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

Anonymous Referee #2

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This paper demonstrates calibration of the distributed hydrological model WaSiM-ETH by usage of the nonlinear parameter estimation tool PEST. PEST is based on the Gauss-Marquardt-Levenberg method, a gradient-based nonlinear parameter estimation algorithm. WaSiM is a distributed hydrological model with a mix of physically based and conceptual process descriptions. The model is applied to the alpine/prealpine Ammer River catchment which is heterogeneous in terms of geology, pedology and land use and shows a complex orography. Results from four iterative calibration steps and one validation period are presented and confidence bounds for the calibrated parameters were obtained by covariance analysis.

Although the presented results raise interesting questions about model uncertainties, neither novel concepts or ideas were presented within this paper, nor substantial conclusions were reached. The work is based on the data and model configuration already presented by Kunstmann et al. (2004), who investigated the impact of climate change on the hydrology of the Ammer catchment. The paper solely contains a more detailed description of the calibration strategy of the former study. PEST is widely applied in many fields of geophysical sciences, as is also stated by the authors. As with other local search method, its solutions seem to show a strong dependency on the starting point of the search and it underlies the risk of being trapped in local regions of attractions (as recently shown by Blasone et al. (2005), who investigated the performance of SCE and PEST when applied to different distributed hydrological models, including MIKE-SHE). The paper fails to address relevant scientific questions within the scope of HESS and it does not provide novel concepts, ideas, tools or data. In its current form, I rank the paper as not appropriate for publishing (at least not in its current shape). The whole paper needs to be rewritten, it's scientific relevance would be significantly improved if the authors were to investigate, discuss and comment some of the following points:

1. Model selection criteria

There is not very much effort put into discussing the influence of groundwater movement on the hydrographs of the eight gauges, which in fact may be significant within all zones of the catchment (alpine limestone, bordering flysch and the folded und unfolded molasses). Considering that an integrated modelling approach could be more appropriate i.e. coupling WaSiM with a more sophisticated 3D groundwater model like MODFLOW (i.g. Krause & Bronstert, 2004). Furthermore, the choise of WaSiM has not been properly discussed and justified.

2. Uncertainty in process descriptions

It has been stated by the authors that the application of WaSiMs own 2D groundwater model resulted in a decline of the model efficiency (Nash-Sutcliffe) compared to application of the conceptual groundwater model. I think this is a very important result and is well within the scope of the PUB initiative which suggests to carry out model inter-comparisons in selected gauged basins to investigate the uncertainties due to choice of model structure. Unfortunately there is no further discussion of this topic. In this context a comparison of results obtained using WaSiM with TOPMODEL approach would also be of interest and benefit the scientific value of the paper.

3. General quality of model results

The model results are poor (efficiency of 0.34 for the entire basin after calibration). There is no discussion about this nor are the results compared to other applications of WaSiM. It seems that more processes and parameters need calibration to ensure a properly reproduction of observed discharge with WaSiM. The figures 7-10 are showing only the "best" fitted hydrographs, but it is obvious that the model tends to fail within most regions of the hydrograph and not only for the highest flow peaks (e.g. some average flow peaks in summer are completely missed and vice versa).

More Specific Comments:

Page 2583, Lines 8-9: "Even in physically based hydrological models... Lumped parameters (such as recession constants) must even be calibrated." Physically based is in fact a very distinct definition. However, within the abstract WaSiM is described as "a fully distributed hydrological model using physically based algorithms for most of the process descriptions." Thus talking about calibrating lumped parameters is not acceptable. A clear classification for WaSiM is required.

Page 2600, First paragraph: I don't understand the conclusion that parameter uncertainties obtained by the use of a local search method are a prerequisite for Monte Carlo Simulations. Even the definition of Monte Carlo Simulations as "stochastic hydrological modelling" is not clear within this context. Does it mean stochastic uncertainty estimation or stochastic global search? Considering the possibility for a local search algorithm to be trapped in a local optimum makes it meaningless for using its results as a prerequisite for a global search. However, the authors suggest this procedure.

Fig. 1: The location of the subbasins is not helpful in this figure. Insertion of the divides in Fig. 4: (location of the gauges) would be more appropriate and improve clarity. Fig. 7-10: Rescale the axes to avoid intersection of precipitation and discharge lines. Something is wrong with the timescale in Fig. 8 (No Feb, Dec and Mar twice).

References:

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