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Hydrology and Earth System Sciences Discussions

Interactive comment on "Analysis of projected hydrological behavior of catchments based on signature indices" *by* M. C. Casper et al.

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This is an interesting study that defines and uses smart signature indices based on the flood frequency curve and on event runoff coefficient to assess change impacts on catchment response behavior. The study is based on a sound data base, the manuscript is well structured manuscript - though some copy editing by a native speaker is still necessary. The presented findings are potentially of high interest for the audience of HESS. Unfortunately the authors miss many opportunities to work out potentially very interesting findings of the proposed study. I strongly encourage the authors to better work out the hydrological insights within major revisions. I hope the authors find the following major and minor points useful for this task:

Major points: - The author should better discuss the potential of the propose indices to assess quality of model predictions where model is defined as code, parameter set for a given catchment and meteorological input data. Even with observed rainfall LARSIM performs pure for instance with respect to peaks flows, and very poor during summer events (30 -40 % overestimation of runoff coefficients for catchments Kronweiler and Kellerbach). Model performance is not acceptable for Gensingen. These findings should be better stressed and discussed. Especially the pure performance in Gensingen needs to be discussed in as it indicates that soil moisture accounting in LARSIM might be ill suited for Loess soils.

- The author miss the opportunity to interpret and discuss the obvious shortcoming of the bias correction in Cosmo. Bias corrected rainfall data do not necessarily cause a better model performance. At least this can not be inferred from figure 6. The authors should always compare both bias corrected and uncorrected rainfall to simulations with the observed rainfall input and to the signature derived from observed discharge. This will show, which COSMO version performs better in comparison to observed rainfall input and how close/far COSMO driven simulations are from the real integral catchment behavior.

- The proposed bias correction is furthermore a non linear transformation and will destroy the spatial covariance and the temporal autocorrelation of the simulated rainfall field (which is only invariant under linear transformations). For me such a procedure is a quick fix to improve the model, good engineering but no science! It destroys the main advantage of dynamic models i.e. to create rainfall with an auto correlation structure and spatial correlation structure that is consistent with atmospheric physics. This seems to be not wise, and does not necessarily increase the hydrological value of the data. Assuming this bias correction scheme as stationary under change is to me a very unlikely assumption. I see that the authors cannot fully address all these issues within their study. They should however show that they are aware of these shortcomings.

- The authors need to address the problem how to judge when a change in one of their

signature indices is significant. What is noise and what is a real fingerprint of change.

Minor points - The future paper should clearly stress the scope of the study, which is for me clearly the use of these signatures to address for different purposes as outlined above.

- Line 25 what is meant with vertical distribution of soil moisture, infiltration and percolation as processes or the soil moisture profiles? The lateral pattern of soil moisture can be of equal importance.

- The catchments are not small but lower mesoscale catchments, please comment on apparent soils and provide data on specific runoff behavior (independent of catchment size).

- The model description is far too brief to grasp the underlying concepts. It should provide the main concepts and model structure.

- Bias correction: from a statistical point of view rainfall simulation is a mixed problem, the occurrence of a rainy day is a discrete event (yes or no), given the condition that rainfall occurs, the distribution of rainfall amounts per time step is a continuous. I do not see that this scheme accounts for this mixed nature of the problem. There is, by the way, a huge bunch of literature that suggests alternatives by means of statistical downscaling (compare to the dynamical downscaling of GCM output used) here for instance by Buerger or Bardossy. These alternatives should be clearly acknowledged.

- Is there any spatial structure in the bias between COSMO and observed precipitation time series? If yes the proposed invers distance scheme might be not the best way to interpolate the scheme to all grid points. I miss at least one statement that within the bias correction data at different spatial aggregation levels are compared (jump in scales).

- Please briefly provide details how the end point of the event is defined.

- Notation and indices in Eq. 10 should be explained.

- The Bias FHV is defined based on the difference in integral upper two percent. Please explain the choice.

Best regards,

Erwin Zehe

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3571, 2011.