.

Reviewer: The undersimulation of the number of wet days is interesting. The authors should try and hypothesise why that is happening. Is it because of a structural deficiency in the model or the nature of predictor variable chosen. I did not see any results on the spatial dependence of the generated rainfall. It might help to include a mention of how well this is represented. Response: We will provide correlations of observed rainfall at the stations at the monthly and daily scale. As already pointed out, the model rainfall model is a transformed multivariate normal. The description of a multivariate normal process includes essentially the inverse of the spatial covariance matrix. In our approach we use an averaged covariance, which will become clear in the new manuscript. We will provide evidence, that the model reproduces the spatial covariance. We will better discriminate the reason for the underestimation of the number of wet days e.g. the possible use of more harmonics to estimate the annual cycles of autocorrelation.

Reviewer: The authors should be careful in stating that this approach can be used for seasonal forecasting. Firstly, the downscaling results are not quite satisfactory (wet day fractions on a seasonal basis are significantly off). Secondly, reanalysis data often bears little semblance to GCM outputs, assuming that one has access to a GCM that is being run coupled to current conditions (using observed conditions to initialise the run) and this GCM is generating outputs that are on the same scale as the reanalysis data (same grid size, location). Response: We agree that the model has to be improved before used with GCM in the prediction mode. It is also true, that GCM's results bar

HESSD
2, S954–S959, 2005

little resemblance of the results of reanalysis data. Nevertheless they are the only tool we have to make projections of future climate. The argue whether downscaling of climate model output makes sense or not, seems to be as old as global climate modelling itself. Downscaling results are of course uncertain, but as long as these uncertainties are clearly named, we regard downscaling of GCM output as important means to assess local effects of climate change. By the way, in the climate of Europe the proposed methodology was successfully applied with ECHAM 4 models (Zehe and Bardossy, 2004).

Reviewer: Stehlik and Bardossy (2002) identified first three harmonics for describing the annual cycles of autocorrelation considering a specific area and record. This aspect needs to be investigated in the light of the data used in the study. Response: We will better discuss this point in the revised manusscript.

Reviewer: Page 5, I believe that NCAR reanalysis data is available on a grid size of 2.5 degree by 2.5 degree interval. Response: The older dataset, we used here is on 5 degree resolution.

Reviewer: Figure 2 is very hazy and difficult to understand and interpret. Response: Quality of Figure 2 and the explanation why the presented results strongly underpin our choice of the predictor variable will be included in the revised manuscript

Reviewer: I found it difficult to judge the model performance using figure 4. Authors may consider presenting the results in some other form that can help conveying the results in a better way. Response: Figure 4 will be changed to a different form in the revised manuscript

Reviewer: Results of correlation between daily rainfall among the stations may also be included in the Table 5. Similarly, results of simulation runs may also be included in the Table 1. Response: Results of correlation between daily rainfall among the stations will be included in the Table 5 of the revised manuscript

HESSD

2, S954–S959, 2005

Interactive Comment

Reviewer: The atmospheric data used in the study is mentioned in an ad-hoc manner. For example in page 9 it says 'geopotential height at 500 hPa level' whereas on page 13 it reads 'NCAR pressure data for 500 hPa level'. Response: For the new revised manuscript we will use the new data set. The selection will be better explained.

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