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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.

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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.

RC1-1a. 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).

Answer: Clarification. It is true that the data is limited, but we believe that it is sufficient and reliable to perform an isotopic mass balance model. We did not initially orchestrate the sampling campaigns and monitoring program to perform such study. However, a 100-year flood occurred during springtime 2017 and we took advantage of the importance of this hydrological event to assess the partition of the water mass balance under extreme conditions.

Considering Quebec's meteorological context, surface water level monitoring and surface water sampling can be hard to achieve during specific periods of the year. For instance, during springtime, it can be dangerous to perform in-lake water sampling due to ice-cover melting and flooding. Moreover, access to the study site was limited during the peak of the flood event. Ice-cover also constitutes a challenge for the lake water level measurements. Although attempts were made at installing loggers throughout late autumn and wintertime, drifting ice can damage the equipment and lead to its loss. Still, near shore lake water samples could have been collected in early May and provide valuable data for the characterization of the evolution of the isotopic signature of the lake. Also, punctual measurements of Lake A water level have been carried throughout the study period and provide complementary data to understand the evolution of the water levels (see submitted Fig. 1 with this response).

RC1-1b. 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.

Answer: Clarification. In a recent paper, Gibson et al. (2017) studied the impact of sampling strategies on the water yield (or depth-equivalent run-off) estimations for the Turkey Lake (32 m deep), Ontario (Canada) under stratified and well-mixed conditions. They reported 18% difference on the water yield when performing grab sampling (i.e., 1 sample at 1 m depth) and bulk sampling (i.e., assessment of the whole lake water column). The difference was less important (i.e., 11%) when comparing bulk sampling to integrated sampling for epilimnion, metalimnion and hypolimnion. They also reported discrepancies up to 20% for the water yield at the same lake according to the timing of the lake water sampling (3 times between Oct 27 and Nov 30). This last result shows that temporal shifts may induce more important bias than the uncertainty related to the lake stratification. Considering the above, we consider that the developed model yields a valuable estimation of Lake A water balance, because we accounted for the lake stratification by using a depth-average isotopic signature to represent δ_L.

RC1-2. 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 ïAËŻd'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?

Answer: Clarification. There is a scientific consensus concerning the isotopic composition of groundwater contributing to a lake (Edwards et al., 2004). The isotopic composition of the total inputs (δ_I) to a lake is resulting from the mixing between the isotopic composition of groundwater inflow (δ_G), surface water inflow (δ_{Is}) and precipitations (δ_P) and can be defined by:

$$\delta_I = (\delta_{Is} * I_s + \delta_G * I_G + \delta_P * P)/I$$

where I_S, I_G and P are the surface water inputs, groundwater inputs and precipitations, respectively. The total inputs (I) are described as $I = I_S + I_G + P$. Conceptually, δ_G, δ_{Is} and δ_P all plot along the LMWL, so does δ_I (given no influence of evaporation). Then, the position of δ_I on the LMWL is controlled by the relative proportions of I_S, I_G and P. In general, δ_I is not significantly influenced by δ_P, because P $<<$ I.

In cases where there is no surface water input (i.e., $I_s = 0$), the $\delta_I \approx \delta_G$ and the isotopic signature of the lake (δ_L) will evolve along the LEL from δ_G. In other words, the intersection between the LMWL and the LEL corresponds to the isotopic signature of the lake if $E = 0$ and is a good estimate of δ_G, when $I_s = 0$. In the case of our study site, the yearly recurring flood events are affecting the isotopic composition of the local groundwater contributing to Lake A. In fact, the hydraulic connection between Lake DM and Lake A and the high water levels result in an enhanced springtime recharge of relatively depleted water. Hence, the isotopic composition of local groundwater was conceptualized as a mixture between flood-water and isotopic composition of regional groundwater and, therefore, more depleted than the amount-weighted average of δ_P, which is $-10.2‰$ for $\delta^{18}O$ and $-68‰$ for δ^2H (calculated for the year 2016). While the partition between I_S and I_G was not known a priori, it is possible to infer that the intersection between the LMWL and the LEL would correspond to the most depleted isotopic signature the local groundwater could have. Computing the isotopic mass balance model with such estimation of the δ_G is conservative because it yields to a lower limit of the estimation of groundwater exchange.

Furthermore, estimating the δ_G from the intersection between the LMWL and the LEL was considered more adequate than measuring it from groundwater samples. Indeed, in hydrogeological contexts where groundwater-surface water interactions are important, the chemistry and isotopic signature of groundwater typically bear some local heterogeneity. Hence, the representativity from a groundwater sample can be hard to understand. In the context of this study, it was preferable to estimate δ_L from the intersection between the lake's LEL and the LMWL, as it represents the mean isotopic signature of the local groundwater contributing to the lake.

Note that this issue was also addressed by M. Rosen (see response to comment SC1-22a).

RC1-3. 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.

Answer: Done. We agree that the more common aspects of the model could be easily described in the appendix and that we would benefit from highlighting the details concerning site-specific points. Hence, we propose placing the description of δ_E (L159-L198) calculations in Supplementary Materials, as well as the results of the evaporative fluxes calculation with the Penman equation (i.e., L240-L249 and L259-L270) and the comparison with two other models (i.e., L250-L258).

RC1-4. 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.

Answer: Done. We made an effort at reorganizing the sections of the manuscript to make a better use of the "4 Results" and the "5 Discussion" sections. Below is the proposed structure (key figures and tables in italic):

4 Results

4.1 Water fluxes and isotopic framework (Figure 3, Figure 4 and Table 1)

4.2 Evaluation of the water budget

4.2.1 Volume-dependent isotopic mass balance (Figure 6 and Table 3)

4.2.2 Sensitivity analysis (Table 2)

4.3 Temporal variability in the water balance partition (Figure 7)

5 Discussion

5.1 Local flood-water marked groundwater

5.2 Importance of flood-water inputs in the water balance partition

5.3 Resilience of Lake A to surface water pollution (Figure 9)
Note that the original subsection “4.2.1 Insights from net water fluxes at Lake A” is not reporting a specific objective of the study, but rather provides a “reality check” by describing the net water fluxes and Lake A volume variation. Hence, we opt to merge it with the newly proposed “4.1 Water fluxes and isotopic framework” section. That being said, we conceive that the results illustrated in Figure 5 could simply be summarized in the text, and we propose including Figure 5 in the Supplementary materials in the revised version of the manuscript. Besides, we believe that it could ease the reading of the article to use a modified version of Figure 8 (see Fig. 2 submitted with this response) as a schematic to illustrate our conceptual model and the site-specific considerations. This could be added to a new subsection, “2.3 Conceptualization of the groundwater-surface water interactions”.

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

RC1-5. 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).

Answer: Done. To facilitate the reading and avoid confusion, we suggest a reformulation of Line 226-229:

> *“Considering the above, it was assumed that the daily outflow flux from Lake A varied linearly according to the lake water level; the minimum and maximum outflow corresponding to the minimum and maximum water level, respectively. The outflow range (i.e., minimum and maximum values) was adjusted to obtain best fit between the calculated and observed δ_L.*

> *Daily inflow into Lake A compensates for the adjusted daily outflow and daily lake volume difference.”*

RC1-6. Line 275: “Interpolation was used to simulate the dP 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?

Answer: Clarification. Reviewer 1 is correct. Sampling for precipitations was done on a monthly time step (approximately). Therefore, we did not analyze the isotopic signature of every single precipitation event and interpolation between the monthly samples was necessary to compute the model at a daily time step. When computing the evolution of the isotopic signature of the lake, precipitations are mixed instantaneously with the whole lake volume. As the daily precipitations are much smaller than the whole lake volume (and the other inputs), the bias caused by the interpolation of the isotopic signature of precipitations is not expected to significantly affect the results of the model. There would have been no gain on the accuracy of the model in sampling precipitations at a smaller time step.

RC1-7. 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?

Answer: Clarification. The slope of the LEL is strongly influenced by the relative humidity, and to a less extent by the temperature and the lake water balance (Gibson et al., 2015). Given the density of surface water bodies in Canada, the relative humidity is almost constant throughout the year and is roughly 80%. Hence, the LEL slope variations are expected to be very small for a specific location.

RC1-8. 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).

Answer: Clarification. There is in fact a water level difference between Lake A and the observation well VP. Note that the water level at VP is always lower than at Lake A, due to the pumping at the neighbouring bank filtration site.

When computing the model, the absolute lake water level is not important. The equations of the model are dependent on f, which is the remaining fraction of lake water:

$f = V/V_0$

where V_0 is the initial lake volume (at the beginning of the time step).

As the time series for Lake A water level was not covering the entire study period, a proxy was needed. The correlation coefficient between Lake A and VP is 0.9885 for all the available data (from 2017 to 2020, which spans both high and low water periods). Hence, while the water levels at Lake A and VP are not identical, the daily variations are expected to be similar. We thus conceive that VP is a good surrogate for Lake A.

RC1-9. 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.

Answer: Done. A reformulation of Line 226-229 was proposed. See response to comment RC1-5.

RC1-10. Line 368 and figure 6: The results obtained from dAËŻdD are strictly redundant to those from dAËŻd18O. What is really missing in this figure is some data at the beginning of May!

Answer: Clarification. The use of dual isotopes (i.e., $\delta^{18}O$ and $\delta^{2}H$) is helpful to

perform adequate parametrization of δ_A, especially in seasonal climates. However, this aspect was not one of the main goal of our study and a comprehensive study on this topic was already published by Yi et al. (2008).

Concerning the apparent lack of data in early May, we reiterate that in-lake water sampling can be dangerous to achieve in certain climates (see our response to RC1-1). In fact, ice-melting and limited access to the study site during the flood event prevented us from performing bulk water sampling along the water column. Despite these field conditions, we were able to perform near-shore lake water sampling. Although these samples are only representative of the surface-most part of the lake, they are still valuable for our understanding of the lake's dynamics. Furthermore, reference scenarios A and B were used to address this issue specifically (see comment RC1-11).

RC1-11. 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.

Answer: Done. We thank Reviewer 1 for this suggestion. The scenarios A and B could be both presented in Figure 6 and we would benefit from bringing the comparison between the two scenarios to the forefront.

RC1-12. 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 dL ". A best illustration of this conclusion would have been to compare the results of the model with and without the groundwater outflow.

Answer: Moot. From our point of view, performing a simulation without any groundwater outflow would not be representative of any realistic scenario. However, it could be interesting to quantify the lake water level increase that would be needed to simulate the same δ_L with and without outflows from the lake. It would underline the importance of considering the groundwater-surface water interactions when studying the water balances of flood-affected lakes.

RC1-13. Line 485-487: confusion between tG and tf. (already pointed out by the other reviewer).

Answer: Done. It is t_f. Thank you for pointing that out.

RC1-14. Line 503-504: confusion on "increase" ?

Answer: Done. Reviewer 1 is right. It should be written "decrease".

References

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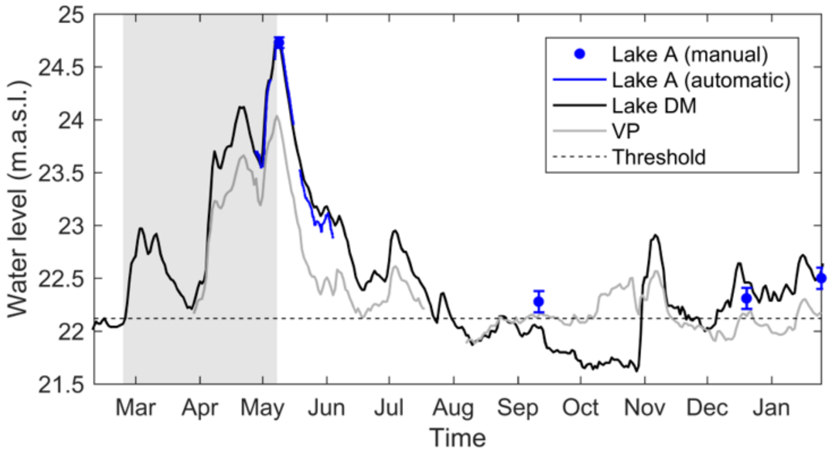

Fig. 1. Revised version of Figure 1.

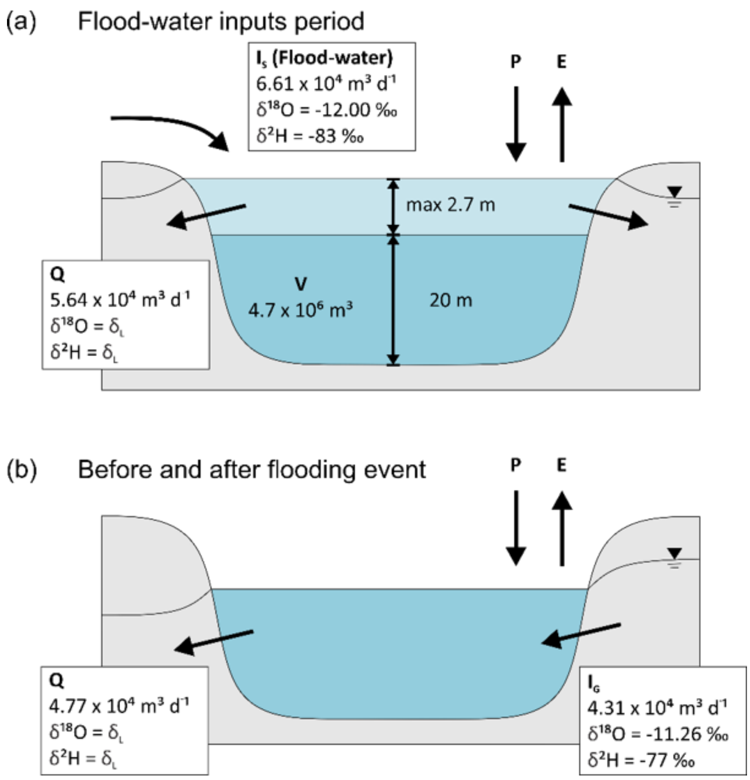

Fig. 2. Original version of Figure 8.