Comments to the Author:
Dear authors,

Thank you for your patience during this review process. I have now heard back from the two reviewers who looked at the last two versions of your manuscript. Both of them commended you on the comprehensive nature of your last revisions. One reviewer has no further suggestions, while the other has a list of minor elements they suggest that you clarify or nuance in your manuscript. One of their remaining concerns is what you present as evidence of bank storage, which they do not find fully satisfactory. To address that concern, you may either choose to change your terminology surrounding bank storage throughout the whole manuscript, or provide additional justification and evidence using your isotopic data (in the absence of hydraulic data). Should the above-mentioned options (terminology change or isotope-based justification) not be possible, then you should probably nuance your bank storage-related findings by presenting them as first-order flux estimates or hypotheses, and discuss the uncertainty associated with them.

I am returning your manuscript for minor revisions and look forward to receiving the new iteration of your manuscript, together with your responses to reviewer comments (which I will evaluate).

With best regards,

Genevieve Ali

Response to Editor

Dear Prof. Ali,

We have considered all the comments and suggestions from the reviewer and revised the manuscript accordingly. To address the main remaining concerns (evidence of bank storage and the use of this term), we opted to i) change the terminology (with temporary subsurface storage), ii) provide additional simulations (to better link the potential flood-water-like subsurface inputs to the modelled isotopic signature of the lake) and iii) emphasize the first-order nature of the water flux estimates throughout the manuscript.

In addition to the reviewer's comments, we modified the definition of the flushing time (t_f). In the former version of the manuscript, we defined t_f as the ratio of the lake volume to the total water inputs (t_f = V/I). In the revised version, we propose to specifically consider the flushing time by groundwater ($t_f = V/I_G$). This modification was deemed necessary to correctly compare our results with literature data (as in Arnoux et al., 2017b). Accordingly, modifications were made to Eq. (4) at Line 281 and throughout Sect. 5.1.

Also note that we checked for typos and have corrected two co-authors affiliations. These changes are indicated in the marked version of the manuscript.

Sincerely,

Janie Masse-Dufresne

Report #1

Submitted on 13 May 2021
Referee #3: Chani Welch, cwelch@okanagan.bc.ca

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication):

The manuscript requires further support of the process of bank storage, and further acknowledgement of the first-order nature of the flux estimates.

Hello,
This manuscript has vastly improved with the reorganisation and clarification provided. I have two general concerns remaining:

RC3-1. The link to bank storage is weak in the modelling and results, but prominent in the findings. The available hydraulic data do not provide supporting evidence (the groundwater level is not recorded above the water level of Lake A). I understand there were technical issues with the hydraulic measurements, so perhaps more use could be made of the isotopic model, or a clearer link provided to the hypothetical scenarios presented for the G-index. For example, can you get a good fit to the isotopic data with a groundwater input that has an isotopic composition intermediate to flood water and groundwater in the May – August time period?

Answer: Done. We are grateful to the reviewer for this comment. As suggested, we modelled δ_L when considering that groundwater inputs are characterized by the isotopic signature of flood-water (δ_{ls}) after the flooding event. We modelled four hypothetical scenarios for which 25%, 50%, 75% and 100% of the outflows of the lake during the flood-water control period were stored in the subsurface and eventually discharged back to the lake (from May to August). It is noteworthy that a better fit is obtained between the modelled δ_L and depth-averaged δ_L when considering that 25% to 50% of the potentially stored flood-water returns to the lake. This result further supports our conceptual model and the fact that considering flood-water-like groundwater inputs is important when assessing water balances.

We thus modified Fig. 6 to illustrate these hypothetical scenarios. Note that the order of Sect. 4.3 (Temporal variability in the water balance partition) and Sect. 4.4 (Importance of bank storage on the water balance partition) has been reversed to accommodate for this modification. Also, Sect. 4.3 (formerly 4.4) was modified to discuss the results concerning the impact of flood-water-like groundwater inputs on the modelled δ_L, while considerations regarding the evolution of the G-Index were merged to Sect. 4.4 (formerly 4.3).

RC3-2. The isotopic data presented in Appendix E (and Figure 6) clearly indicates that the lake is not well-mixed in August (the only depth profiles outside of winter). Arguments supporting the well-mixed model choice are provided; however, I do not think the first-order nature of the estimate of water fluxes is clearly emphasized throughout the manuscript (for example, it is not mentioned in the abstract).

Answer: Done. As suggested, we opted to specify the first-order nature of the water flux estimate at Line 17 and to state a range for the relative contribution of groundwater and surface water inputs to the lake (based on scenarios A and B) at Line19 (in the abstract). It was additionally specified at Line 402 and Line 469.

Please note that the listed line numbers in this response refer to the revised version of the manuscript.

Other minor comments are provided below.

RC3-3. Line 21: Lake A water budget?
Answer: Done. It was modified to "Lake A water budget".

RC3-4. Line 22: This is contradicted in the discussion.
Answer: Clarification and done. At L22, we refer to the resilience of the lake obtained from the reference scenarios A and B, which only considers the direct flood-water inputs. Then, we discuss the potential impact of considering subsurface flood-water-like inputs on the resilience of the lake to groundwater changes (see downward arrow in Fig. 8). To avoid any confusion, we opted to reword L20-24. It now reads as:

> *"However, when considering the potential temporary subsurface storage of flood-water, the partitioning between groundwater and surface water inputs tends to equalize, and Lake A water budget is found to be more resilient to groundwater quantity and quality changes."*

RC3-5. Line 48: Delete "nearly impossible". The reference doesn't say it's impossible. Just time-consuming, expensive, and difficult.
Answer: Done. It was modified to "time-consuming, expensive, and difficult". Note that it was also deleted in the abstract (at Line 14).

RC3-6. Line 54: This paragraph would benefit from a topic sentence before diving into the specifics of previous studies.
Answer: Done. We are grateful to the reviewer for this suggestion. The following sentence was added before the paragraph:

> *"To gain information on the timing of hydrological processes, one may use a transient and short time step isotopic mass balance."*

RC3-7. Line 66: I remain unconvinced that bank storage is the correct term – perhaps a more general delayed (in subsurface) and direct flood water inputs?
Answer: Done. We opted to modify for "direct flood-water inputs" vs "temporary subsurface storage of flood-water" at L66 and throughout the manuscript (including the 4.4 sub-section title). To better define these terms in the context of this study, we also added the following material at L53 (after the first mention of "temporary subsurface storage of flood-water", except for the abstract).

> *"In this study, we define the direct inputs refer to the flood-water that enter a lake via the surface (e.g., by inundating and/or flowing through a stream), while temporary subsurface storage of flood-water encompasses the flood-water-like inputs that reach the lake via subsurface (e.g., through floodplain recharge or bank storage)."*

RC3-8. Line 72: Flood water storage – surface (in the lake) or subsurface?
Answer: Done. To avoid any confusion, and in line with other comments (RC3-8), we opted to modify "implications of flood-water storage on the water balance" to:

> *"implications of flood-water-like subsurface inputs on the water balance partition".*

RC3-9. Line 81: "a" not "an"
Answer: Done. It was corrected to "a".

RC3-10. Line 100: Rephrase: The direction of the surface water flux in S2 reverses when the water level in Lake DM exceeds… Does flow reversal also occur in S3?

Answer: Done. Flow reversal also occur in S3, but no topographic land survey was conducted along this stream. Hence, the elevation of the topographic threshold is unknown. To clarify, we added the following material at L102:

> *"Flow reversal also occurs in S3, but the elevation of the topographic threshold is unknown."*

RC3-11. Line 124: "Tin Lake A" – rephrase or define.
Answer: Done. It was corrected to: "The water level in Lake A".

RC3-12. Line 125: Define the observed period here at first mention.
Answer: Done. We defined the observed period at L125 (April 27th, 2017 to May 17th, 2017).

RC3-13. Line 127: Suggest rephrasing - It remains an assumption that Lake DM controls the water level variation at obs well VP. Perhaps "the data indicates". To the reader it is unclear why the Lake A water level is not used – state here that the logger broke.
Answer: Done. It was rephrased to "the data indicates". Also, the following material was added to L128 to specify that logger in Lake A broke and prevented comparison:

> *"Lake A water level was also presumably controlled by Lake DM until late July 2017, but technical issues prevented confirmation (i.e., logger in Lake A broke on May 17th, 2017)."*

RC3-14. Line 131: Suggest rephrasing to say that Lake A water level is not controlled by Lake DM in this period.
Answer: Done. It was rephrased to:

> *"It is thus possible to infer that Lake A water level was not controlled by Lake DM from August 2017 to late October 2017."*

RC3-15. Line 137: Figure 2 indicates the water level in Lake A is below Lake DM in mid Dec (even considering error bars shown) – why wouldn't the water flow into it? Maybe it only happens for a short time (which could explain the lower correlation)
Answer: Clarification and done. The reviewer is correct – the water level in Lake A is below Lake DM in mid Dec. However, it is uncertain whether a hydraulic connection can establish during wintertime (due to freezing of the stream). To avoid misleading interpretation, we corrected L137 to:

> *"It is thus likely that Lake A received little to no surface water inputs from Lake DM from November 2017 to January 2018. In this context, surface water inflow from Lake DM during autumn and winter are considered negligible in this study and not included in the developed stable isotope mass balance model (Sect. 4.2)."*

RC3-16. Line 151: Could there not also be Qg out the southern end of Lake A? Ie in the direction of regional groundwater flow.
Answer: Clarification and done. The reviewer is correct – there is a regional groundwater flow in the NE-SW direction. It is noteworthy that this information does not appear in this version of the manuscript. It was only mentioned in response to the reviewer (last revision round, RC3-34). We opted to add the following material at L150:

> *"Given the regional groundwater flow in the NE to SW (Ageos, 2010), QG can also presumably occur along the SW bank of Lake A."*

RC3-17. Line 157: Rephrase to put Lake DM immediately after Qs otherwise this sentence reads as if Qg is possible going to Lake DM, which contradicts Line 150.
Answer: Done. It was rephrased accordingly to the reviewer suggestion.

RC3-18. Line 161: If the water causing the water level increases is not coming from Lake DM, then where is it coming from? Direct precipitation? S1?

Answer: Clarification and done. We are grateful to the reviewer for this comment. Some clarification was needed. L161 was corrected to:

> *"In this context, we conceptualized that Lake A water level variations are mainly controlled by groundwater flows (I_G and Q_G). Surface water inputs (I_S) are set to zero during this period (see Sect. 2.2)."*

RC3-19. Line 274: The manuscript first argues that there is not a strong correlation, and then use Lake DM as representative? I get that the absolute level doesn't matter, but why not just use the obs well the whole time? Does this change the results?

Answer: Clarification. First, water level in the observation well VP was not available for the whole simulated period. The water level in Lake DM is the only available measurement for Feb 2017 to late March 2017. Second, during the period of high-water level (late April 2017 to mid-May 2017), there is a good correlation between Lake DM and Lake A water levels. During this period, the daily water level variations at observation well VP were less important, and using observation well VP as a proxy would have resulted in an overestimation of the surface and groundwater fluxes during springtime. Concerning the autumn-winter period (November 2017 to late January 2018), the reviewer is correct – the correlation between Lake DM and observation well VP water levels are lower. If the water level at the observation well VP is used as a proxy for Lake A for the autumn-winter period, the simulated water fluxes would be lower and the modelled δ_L would be more enriched. While using VP as a proxy during this period does not significantly impact the modelled δ_L in scenario A, it does slightly affect scenario B. To obtain a good fit between the modelled δ_L and depth-average δ_L, the Q_{min} can be adjusted to 1.5×10^4 m^3/d (instead of 1×10^3 m^3/d). Although the simulated fluxes (Q and I) increase, the share between I_S and I_G (40% - 60%) and the t_f (103 days) remain similar to the reference scenario B (40% - 60%; 110 days).

RC3-20. Line 283: Are there 2 Penman-48 methods? Suggest rephrasing as it is unclear which is used in the model, the one that underestimates or the one that doesn't. If it's the one that doesn't underestimate in late summer-fall, the results are unlikely to be affected. Otherwise: Line 284: how did you resolve this? Specific heat capacity of water. Does this markedly affect the isotopic balance and interpretation?

Answer: Done. We opted to better specify the "standardized Penman-48" and the "simplified Penman-48" equations in the manuscript. In the model, we use the "standardized Penman-48" equation – the one that does not underestimate E in late summer-fall.

RC3-21. Line 291: "The outflows of the lake are thus…" I still find these comments misleading. I suggest rewording to explain that the change in outflows from the lake is roughly proportional to the change in water level.

Answer: Done. We clarified L290-293. This now reads as:

> *"The direction and intensity of the water flux at the lake-aquifer interface can be conceptually described by Darcy's Law which states that $Q = KAi$, where K is the hydraulic conductivity, A is the cross-sectional area through which the water flows, and i is the hydraulic gradient. Given the significant depth of Lake A (i.e., 20 m) in comparison to the maximum water level change during the flooding event (i.e., 2.7 m), the variation of the A and K are expected to have minor impact on Q. Hence, the change in outflows from the lake is expected to be mainly controlled by i changes and, consequently, to be roughly proportional to the change in lake water level"*

Note that L367-369 was also modified according to the reviewer's suggestion.

RC3-22. Line 302: Suggest inserting "it is assumed that" immediately before "the rising water level". Groundwater may still be entering lower in the lake.
Answer: Done. The reviewer is correct. We modified L302 accordingly to the reviewer's suggestion.

RC3-23. Line 344: Does this use of the LMWL-LEL account for flood waters? This assumption will likely only hold where groundwater is sourced from the local precipitation, and is not subjected to different rates of evaporation during infiltration. It is curious to me that no isotopic data collected from the groundwater wells at the site is presented to support this choice.
Answer: Clarification. Please see response to comment RC3-24.

RC3-24. Line 348: Where is Saint-Telesphore and why is it considered a useful comparison?
Answer: Clarification. St-Télesphore is located at ~43 km from the study site (Fig. 1b). It was used to compare the isotopic signature of groundwater in a similar context, i.e., where there is a widespread cold season bias to groundwater recharge. Such comparison was added to the manuscript in response to a previous comment (last revision round, RC3-58). Please note that this sampling station was first named "Vaudreuil" in the response to the reviewer. It was corrected to "Saint-Télesphore" in the last revised version of the manuscript.

Note that the following was added to L349:

> *"Note that the location of Saint-Télesphore station and Ottawa are depicted in Fig.1b."*

RC3-25. Line 377: depth-average"d" – the "d" is missing
Answer: Done. It was corrected.

RC3-26. Line 381-384: Unclear where the dates and isotopic values come from until reading the caption to Table 1. It would help the reader if the scenarios were more clearly presented and their purpose more clearly articulated – ie is it to compare lengths of flood-water control?
Answer: Done. We made efforts at reorganizing the paragraph to better explain that one of the two samples collected at the surface of the lake may be representative of the whole water column. This now reads as:

> *"While depth-averaged δL was not available during the flood-water control period (i.e., late February to early May), water samples from the surface of Lake A provide relevant evidence to better constrain the model. It is likely that Lake A was fully mixed during the flood-water control period, and that the water samples collected at the surface of Lake A on April 27th, 2017 or May 9-10th, 2017 are representative of the whole water body. Indeed, the observed surface water temperature was < 5°C until early May (see Fig. C1) and suggests a limited density gradient along the water column which does not allow for the development of thermal stratification. In this context, we opted to simulate two scenarios (A and B), for which the isotopic mass balance model is either constrained at $\delta^{18}O$ = -11.20 ‰ and $\delta^{2}H$ = -76 ‰ on May 9-10, 2017 or at $\delta^{18}O$ = -11.86 ‰ and $\delta^{2}H$ = -80.68 ‰ on April 27th, 2017.*

RC3-27. Section 2.2 Sensitivity analysis. Should this be Section 4.2.2? Given what the paper is aiming to do, this section would benefit from linking the effects of these changes to the different water fluxes.
Answer: Done. The reviewer is correct. It was modified to "4.2.2". Also, we added links between the uncertainties associated with the input variables and the flux estimate changes. This further helps to underline the first-order nature of the water flux estimates (see comment **RC3-2**).

RC3-28. Section 4.4 I'm struggling to reconcile this with the isotopic modelling – if the water discharging back to the lake as flood water, then wouldn't it have the isotopic signature of flood water? My understanding is that deltaG is held constant in the model. To test this, why not alter deltaG over this time period?

Answer: Done. The reviewer is correct – the δ_G is held constant in the model. Our intention to discuss the theoretical impact on the G-Index. However, we agree that our work could benefit from discussing the results when considering flood-water-like groundwater inputs after the flooding event. As mentioned in response to **RC3-1**, we did perform such simulation and the results were added to the manuscript (Fig.6, discussed in Sect. 4.3).

RC3-29. Line 577: This contradicts line 522 which categorises the lake as relatively resilient – clarify.

Answer: Clarification and done. At L577, we refer to the resilience of the lake obtained from the reference scenarios A and B and the associated scenarios considered in the sensitivity analysis, which only considers the direct flood-water inputs. At L522, we discuss the potential impact of considering the temporary subsurface flood-water inputs on the resilience of the lake to groundwater changes (see downward arrow in Fig. 8). To avoid any confusion, we opted to reword L577-580. It now reads as:

> *"Despite sensitivity to some variables, all model scenarios considered in the sensitivity analysis converged on the results that Lake A is mainly dependent on groundwater inputs and has a rapid (<1 year) flushing time by groundwater, suggesting that Lake A would be highly sensitive to groundwater quantity and quality changes.*
>
> *When taking into account for potential subsurface storage, a better fit could be obtained between the modelled and depth-averaged isotopic signature of the lake, suggesting that the contribution of flood-water-like subsurface inputs is important to consider when assessing for water balance at flood-affected lakes. In fact, the increased contribution of surface water (from subsurface storage) resulted in a lower the contribution from groundwater and, consequently, in an increased resilience to groundwater changes."*