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Hydrology and Earth System Sciences Discussions

Interactive comment on "Quantifying spatial and temporal discharge dynamics of an event in a first order stream, using Distributed Temperature Sensing" *by* M. C. Westhoff et al.

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We would like to thank the reviewer for his/her useful comments. From the comments we understand that some important parts of the paper were not explained well enough. In the revised manuscript we shall explain these issues better.

The main issues raised are answered below with the reviewers comments in italic.

The manuscript deals with the calibration of a numerical model with observations as

a learning tool to understand the dynamics of a small stream flow. In particular, the Authors try to reconstruct the spatial and temporal variation of the discharge of the stream by comparing the model results with high resolution temperature measurements.
The narrative description of the model calibration is interesting and allows the reader to explore the steps towards the definition of the set of parameters that best fit the observations and to test critically the assumption and rejection of different hypotheses. On the other hand, with so many parameters to be calibrated and such a large number of degrees of freedom in the search, doubts may arise about the uniqueness of the solution. The Authors partially tackle this problem in section 5, but the analysis is not always clear. Another shortcoming of the work is the lack of clarity in the description of the model.
A valid question is raised about the uniqueness of the model results. However, we do not claim that this is the true solution. The model is used as a learning tool with which we were able to formulate and test hypotheses. We showed that some hypotheses can be rejected, while others are more or less likely, but cannot be rejected with the currently available information. Regarding the description of the model, we shall expand the method section to fully explain the structure of the model (see below).

As a whole, the manuscript may be of interest of the readers of HESS, but some improvements are needed.

1) The description of the model in section 3.1 is not sufficient to understand its correctness. The Authors write: "This study builds on previous work by Westhoff et al. (2007, 2010, 2011). In this section we only give a short description of this work. For further details, the reader is referred to the original studies." However, the model for the hyporheic zone is described in Westhoff et al. (WRR 2011) that is only submitted and therefore is not available at the moment of this review.

In the revised manuscript we will expand the method section in which we will describe the methods in more detail (as described in Westhoff et al., 2011).

2) Equations 1-3 do not constitute the complete set of governing equations: there are several unknowns (at least Aw, Q, Ab, Tw, Thz, Ts) and only three equations. Let's see what is missing: 1. a geometrical relationship can be probably found to infer Ab from Aw; 2. the discharge Q (which is a "spatial and temporal varying discharge", p. 2181, l. 9) can be determined by means of the usual momentum equation; 3. an equation for the hyporheic temperature Thz is needed (or is it an imposed boundary condition?). Moreover, equation (3) is questionable from a formal point of view: it has the form of a second order differential equation describing the diffusion of temperature in the subsurface zone, but the boundary condition (Phi_bed) is included in the equation, with a specification in the text (p. 2183, l. 8-9) that the term should be considered only in the top layer (presumably, but it is not said, dz is the thickness of the top layer). Although the computational result can be the same, the equation could be written in a more precise form.

The reviewer is correct that there are too many unknown when using only these 3 equations and he/she also gives the correct answer that there is a geometric relationship between A_b and A_w and that Q (and A_w) is determined with the momentum equation. This has been shortly stated in the original manuscript (P2181 L7-10), but we'll make this more clear in the revised MS.

T_{hz} equals T_s, but only at the grid cells were hyporheic exchange is determined. We will make this also clearer in the revised MS.

We do not agree that the third term in Eq (3) should be removed. The third term is a sink source term, which, in principle, could be added to any place along the vertical. We changed dz into ∂z to write it in a more precise form.

3) The model (eqs. 1-3 and following lines) uses a lot of variables with different

subscripts representing quantities in different parts of the cross-section. It is not easy to understand where the variables are defined and how they interact. To improve clarity, the Authors should include a conceptual illustration of the different regions of the cross-section indicating variables and fluxes.

We will add a conceptual figure.

4) It is not always easy to follow the changes discussed in the sections 3.2 and 4 concerning the set of parameters used in the model. A table summarizing the values of all the main parameters and their changes during calibration is needed.

This is a good suggestion. We will add this to the revised MS.

SPECIFIC REMARKS

- p. 2184, l. 4, "losses of water": indicate which is the corresponding parameter in the model.

The corresponding parameter is q_L. This will be added in the revised version

- p. 2184, l. 22, "Qhyp" and "Vhz": why are they used instead of the parameters per unit length (alpha Aw, Ahz) that are presented in the model in section 3.1? Formally, they depend on the spatial integration step (if this is the meaning of "dx", which is not defined) so their value will change with the discretization.

This is a good point. We will change this into $q_{hyp}(=\alpha A_w)$ and A_{hz} to indicate that these are per unit stream length.

- p. 2190, l. 6, "when the infiltration loss . . . is taken constant over time, the peak in downstream discharge occurs 50 min too late. Therefore we can conclude with high certainty that this loss increases with increasing discharge": this statement is not obvious. It is not clear how a delay of 50 minutes can depend on the infiltration loss. If

the delay is due to the infiltration of water and its release after some time, much more diffusion in the discharge peak is expected (i.e. a much wider peak).

We meant to say that when the observed upstream discharge peak is routed down while keeping the stream losses constant, the simulated downstream peak arrives 50 min later than the observed. We therefore conclude that the observed downstream peak is not the same water as the observed upstream peak, since a wave celerity of 0.35 m/s would be needed to have a correct timing of the simulated downstream peak, while we simulated a mean celerity of 0.12 m/s.

When we conclude that the upstream and downstream discharge peaks are not from the same water, then the observed upstream peak should be lost somewhere and we conclude that it is likely lost between 60 and 77 m. "Rain on water" would then be responsible for the observed downstream discharge peak.

- fig. 2, caption, "the noise in upstream discharge observations was removed to decrease calculation time": filtering the noise may be reasonable, but why does it increase calculation time?

Discharge fluctuations increase calculation time since there is iteration in the routing model between water depth and cross-sectional area. But the reviewer is right that this comment is irrelevant here, and it will be removed in the revised manuscript.

- fig. 2 and fig. 5 are almost identical, so they can be joined in one single figure.

The reason to split these figures was that otherwise the reader would be confronted in an early stage with model results that are discussed much later. In the revised MS we will check if it is worthwhile to combine the figures into 1.

All typos will be corrected in the revised MS.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2175, 2011.