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Hydrology and Earth System Sciences Discussions

Interactive comment on "Inverse distributed hydrological modelling of alpine catchments" *by* H. Kunstmann et al.

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Within there study the authors present an iterative automated calibration approach for a distributed hydrological model that is applied to 710 km2 large Ammer catchment in upper Bavaria. They employ the process based model WASIM, that has been coupled to a dynamic groundwater model, as well as the automatic parameter estimation routine PEST for estimating hydrological model parameters within several steps. First WASIM is calibrated in a decoupled mode. The recession constants of the surface reservoir, the interflow reservoir, the base flow reservoir and the drainage density were automatically estimated using initial values derived from a recession analysis. In the step second the groundwater model is switched on an the hydraulic conductivity values for the aquifers in each sub catchment were estimated using PEST. Interestingly the coupling to the groundwater model yields a decrease of overall model performance when compared

to those from step 1. Third the surface model parameters and the groundwater model parameters were allowed to vary simultaneously within the calibration, still for all except one catchment the model performance does not better compared to step 1. In a last step the authors calibrated additionally 4 parameter of a complex snow routine, which did again show no improvement.

The authors bring out nicely that even in catchments with complex interactions between surface water and groundwater bodies, coupled models do not necessarily yield better results. The authors propose furthermore a method for assessing confidence ellipses for pairs of model parameters based of a projection of the Hesse matrix which indicate uncertainty ranges as well as parameter interaction during the calibration process.

EVALUATION The presented study contains valuable information on automated calibration of coupled models when applied to Alpine catchments, which are in general not easy to model. The study is therefore of high interest for the audience of HESS. Unfortunately, the paper suffers from quite a number of short comings concerning the manuscript structure, the referencing, the language and the conclusions The authors should revise there manuscript addressing the general and detailed comments below.

GENERAL COMMENTS - The referencing is not appropriate. The authors should refer to other model studies that deal with automated calibration e.g. (Hundecha & Bárdossy, 2004 J. Hydrol.) or the shuffled complex evolution algorithm of Gupta and explain why these approaches are not appropriate for their model. (The few statements in section 9 are not sufficient)

- The structure is non standard, nine sections are too much. Some of the chapters would make up nice sub-headings in the normal structure.

- The paper needs prove reading by a native speaker, because a lot of statements are written in very “German” english

DETAILED COMMENTS - As already pointed out the introduction should contain a

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more thorough review of the recent literature

- A few statements on how the groundwater model and WASIM are dynamically coupled would be very helpful, especially how the lower boundary condition in the unsaturated zone is described in case of fluctuating water tables

- How did you estimate the van Genuchten Mualem parameters for the Richards Eq? What was the grid size of the model? Did you use simply point values, if so, do you think this is reasonable?

- I do not understand what is meant with " simulated discharge were replaced by observed discharge to avoid downstream error propagation". Did you calibrate the model seperately for each subcatchment or did you form an overal objective function?

- Eq. 2, please replace = by something like @ because it is an approximation

- In Eq. 3 do the eights in the matrix W sum up to one?

- Eq. 3 to form an objective function Eq. 3 should contain the observed discharge vector! Currently it just contains simulated discharge values for to different parameter sets.

- How does Pest sample the parameter interval?

- How did you separate fast from slow recession to estimate the different recession constants?

- Normally in such a study a threshold for an acceptable model performance is defined. This is not done here. Do the authors consider the model performance as acceptable? If so, please explain why?

- The statement on the projected Hesse matrix on page 13 is incomplete, I assume the authors mean that the two parameters are correlated if the principle axes of the ellipsoid are rotated with respect to the parameter axis

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Interactive Comment

- If this is so, the drainage density and the recession constant are correlated, as suggested by Figure 12

- The final statement of the authors is not convincing. WASIM in the decoupled mode yielded the best results. Why should someone use the coupled model which performs much worse?

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Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2581, 2005.