Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-101-RC1, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantifying flood-water impacts on a lake water budget via volume-dependent transient stable isotope mass balance" *by* Janie Masse-Dufresne et al.

Bruno Hamelin (Referee)

hamelin@cerege.fr

Received and published: 31 May 2020

On one hand, I consider this study as a stimulating contribution to the ongoing effort of improving the reliability of isotope-based modeling for the prediction of lakes hydrological behavior. Based on an attractive, although complex, case study, it presents a commendable attempt to assess quantitatively the sensitivity of the model results to the variations of the parameters. On the other hand, reading the manuscript is a bit frustrating due to a number of weaknesses in the data set and hypotheses, and also in the description of the model algorithm.

As for the data, the confidence that we can have on the robustness of the authors'

conclusions clearly suffers from the lack of continuity of the lake level monitoring, and from the lack of documentation of the lake stratification. Due to the ice cover (line 124) and logger failure (line 140), the level of the lake is available only for a part of the flooding period. In particular, we miss the comparison between lake A and lake DM for summer and fall 2017, when lake DM was seemingly above the threshold, and should thus have overflown again into lake A (Figure 2). More importantly, the lack of temperature and isotope data during the peak of the flooding period, and around the fall overturning, casts a strong doubt on the justification of the assumption of lake homogeneity. This weakness is acknowledged by the authors at several places (line 260, 269-270, 311, 433, 519). However, the reader is left with the impression that a two-layer model would definitely be needed, and that the model results might be much more sensitive to the seasonal stratification than to the other parameters tested in the study.

Another source of worry arises from a possibly spurious choice for the isotope composition of the groundwater inflow end member. As soon as the authors show that one of the two main supplies of lake A, i.e. the flood water overflowing from lake DM, shows an isotope composition which is below the lake evaporation line, one would expect the other main source, i.e. groundwater, to lie above that line, and not on it (line 290). This needs to be thoroughly discussed, all the more as the authors emphasize that ïĄď'G is among the most stringent parameters. More generally, some explanation is needed about the origin of the isotopic difference between the groundwater and flood water. Would it be possible to collect samples from the aquifer, away from the influence from the lake? How does the aquifer composition compare with the amount-weighted average of the rainfall seasonal variation?

On another aspect, I have some reservation about the way the model is described. A luxury of details is given on several very classical aspects already extensively described in previous works (i.e. Craig & Gordon's approach of the isotopic budgets), which could thus be better placed in appendix, while the description takes shortcuts on

other key linkage in the modeling procedure. For instance, the reader should not have to wait until line 357 (results section) to learn the hypotheses leading to the outflow estimate! Another example is the emphasis put on the volume-dependent modeling (line 49 and 54, 200-210). If the authors want to convince the reader that this is important, they have to better explain equation (6) to (10), which are quite cryptic, and compare with the same equation based on a constant volume approximation. This should also be discussed when looking at the results. This whole section should be written again, as a true instruction manual for anyone willing to apply such a model to another case-study.

In general, I have the feeling that the overall structure of the manuscript is a bit messy. I would recommend giving first all the information that can be deduced from the lake level variations (i.e. line 330-349), in order to introduce properly the aims of using isotopes to better unravel the contribution of the different sources.

Specific comments, in addition to those already pointed out by the other reviewer:

Line 220-239: "Outflow fluctuations were derived from water level variations at Lake A using linear interpolation between adjusted daily minimum and maximum outflow. Daily inflow into Lake A was calculated to compensate for the adjusted outflow, as the net water fluxes are required to be equal to the lake's daily volume variation." I still do not understand what is done exactly on this key point. This needs to be written in equations and related to the main unknowns in equations (1) to (10).

Line 275: "Interpolation was used to simulate the δP on a daily-time step." This suggests that the rain data show a smooth evolution through time along the season. Is this really the case?

Line 284: same evaporation slope for lake waters and flood water. Is this not surprising, as this slope depends on the climate parameters of Craig & Gordon's equation, while flooding and evaporation do not occur at the same period of the year?

Line 333: lake elevation assessed from well VP. Unclear what is meant by this statement as the difference of elevation of the water level between lake A and well VP is supposed to change with time along with the recharge/discharge alternation. (already pointed out by the other reviewer). Line 357: "the outflow fluxes are proportional to the lake's water level. We adjusted minimum and maximum outflow fluxes (Q) so that the latter respectively correspond to the minimum and maximum water levels." Again, (see comment above), I do not understand what this means. Line 368 and figure 6: The results obtained from ïĄď'D are strictly redundant to those from ïĄď'18O. What is really missing in this figure is some data at the beginning of May!

Line 434-437: scenarios A and B are supposed to compensate for the lack of data at the peak and end of the flood period. However, just mentioned like this without description, and sent back to Appendix C leaves a disastrous impression on the reader. Line 452: "The isotopic mass balance model revealed it was necessary to allow for significant groundwater outflow from Lake A during springtime to correctly reproduce the observed δL ". A best illustration of this conclusion would have been to compare the results of the model with and without the groundwater outflow. Line 485-487: confusion between tG and tf. (already pointed out by the other reviewer).

Line 503-504: confusion on "increase" ?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-101, 2020.