Hydrol. Earth Syst. Sci. Discuss., 4, S505–S508, 2007
www.hydrol-earth-syst-sci-discuss.net/4/S505/2007/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparison of different multi-objective calibration criteria of a conceptual rainfall-runoff model of flood events" *by* N. Chahinian and R. Moussa

Anonymous Referee #3

Received and published: 10 July 2007

Overview of Contribution: The authors present an analysis of six calibration criteria classified as being either "global" or "relative" in terms of how they account of errors per event analyzed. Global criteria average over all events and will be heavily influenced by statistical outliers whereas as defined by the authors relative criteria seek equal event weighting. The purpose of this paper is to analyze the value of mono, dual, and tri-objective formulations for event-based calibration. The authors present calibration formulations increasing in the number and type of component objectives. The intent of the paper is to clarify how expertise should be used to inform and improve calibration formulations. The authors claim that a carefully selected 2-objective formulation for the presented case is equivalent or superior to mono- or tri-objective formulations.

General Comments:

The authors' presentation is interesting and does build on prior multi-metric calibration studies. The results of the study could be better organized and the analysis of the individual formulations ranges from clearly described/plotted (see Fig. 3 and text on page 1046), to poorly described (the text supporting Figs. 4 & 5 is insufficient), to inadequately presented/described (3-objective analysis on p. 1048 and Fig 6). Building on the comments of Referee #1, the following additional issues should be addressed in a revision of this manuscript:

1. The authors assume that the weighted objective aggregation technique of Madsen (2000) provides a sufficient representation of the Pareto optimal set independent of the number and formulation of component objectives used in the study. Three or more objectives could yield very complex geometries for the Pareto front. Additionally, the goal of the weighting scheme is to make the summation of objectives scaling invariant (i.e., a single large objective range does not control the overall direction of search). The authors need to clarify the details of how they implemented the aggregation approach.

The weighting scheme requires users to specify their preferences before understanding the structure or content of their tradeoffs (especially when using 3 objectives). Alternatively, a posteriori decision making assumes that the structure and content of the tradeoff surface should be used to inform parameter set selection. For many hydrologic models, nonlinearities and thresholds could likely yield non-convex Pareto fronts. How do non-convexities (see Fig 5a & 5c) in the tradeoff surface affect the applicability of the weighting scheme? Are there any limits to the generality of your analysis and conclusions for higher numbers of objectives? This issue has a long history and should receive a more detailed discussion in the text.

See Chankong, V. and Y. Haimes, Multiobjective Decision Making: Theory and Methodology. North-Holland Series in System Science and Engineering. 1983, New York, NY: North-Holland.

HESSD

4, S505–S508, 2007

2. In Section 5 on page 1044 lines 21-23: The a priori ranges for the model parameters are quite important and the authors do not provide a sufficiently detailed justification.

3. Given the a priori ranges, how do resolution changes in the response surface impact your conclusions? Initial course trials may in fact not reflect where subsequent areas of high resolution focus should be located (due to multi-modality and deceptive fronts). The authors need to provide more detail on this aspect of the study.

4. p. 1048, Section 5.3: Why not show a volumetric view of your approximations to the Pareto surfaces? It would be informative to know where the aggregated point falls. If the authors do not want to generate 3-D plots, they could simply show the higher dimensional Pareto optimal sets in each of the 2-objective planes that define the 3-objective spaces. This would also provide a better understanding of objective conflicts. In general, the authors' conclusions on the value of the 3-objective formulations are not well supported. The 3-objective analysis is not as detailed as the 2-objective cases. It is not clear where the aggregated 3-objective solution falls or in general the degree of conflict between the objectives. Mathematically, the Pareto surface will "collapse dimension" in sub-spaces if conflicts do not exist between the subsets of the analyzed objectives (see Fig 3c for an example of dimensional collapse).

The authors are actually interested in a 6-objective space. Theoretically, a single analysis using all of the objectives would implicitly yield all of the sub-spaces plotted in Figs 3-6 (as well as several others not shown). The full objective space formulation could provide a more general framework for drawing conclusions on the objectives' interactions and performance. The presentation could be shifted to explore 3-objective and 2-objective subsets drawn from the approximation to the full 6-objective Pareto surface (moving from higher to lower dimensional analysis).

Minor Comments:

-Contractions should not be used in the paper.

HESSD

4, S505–S508, 2007

-The paper has some grammar and syntax errors that need to be corrected.

-lines 12-14 on p. 1038: awkward sentence that should be re-worded

-the text in Section 3.2 defining Pareto optimality is awkward and could be improved. Also the authors need to clarify that they are approximating the Pareto front in the subsequent results.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 1031, 2007.