Hydrol. Earth Syst. Sci. Discuss., 6, C3010–C3015, 2010
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing the added value of high-resolution isotope tracer data in rainfall-runoff modelling" *by* C. Birkel et al.

Anonymous Referee #2

Received and published: 3 January 2010

First, I would like to excuse the late review. The manuscript presents a modeling study assessing the value of isotope data at high temporal resolution to assess model performance and parameter sensitivity. In addition, the study also tries to assess the value of multi-calibration strategies and incorporation of uncertainty into the model calibration approach. In general, I like the presented ideas and approach, however, the paper is poorly written and many important aspects of the paper are either incorrect (CIM model) or the methods are not clear. In addition, many of the concluding remarks cannot be derived from the presented data and analysis and the authors need to be more careful in their statements. The current version of the paper should not be published in HESS and I suggest very major revisions with resubmission into HESSD.

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Interactive Discussion

Discussion Paper

General comments: 1) The title is too general and promises more than the paper finally delivers

2) What is a low-parameterized model? Are 7 parameters low? I would suggest to use "lumped conceptual model" or some description that has been established in comparing hydrological models

3) The introduction is very self-focused. Mostly self-citations of the authors are presented. It looks like that only the 3 senior authors of this manuscript have worked in the general area of catchment modeling, isotope hydrology and parameter uncertainty and sensitivity. In particular the initial papers of many ideas and approaches have not been cited accordingly.

4) In my opinion, objective 1 is not an objective – just the data that was collected. Objective 2 is too strong. These kind of models have already been developed – CIM is just a slightly different way in combining mobile and immobile storage boxes.

5) What is the time step of observations used in the model? Daily, 15 Minutes?

6) The decision to use Deuterium (D) instead of 18O is a bit arbitrary. Sure, there is an interdependence between the two, but the change in this also includes many processes that are important (fractionation, etc.)

7) The authors try to argue that discharge measurement and stable isotope sampling are the largest sources of uncertainty in the modeling process. I cannot generally agree with this. The error of precipitation measurement can be very high, in particular wind induced error (up to 50%) – was this corrected for and if, how was this corrected? I assume wind is very strong in Scotland during rainfall events. In any case, it needs to be shown by comparing the different sources of measurement uncertainty, which are dominating in this catchment and environment since this is one of the major objectives of the paper. A simple reference to another study cannot be sufficient in this respect.

8) The CIM model needs to be better described to completely understand the differ-

ences of the 5,6 and 7 parameter model. Also Figure 3 does not help much to understand the concept of the model and the flow of water and tracer mass. In my opinion, there are also several flaws in the model which I try to highlight:

a. The model does not balance tracer mass. Since continuity of mass should be the first principle in hydrology, I believe each hydrological model should balance water and racer mass. Since D input is defined by D concentration in precipitation (Eq 12 is not defining tracer input but only change in mass in the storage) and D is not be removed by ET (which is physically the case since water is being lost and hence water isotopes as well – see recent paper by Botter et al, 2009, WRR), the tracer mass balance is incorrect.

b. Similar issue with tracer and water loss can be pointed out for the GWloss. This is only defined for water, but not for tracer. Therefore, water is being removed from the model without removing the appropriate tracer mass according to the concentration in the storage box.

c. CQ is a product of concentration and flow, hence it is a mass flux, not a concentration (Eq 14,15). There seems to be a major confusion between tracer concentration C and tracer amount CS and tracer mass flux (CQ). The units are incorrect and the description. This needs to be corrected! Since the description of the tracer part of the model has so many flaws – I am not even sure if the model is correctly coded and mathematically correct.

d. The upper storage can become negative! What happens with the tracer mass in this storage if it is negative – does it also become negative?

e. The parameter names in the text, table 2 and Figure 3 do often not match – this makes it really difficult to understand the model.

f. The solute model is not fully dependent on the water flux model since 2 additional parameters were included.

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g. ET depends on precipitation in the model at each time step. How can this be physically justified?

h. The parameter c has nothing to do with preferential flow, it is just a direct runoff component.

i. What happnes with the tracer if parameter c is included – this has not been described.

j. How are the initial conditions of the model being determined? I assume it is very important to the model if only 1 year is being simulated of setting the state variables for water and tracer.

9) It is argued in 4.2.2 that D is independent of Q. Is this true? What is the correlation coefficient between D and Q?

10) The argumentation in 5.2 is a bit difficult to follow. The presented statistics of the data does not really convince me that the sampling resolution makes a big difference. In addition, the statistic is only based on concentration without weighting to flow or rainfall amount. I think it would be important to include the weighted statistics (or a mass flux statistics) since this may really show a difference.

11) The estimation of uncertainty is mostly based on assumption, but not on actual measurements and error analysis. Since the discharge (flow velocity) measurements (how many have been done?) and rating curve data are available, they should be used to estimate the error for this particular site. In addition, discharge error is rarely a simple relative error since it varies between high and low flows. On the other side, stage recording is mostly an absolute error. The different error sources need to be better defined, calculated and combined since this is a central objective of this paper. If relative and absolute error terms are combined, Eq 2 needs to be extended!

12) The calculated of uncertainty for isotope sampling is unclear. I assume the analytical error is an absolute error, is the sampling method and preservation error relative or absolute? This is not clear since units are missing. Is the combined error relative? This

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is really strange for stable isotope data (as also seen in Figure 5) since the analytical error is an absolute error and the other error sources should be absolute as well. This needs to be clarified and corrected.

13) Section 5.3.2. and 5.3.3. are not clear since the whole multi-calibration procedure is not well explained. I cannot evaluate these sections since I do not completely understand how the procedure was applied and what can be seen in the different figures. Thresholds for the sensitivity analysis in 5.3.3. are suddenly set much higher than best results in 5.3.2. Why are these results better?

14) The paper never clearly shows if the model uncertainty or parameter sensititivity is relay increased and reduced, respectively, by including tracer data. I believe more MC runs are necessary to defined the parameter uncertainty for including the tracer data since the number of behavioral sets are so much reduced that the distribution of in the parameter set cannot be determined appropriately. I would suggest to do some formal sensitivity analysis methods (SOBOL) do better define parameter sensitivity and not just showing some dotty plots. In addition, a formal method to present, quantify and compare the model uncertainty needs to be included in this paper. Otherwise, the conclusions are not supported by the results and the chosen method.

15) I also miss that the input uncertainty of precipitation and in particular isotope data in precipitation was not considered. I believe the input uncertainty could also have a substantial impact on the model uncertainty.

16) In 6.1. it is mentioned that CIM was also calibrated to weekly data and the model performs better but the data are not presented. I would have expected this kind of comparison: Calibration to daily data, weekly data, global parameter sensitivity analysis, comparison of parameter ranges and model uncertainty from the proposed title and objectives. But the model is only being calibrated to the same data set. I cannot see how the value of high resolution isotope data can be shown with this approach.

Specific Comments:

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1) Grammar and spelling needs to be improved

2) Are there three of four objectives (19/6210)

3) Please mention the method that was used to calculated ET (not only a reference) and show the location of the meteorological site on the map in figure 1. In addition, it should be clarified if this was potential or actual ET?

4) The last paragraph in section 3.2 is not clear.

5) Figure 2 appears after Figure 3 in the text.

6) Table 3 is difficult to read and should also be combined with Table 4 since similar information are being presented.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6207, 2009.

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