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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 #1

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This manuscript assesses the value of isotope tracer information in rainfall-runoff modeling. The authors collected valuable tracer data both in precipitation and stream flow with high temporal resolutions (daily) than many other studies (weekly or bi-weekly). Based on numerical experiments incorporating uncertainty assessment, the authors concluded the necessity and the importance of daily isotope precipitation for simulating reasonable temporal dynamics of "stream flow isotope". The results can be shown only with the precious dataset. However, it is somewhat confusing if the authors are trying to show 1) the isotope can improve "rainfall-runoff simulation" or 2) the isotope can improve "tracer simulation". According to the title and introduction, readers may think the objective is 1). If so, the following questions have to be clearly answered to come to this conclusion.

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1. Can the reduction of uncertainty bands of simulated stream flow be the good evidence of the isotope information? The uncertainty bands may be varied if the subjective evaluation criteria (P6220 5.33) change. For example, if subjectively decided D_VE>0.975 is changed to D_VE>0.8, the uncertainty bands maybe be wider. Likewise, if the authors choose higher criteria for runoff simulations, the simulated uncertainty bands can be also reduced.

2. How did the tracer information help to decide the model structures? The experiment result that the 7 parameter model with unmixed portion and GW leakage was chosen as the best performance model may be due to the model flexibility compared to 5 or 6 parameter models. In addition, how can the tracer information actually help for understanding the hydrologic processes?

These questions have to be answered to conclude 1). If the authors intend to conclude only 2), the introduction (and maybe the title) should be addressed for that. It is also important to convince readers why simulating such a high temporal variations of tracer is necessary.

In addition to the above main review comments, the followings are the minor comments that should be answered or addressed.

1. P.6215 L1: The unit of dN/dt (flux/d) is not reasonable because the unit of "flux" is already per unit time per unit area.

2. P.6216 L9: It should explain “stepwise, multi-criteria calibration” briefly, even if the method is published in a cited paper. In the current manuscript, it is not clear how the simulated uncertainty band and the best parameter sets are obtained in 5.3.2.

3. P.6222 L11: What is the implication of the results stated in the following sentence? "CIM model calibrated against weekly data performs slightly better (10%) than applied to daily data". Explain first how the performance is evaluated (what is the meaning of 10%) as well as what performance is evaluated (stream flow or tracer). Then, please

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explain how this result relates to the main objective of this paper. It is confusing also why the coarser temporal resolution data were better than the finer resolution data.

4. The model structure is difficult to understand because of the unclear explanations, especially about the following points.

- Figure 3 is very difficult to understand. Please re-think about better illustrations of the model structure.

- The followings questions are related to the equations in Table 2. GWloss and Linear reservoir outflow are both calculated with the same equations. Why these two terms can be the same?

- For the tracer input that is calculated by "S = (S - Q) + N", the units of these terms are all different.

- How is the tracer concentration in storage CS(t) updated?

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