Hydrol. Earth Syst. Sci. Discuss., 2, S1499–S1505, 2005
www.copernicus.org/EGU/hess/hessd/2/S1499/
European Geosciences Union


Hydrology and Earth System Sciences Discussions



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

**H. Kunstmann et al.**

Received and published: 14 March 2006

Dear editor, dear anonymous reviewers,

Thank you very much for the evaluation of our article and particularly for the helpful comments to improve the quality of the manuscript. Please find below our response to the three interactive comments and attached the revised manuscript. In cases where we disagreed with the referees' comments we indicated the reasons. We would be glad if the revised version of the manuscript fulfils the quality criteria of HESS and we are looking forward to the final decision. Thank you very much.

Yours sincerely,

Harald Kunstmann

## Response to editor

*General comments:*

- “Reference to other model studies on automated calibration”: In section 1 other studies and approaches are briefly discussed now.

- “Structure of article”: The structure is changed, subheadings are used.

- “English improvement”: The article was already once checked by a native speaker. We are sorry for all remaining grammatical and linguistical shortcomings.

*Detailed comments:*

- “extended review of recent literature”: the review is now extended (see section 1)

- “dynamical coupling of groundwater model in WASIM”: The description is extended now. Anyway, for further details (formulae, FD schemes, etc.) we refer to Schulla and Jasper (2001) because the technical details of the coupling strategy between groundwater and unsaturated zone is quite a comprehensive task which cannot be satisfactorily presented in a brief manner.

- “estimation of van Genuchten parameters”: The van Genuchten parameters where taken from tabular literature values. A corresponding remark is added in “Model Description and Setup” of section 2. We point at the fundamental problems of upscaling issues, which are, however, not the focus and the scope of this study.

- “grid size of model”: The grid size of the model is 100x100 $m^2$. This is indicated now in “Model Description and Setup” of section 2. Thanks for this notice. We had only mentioned that the river courses were derived from a 100x100 $m^2$ DEM, but not the model resolution itself.

- “simulated discharge Ě”: The subcatchment-specific parameters are calibrated separately for each subcatchment. In case of parameters that are not subcatchment specific (like parameters of snow model or groundwater model) an overall objective function



HESSD
2, S1499–S1505, 2005

(sum of all individual NS) is used. This is describe din section 4 (page 12)

- comment on Eq. 2: the " =" is substituted by "$\approx$" now.

- comment on weights in Eq. 3: the weights do not necessarily sum up to one (according to Press et al., 1992; Doherty, 2002). We have added a corresponding remark.

- comment on observed discharge vector: we reformulated the text and the formulae according to the suggestion.

- "How does PEST sample the parameter interval": The sampling is along the gradient, starting with an initial value. The details are given in section 3 (review of Gauss-Marquard-Levenberg). Limits/boundaries of the parameter space can be accounted for.

- "separate fast from slow recession": the solution/strategy to this important fact is mentioned on page 10, where the calibration strategy is explained: "The recession constant for interflow is restricted to be larger than the recession constant for direct ("quick") runoff."

- "threshold for acceptable parameter performance": No threshold criteria for the model performance (e.g. for NS values) was chosen. Instead, the following threshold strategy was applied: within every general iteration step 1-4, the internal Marquard-Levenberg iteration stops, when change in between two successive iterations is smaller than a threshold value. This is mentioned on page 8, end of chapter 3.

- "HESSE matrix": thank you for the nice formulation, I adapted the corresponding sentence accordingly and hope it is much clearer now. The slight correlation in Figure 12 is indicated (page 15).

- "final statement": the fact that the complex 2-dim numerical groundwater model does not necessarily improve the quality of the model results is more elaborated (see abstract and conclusions).

**Response to referee 1**

- "local search and global search automatic parameter estimation algorithm": In section 1 the review on further automatic parameter estimation methods is extended. Moreover, the disadvantage of local search algorithms is discussed (dependency of initial values).

- "groundwater depths additional to observed discharge": Due to the findings of this study, we recently started to investigate automatic parameter estimation for the Ammer by joint consideration of observed river discharge and groundwater tables. The investigation, however, is hampered by the fact that only little hydrogeological information and only a few groundwater table elevations are available. Boreholes are unevenly distributed (mainly in the low land parts).

- covariance analysis: the covariance analysis as realised in PEST calculates the correlation among all parameters at a time in a "dry run" in which more or less all sensitivities along the final parameter set are re-calculated. It is only for visualisation of confidence bounds that the projection of the n-dimensional hyperellipsoid in a 2 dimensional subspace is performed, reducing the hyper-ellipsoid to ellipses.

- "conclusions": the fact of decreased model performance using the complex groundwater model is elaborated in more detail now.

- "missing references": the missing references are included now

**Response to referee 2**

- " ...neither novel concepts or ideas ...", "PEST is widely applied in many fields of geophysical sciences", " Blasone et al., 2005 ": PEST was mainly applied in hydrogeology and used for calibrating groundwater models. But it was up-to-now and to our knowledge not yet applied to a distributed hydrology model WaSiM or similar complex surface hydrology models. The referee mentioned Blasone et al. (2005). This reference is a conference abstract, but not a published paper. We therefore cannot check the contents of the reference. It shows, however, that coupling PEST to more complex hydrological models is under recent investigation by other scientists, too, and is indeed a novel approach and idea (contrary to the referee's assessment). As suggested by the referee and the editor, we introduced/extended the literature review on parameter estimation algorithms and discuss in more detail the problem of local search vs. global search algorithms.

- "1. Model selection criteria": Due to the fact that there is only little groundwater information available, but in no case detailed 3-dim. hydrogeological information, we do not see a justification for a 3-dim groundwater model. Application of a 3-dim groundwater model like MODFLOW would need detailed information on the vertical structure of the unsaturated and saturated zone, the location of impervious layers and subsurface boundary conditions and 3-dim distribution of physical values for hydraulic conductivities, porosities, leakage coefficients, etc.. This information, however, is not available in the required detail for the Ammer catchment. We added a justification for the choice of the model on page 4.

- "2. Uncertainty in process descriptions". Focus of this article is the discussion of potentials and limits of a parameter estimation technique for CPU intensive distributed hydrological models. It is beyond the scope of this work to present a model intercomparison (WASIM version 1, i.e. TOPMODEL approach, vs. WASIM version 2, i.e. Richards approach) as suggested by the reviewer. It should be mentioned that the TOPMODEL approach in WASIM is quite old and it is even not continued any-



HESSD

2, S1499–S1505, 2005

more in the WASIM model development since 1998. We therefore disagree with the reviewer and do not see reasons to additionally apply WASIM version 1 and compare it to WASIM version 2.

- "3. General quality of model results": 1) NS=0.34 is the value for subcatchment 8, not for the entire basin. 2) Quality of results: in chapter 5 (new labelling) on page 16, second paragraph, a detailed discussion on the quality of results (in particular comparatively low NS values) was given. It remains one of the messages of this article that decreased model performance may have to be accepted when increased model complexity is desired or required. The reason that summer peaks are sometimes missed is drawn back to the complex orography and the location of the precipitation stations (all outside the catchment). This shortcoming cannot (and should not) be compensated by parameter estimation.

More specific comments - "page 2583, lines 8-9": The expression "fully" in "fully distributed model using ..." refers to "distributed" not to "physically based". In the model description it is in detail discussed which process descriptions are based on physical assumptions and which process descriptions are based on parameterizations. A clear and transparent classification and descriptions of WASIM therefore is given. It is clearly indicated that the "lumped parameters" refer to the non-physical parameterizations.

- "page 2600, prerequisite for Monte Carlo": unconditional Monte Carlo analysis requires the uncertainty range of input parameters (i.e. standard deviation). Based on this standard deviation (and an assumed probability density function) parameters are generated and for every realization of parameters the model is evaluated, finally leading to an ensemble of results which in turn is analysed statistically. The required standard deviations can directly be obtained by the confidence ellipses. This approach is fully independent of "local" or "global" search methods.

- "Fig. 1, location of subbasins": Fig 1 aims at showing the geographical location of the catchment within Europe. Including the gauges would be too much detail in this figure.

The location of the gauges is given in Fig. 4.

- "Fig. 7-10, Rescaling of axes". Thank you very much for the suggestion. We changed the figures accordingly and corrected the labelling error.

---

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



HESSD

2, S1499–S1505, 2005

Interactive Comment