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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.

Anonymous Referee #2

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* GENERAL COMMENTS

In this manuscript entitled "Quantifying flood-water impacts on a lake water budget via volume-dependent transient stable isotope mass balance", the authors focus on an artificial lake and justify their study by stating that "[understanding] the relative importance of the hydrological processes in lakes can also help to depict the vulnerability and/or resilience of a lake to pollution". They aim to develop a predictive model of both atmospheric and water balance controls on isotopic enrichment, quantify of flood-water inputs to the lake, and conduct a model sensitivity analysis was conducted to evaluate potential sources of uncertainty. Overall, the manuscript is of appropriate length and

well written. Figures and tables are also of good quality and rich in information without being too crowded. While I enjoyed reading this manuscript, I think that the authors need to make a strong case for the broader relevance, impact and transferability of their methods or conclusions, in addition to revisiting the structure of manuscript. My most major criticisms are as follows:

** In its present state, the manuscript pretty much reads like a case study report. There is nothing wrong with case studies per se, as the uniqueness of place makes the conclusions of many papers inherently site-specific. That being said, I think that the authors should try to extrapolate their conclusions (or speculate about how their conclusions might extend) to other lakes (artificial or not) in Canada, North America and around the World. What makes Lake A and Lake DM different (or not) than other lakes where similar isotope mass balance approaches have been used in the past? In other words, what makes the present study novel? What are the really key contributions that represent an advancement to the science – and that may be relevant beyond the particular site that the authors focused on? Can the results be extrapolated to depressional wetlands which are affected by flooding as well? And if results and conclusions cannot be extrapolated, what about some of the methods applied in the current manuscript? My asking those questions is not my way to say that there are no novel contributions in this manuscript, but rather to say the authors have not explicitly identified them and should highlight them better.

** The introduction lacks an overarching goal or research question for the study, as well as specific research objectives or questions. Instead, the last paragraph of the intro just states that the present study builds upon two other studies. The only sentence of the introduction that could be seen as a research goal is the one that reads as: "The main purpose of this study was initially to expand our understanding of flood-affected lake dynamics in the context of a seasonal climate". It is quite vague, though, so I suggest that the authors include some more specific objectives or questions at the end of the introduction. This should also help highlight the novel contributions that the present

study intends to make.

** There seem to be a lot of "results" that are listed in Section 3, which most readers would equate to a Methods section (and not a results section). I would suggest that the authors try to reorganize their text a bit, so that section 3 only focuses on methods while section 4 summarizes results.

** Following up on the previous point: Section 4 does not seem to focus on "plain results" only, as it includes several interpretations, discussions and linkages to the literature. Section 4 therefore reads as a combined Results and Discussion section, which is a bit surprising as there is a separate discussion section later. I suggest that the authors try to better distribute methodological aspects, results and discussions/interpretation into distinct sections (and sub-sections).

** Along the same lines as the two previous points, Section 5 is a bit confusing. There are completely new results (e.g., Table 3, Figure 7) first reported on in this section. Conversely, there are not a lot of literature references (none in sub-section 5.1, and only 1 literature reference, as far as I can see, in sub-section 5.2). So, a lot of the text listed under the "5 - Discussion" header does not really read like a typical discussion section, in the sense that there is very little confrontation of the present study results with the existing literature. The authors should rectify that as much as possible.

** Sub-section 5.3 is a bit confusing. The authors provided a list of physical water quality parameters + ions earlier in their manuscript. Based on their introduction, I expected those physical water quality parameters + ions to be used to support the "surface water pollution" aspects of the manuscript. However, in sub-section 5.3, there is no reference to those parameters/ions, and the assessment of resilience to water pollution is solely based on mean flushing time. Why were the parameters/ions described earlier not used? And is it adequate to use the mean flushing time as a proxy measure for a lake's resilience to "all" surface water pollution, regardless of the reactivity/sorption co-efficients of the chemical determinants under consideration? This last question may

be out of scope for the present manuscript, but a clarification sentence would help manage readers' expectations.

** The first sentence of the conclusion states reiterates that the "goal" of the present study was to "develop a volume-dependent transient isotopic mass balance model, assuming well-mixed conditions, in order to better understand the dynamics of the hydrological processes at a flood-affected lake in southern Canada". As commented upon above, I find this to be rather vague. After reading through the details of the manuscript, it seems like the authors specifically want to address questions related to the relative importance of groundwater for Lake A on an aggregated annual scale as well as through different seasonal/wetness conditions (what they refer to as temporal variability of hydrological processes). The authors also dedicated a fair amount of time/manuscript space to discuss many different elements, e.g.: the peculiarities of lake dynamics under flooding conditions, uncertainties associated with their isotope mass balance model (those uncertainties are multiple in nature, i.e., input data uncertainty, structural data uncertainty, output data uncertainty, even maybe model parameter uncertainty), and the application of pollution resilience assessment framework. It is quite difficult, from the whole manuscript, to figure out which of those elements are primary versus secondary targets/goals/objectives of the manuscript and how they relate (or not) to one another.

I think that there is a nice science story in the manuscript, and I hope the authors will see my comments as suggestions for strengthening it and making it interesting to the broad readership of HESS.

* SPECIFIC COMMENTS ABOUT SOME TEXT SECTIONS OR FIGURES

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* TYPOS AND EDITORIAL SUGGESTIONS

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