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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantifying groundwater dependence of a sub-polar lake cluster in Finland using an isotope mass balance approach" *by* E. Isokangas et al.

Anonymous Referee #1

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This paper uses available hydrologic, meteorologic, and isotopic data from a set of 67 lakes in Finland to develop a framework to assess the relative contribution of groundwater inputs to each of the lakes. The results of this study are likely to be useful in efforts to preserve the oligotrophic nature of the lakes. The approach has been used elsewhere but is somewhat novel in its application. As the study of limnology moves toward regional and larger-scale understanding, the use of water stable isotope techniques to place lakes in appropriate hydrologic context is going to be critical. For these reasons, this manuscript is appropriate for publication in HESS and will likely be cited and replicated in other studies.

However, I have several fairly substantial reservations to publishing the manuscript as it is currently written. The most serious of these are the simplified assumptions embedded in the calculation of the total inflow to evaporation ratio (Itot / E). The assumptions are logical and necessary in this study, but they should be investigated more fully before the manuscript can be published. They include 1) assumptions of steady-state isotopic signature in the lakes; 2) implicit assumptions that all of the lakes are completely mixed; 3) assumptions that all inflowing water has the same isotopic signature. All of these assumptions should be discussed in more detail and in the case of number 2 & 3, the sensitivity of the outcomes should be tested against these assumptions.

Beyond that, the paper is well-written and well-presented. A minor point where the manuscript could be improved is that the motivation for the study is described very clearly at the end of the Introduction, but it is never revisited later in the paper. The authors have some important results which could help inform management of these lakes and a few sentences placing the results in that context would improve the overall manuscript.

Detailed comments follow:

p. 9186, line 11 – "a. . . one-off survey" – unscientific language.

p. 9816, line 15 – "more trophic" – change to "more productive".

p. 9191, lines 8-11 – This approach assumes that all of the lakes mix completely. How does changing this assumption change the outcome of the study? (see Stets et al. 2010).

p. 9192, line 3 – dLS assumes that the lakes are in isotopic steady state, but this is almost certainly not the case (see figure 6). How does this assumption affect the study?

p. 9192, line 11 – "If . . .the isotopic composition of atmospheric water vapour is in isotopic equilibrium with the total inflow" – This seems like a poor assumption because

most of the groundwater wells in figure 4 are close to the average recharge, not the current atmospheric conditions. Why not set the isotopic composition of atmospheric water vapor equivalent to precipitation at or near the time of the survey? (see Gibson et al. 2002)

p. 9192 – It is worth pointing out that isotopic enrichment (delta d) was calculated using measured dLS and assumed dTI.

p. 9192 – I didn't see anywhere in the manuscript where lake water budgets were calculated using 2H. It may offer some insight. If the authors decide not to use 2H for that purpose, the references to 2H in the methods section should be removed.

p. 9194, lines 13-16 – The most direct evidence for evaporation in the groundwater flowpath is the fact that a number of wells lie along the LEL.

p. 9196, line 4 – "Steady-state isotope enrichment is primarily controlled by water balance. . .." Again, I don't see any evidence that the lakes are in steady state. The ones most likely to meet this condition are the very low Itot/E lakes. It would be worth comparing the results of lakes with very low Itot/E to those with high Itot/E.

p. 9196, line 23-24 – "Introducing the assumption that the total inflow to each lake also contains a groundwater component" – I think the authors have good evidence that these lakes do have groundwater inputs. In which case, the governing assumptions used to calculate hN are not correct. The authors rightly include a sensitivity analysis of this assumption, but it should be a more fundamental part of the overall modeling effort.

p. 9196, line 26-27 – "d18O value of the total inflow -14.1 per mil". The authors show that the isotopic composition of groundwater can evolve in this landscape. Why not incorporate some of the spatial heterogeneity into assumptions about the d18O of groundwater? Or at least provide sensitivity analysis of how this assumption affects the results.

p. 9196, line 26-27 – A quick sensitivity analysis using equation 5 and five random lakes from Table 2 shows that assuming that the d18O of groundwater is -13.1 per mil instead of -14.1 per mil introduces changes ranging from 60 to 143 % in estimated Itot/E. Maybe a 1 per mil error on dTI is too high, but the authors should address this assumption more thoroughly. A slight reformulation of the LMWL & LEL lines (Figure 4) could allow the error on average recharge to be approached statistically (i.e. as std err on the intercept). Although, just looking at figures 4 and 5, I suspect that the groundwater flow system is fairly heterogeneous in its isotopic composition.

p. 9197, line 13 – "The link between MTT and the Itot/E ratio is much weaker" – I wonder if E/Itot vs. MTT would provide more insight?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 9183, 2014.