Interactive comment on "Landscape and groundwater controls over boreal lake water chemistry and water balance heterogeneity in an esker complex of northeastern Ontario, Canada" *by* Maxime P. Boreux et al.

Anonymous Referee #2

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Landscape and groundwater controls over boreal lake water chemistry and water balance heterogeneity in an esker complex of northeastern Ontario, Canada

Boreux et al.

Review: Boreux et al. utilize lake water isotope and chemistry data to identify lake types in an esker complex in Ontario, and specifically characterize the role of groundwater and landscape position on lake conditions. I enjoyed reading the paper. This is an excellent dataset and, by and large, the interepretations appear to be sound and

supported by the data. So, from an overall perspective, this paper will make for a useful contribution on lake hydrology. In particular, the links between water isotope composition and water chemistry serve as useful example of the strength in such an approach to characterize lake conditions at numerous locations across landscapes. That being said, there are both analytical and structural issues with this manuscript that, in my view, should be addressed. I elaborate on these below.

1. Analytical. Specifically here, I refer to calculation of the isotope-inferred E/I ratios. If the authors intend on retaining this piece of the manuscript, much more detail needs to be provided as to how these calculations were performed. For instance, at the top of page 9 where the E/I ratio is provided, there is an incorrect definition of dE. Several of the papers listed in the reference utilize the Gonfiantini (1986) equation, but I do not know if the authors used this equation. Furthermore, there are no details provided regarding how they computed dA, or what they used for temperature or relative humidity. Fundamentally, insufficient text and details are provided, such that the calculations cannot be reproduced by a reader. So, much more needs to be provided here. But I wonder if this is even necessary? The E/I results are barely mentioned in the Discussion. It seems that, for the apparent purposes of this paper, the authors could simply get away with comments referring to differences in the degree of evaporative isotopic enrichment.

Perhaps it is just my weak understanding of statistics, but I would like to see a bit more background on the breakpoint analysis. This seems to be a very important part of the paper, as it apparently identifies 282 m (or is it 242 m? – both are listed, which added to my confusion) as the landscape position in which the role of groundwater changes. My limited understanding of breakpoint analysis is that it is used to detect when there is a change in a trend (often applied to time-series measurements), but it seems like this analysis is being applied differently here.

2. Structural. In its present form, the manuscript imposes the lake classification scheme on the results, but it is only part-way through the Discussion where the ra-

tionale for the classification scheme is presented. This creates a lot of awkwardness with the paper. The reader is quite literally forced to take the author's word for it that there is some basis for the classification scheme while they read the results (but they have no idea what that is). Since my expertise lies more with the isotope component of the paper, I kept wondering how the authors are going to distinguish the role of groundwater versus precipitation, since they often have very similar isotope compositions. It was only much later, in the Discussion, where I learned that the role of groundwater, in fact, is largely, maybe exclusively (?) based on the water chemistry. There needs to be some re-structuring of the manuscript, so that the reader can examine the results, unencumbered by the imposing of the classification scheme, and then use those results to develop the classification rationale. I think you are likely to end up with the same ultimate intepretations and conclusions, but the path to getting there needs refinement and re-organization so that it will be easier for the reader to digest.

Other comments are listed below:

Line 17: Insert a sentence explaining why the study was conducted. Line 21-22: Interesting that evaporation did not lead to concentration of ions. Seems like an unexpected relation. Line 28: 'characteristics' Line 33: 'discharge lakes showed' Line 34: 'would only be likely affected' Line 64: delete 'changing' Line 84: replace 'to test' with 'examine' . . .. at this point, I'm wondering how the authors might distinguish influence of groundwater from precipitation since they often have similar isotopic signatures. Authors might want to acknowledge this challenge here, and may use this to also rationalize the combined use of water isotopes and chemistry. Line 91-92: re 'water management and conservation goals' . . . some mention of this applied aspect should be stated in the abstract, and furthermore, the abstract might relay more directly how the findings from this study contribute to these aspects Figure 1 caption: 'northeast'; label 'a' and 'b' on figure and label lakes and locations mentioned at the top of page 5 Line 107: delete 'the' Line 129: 'texture. The esker' Line 133: 'aquifers at its edges where' Line 153-154: Can you explain the three-time sampling rationale? This puz-

zled me a bit because the data are weighted to the early ice-free season, as mostly mean values are used, and there is very little mentioned about seasonal patterns in the data that are obtained. Line 209: As stated above in my comments, I wonder if you even need to perform these calculations. If you would like to retain, more details are required as per comments above. Line 233: Another point of confusion for me was that dI was assumed to be the average of precipitation, and was assumed to be the same for all lakes. Yet knowing that the goal here is to detect influence from groundwater, I wondered then if there might be some variability in dI among your lakes. And, of course, I remained curious to know how groundwater influence might be distinguished from precipitation since they often have similar isotope signatures. I'm not sure any of this was ever explicitly discussed in the manuscript. Line 234-235: Incorrect calculation of dE. Very puzzled by this statement. Line 242-244. I gather you are summarizing here, but you have two springtime samples and one late summer sample, which makes this a bit awkward. Line 250: 'Local Evaporation Line' Line 251-252: Concern about overlapping isotope signatures of gw and precip confirmed! Line 256-257: Awkward incorporation of elevation here. Seems like this should be saved for the next paragraph. Figure 2b: Label your figures 'a' and 'b'. First awkward imposing of lake type here, without rationale being provided. Line 266: A smalll thing but I would replace 'indicating' with 'suggesting'. Of course, this may be just one factor driving the water balance. At this point in the article, it seems the authors are overly anxious to get to the conclusion without proper development of the results and interpretation. Figure 3: I don't understand the application of the breakpoint analysis. Line 278: delete 'high' Line 279: An E/I ratio of '8' cannot be quoted so directly if using a steady-state model. Line 280: More awkward imposing of lake types without rationale. Same with Figure 4, which isn't a very effective way of presenting E/I ratios, as they are calculated from the isotope measurements. Line 296: Are they really conservative isotope tracers if they are changing as a consequence of evaporation? Table 1. More confusion about the breakpoint analysis, confounded by listing of ∼'282' m in the table and '242' m on p. 14 and elsewhere. Lines 323 to the end of this section: Again, imposing the lake

types here is simply not warranted. Extremely confusing to the reader. Data needs to be presented first. Then it can be interpreted. Then a classification system can be developed. P. 15. At this point in the paper, I had too many questions to be able to critically evaluate further. But as I say in my opening comments, I believe the story may largely be ok, it is just the development of that story that really needs to be over-hauled. Figure 7. First (and perhaps only) characterization and use of the seasonal differences (although literal use of the very elevated E/I ratios is beyond the limitations of the model). Difficult to detect if this is really utilized in the Discussion. Is it relevant? Or can more be made of it? But again, as written, the reader is forced to accept the lake type classification. Line 369: replace 'high' with 'short' Line 373-374. So now we're finally getting some explanation of the different lake types, but this is confusing too. How do you know that gw is causing the difference here (which is not explicitly stated)? Line 379: replace 'composition' with 'water chemistries' Line 384, 386: How do you know the 1 km statements? Where does that come from? But here, I get the sense that the gw role is mainly based on the water chemistry differences, and less so on the isotope data. Line 404: This classification section has been pre-empted by all the comments before this point on lake types, which is the most awkward aspect of this paper. Sorry to keep emphasizing this. I'm sure it can be resolved! Line 407: 'conservative water tracers (water source)' – I am very confused by this terminology. The isotope data reflects more than just source water. It is modified by varying degrees of evaporative enrichment. Line 428: Awkward sentence. Line 436- 437: 282? Or 242? But I still don't understand how this elevation was defined. Line 451-453: Not a sentence. Line 473: What is meant by the word 'mounded'? Line 545: 'those'? Line 575: 'extent' Line 600-602: Interesting. Shouldn't these activities be mentioned in the Intro to help rationalize the study? Line 632: Awkward phrasing. Line 635: delete 'are' I like the conclusion – well stated and summarized!