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Hydrology and Earth System Sciences Discussions

Interactive comment on "Increasing parameter certainty and data utility through multi-objective calibration of a spatially distributed temperature and solute model" *by* C. Bandaragoda and B. T. Neilson

Anonymous Referee #1

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GENERAL COMMENTS: The manuscript discusses the issue of parameter calibration of stream transport of solute/heat, and focuses on the use of a multiobjective optimization method for reducing parameter uncertainty and reproducing multiple series of observed temperature and concentration at different stream sections. The topic is interesting as current models for stream transport rely on a high number of parameters to describe exchange of mass and heat between the stream, surface storage zones, and hyporheic sediments. However, there are some technical issues that need to be

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Discussion Paper

described in more detail in order to make the manuscript clearer and to clarify the relevance of its results.

SPECIFIC COMMENTS:

- The paper largely follows the methods described by previous works (Neilson et al. 2010, which are correctly cited), and in particular adopts the same mathematical model for heat/solute transport and an improved version of the calibration procedure. The novel aspects introduced by this work is the use of a narrowed parameter space to improve the performance of the optimization procedure and the parameter uncertainty. In my opinion, the authors should make more explicite that this is the novelty of the work, e.g. in the abstract and in the introduction. More importantly, they should also provide more details on how the optimization algorithm works, since this aspect is intimately related to the reason for the remarkable reduction of parameter uncertainty when narrower parameter bounds are used. Apparently, the algorithm is converging to a local minimum at level 1-2, and providing a better first guess of the parameter values enhances the convergence toward the global minimum. However, it is difficult to understand it from the brief description presented in section 3.2 (first paragraph). While a complete description of the optimization algorithm would be excessive, some additional explanations would be helpful, in particular since the improved calibration technique is the novel contribution of the manuscript.

- The model equations are not presented in the paper, which makes difficult to understand the number of parameters involved in the modelling as well as the precise meaning of the calibrated ones. They should be included in the methods section, or at least in an appendix.

- The significance of the calibration depends on the interplay between the timescales of in-stream transport and transient storage. I suggest to evaluate values of DaI to verify the amount of information about storage processes carried by in-stream breakthrough curves.

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- The reason for adopting a priori values (see p.8317, l.6-7) for roughness, n, and channel width, Btot, instead of calibrating them is unclear. The authors do not report the values, and only give a reference to a manuscript (Bingham et al - also cited later in this work) which is still under review. This reference cannot be used unless it has been accepted. Clearly, the larger the number of parameter to calibrate the more delicate is the calibration. The uncertainty of estimated parameter values also increases with parameters, and given that in their previous work (Neilson et al, 2010b) the authors considered 15 parameters they could do the same here. Otherwise, they should explain the reason for keeping n and Btot constant. Moreover, the temperature of deep sediments layers below 'active' hyporheic exchange zone (T_gr) appears in the model equations. Please specify how this value has been estimated.

- In the discussion section, it would be interesting to report the ranges of calibrated values (which are now only given in scaled form in fig.10) and comment them in the context of stream transport processes (e.g. which are the dominant exchange processes in the study reach?).

MINOR COMMENTS / TECHNICAL CORRECTIONS:

p.8311, l.21: Shouldn't be 'global optimum' instead of 'global optima' here? Please check also in the following lines

p.8312, l.21: 'distributed laterally ... and longitudinally...' This sentence is unclear. Please explain better.

p.8314, l.9: why is the gw exchange important?

p.8314, l.12-13: 'calculate atmospheric fluxes': please explain which flux is referred to here, and how it has been determined.

p.8316, l.1-10: this is a point that could be expanded to better clarify how the optimization algorith work. For instance, the meaning of 'parameter sets ... evolved using two complexes' is not clear.

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p.8316, l.25: the choice of the a priori parameter space seems very important for the convergence of the optimization. Please explain how this choice was made in the cited work.

p.8316, l.5: why a single value is assumed for Y_gr?

p.8319, l.9: the definition of the narrower bound is a central point. Please clarify better how this operation is performed.

p.8321, l.14: AE_s is actually equal (not greater than) 0.81

p.8322, l.8: which are the 'pareto rank one sets'? In general, the manuscript lacks a figure showing the pareto curve and the 'best' result of the multiobjective optimization, which would be helpful for the reader. The authors could consider to include it or to provide a reference where such a figure is presented.

p.8324, l.7-10: the fact that using two observation series instead of one improves the calibration performance is not unexpected. It is clear that adding more information (assuming they are not flawed by errors) reduces the uncertainty of calibrated values.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8309, 2010.