A Final Author Comment.

The manuscript HESSD-11-183-2014 “Quantifying groundwater dependence of a sub-polar lake cluster in Finland using isotope mass balance approach” by E. Isokangas et al.

We very much appreciate thorough reviews by two anonymous referees. Their insightful and comprehensive comments helped us to make numerous changes in the way how our data are interpreted, presented and discussed. Isotope mass balance calculations have been refined through including isotopic composition of local atmospheric moisture derived from isotope signal of local precipitation and through assessing the isotopic composition of total inflow individually for each of the studied lakes. Moreover, isotope mass balance calculations were run also deuterium data. The manuscript is in much better shape now. We followed large majority of the reviewer's comments and suggestions when preparing the revised version of the manuscript. In some points, however, we do maintain our views and opinions. Detailed comments addressing all questions/comments of the reviewer #1 are listed below.

Anonymous Referee #1

General comment:

1. C3857, line 1-9: *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.*

We appreciate very much this comment. Our original idea was to present a simplified, operational approach to derive the parameters characterizing the dependence of Rokua lakes on groundwater, based on the results of isotope survey made between July 29 and August 27, 2013. We have now modified the text and discussed especially the assumptions made in more detail.

1. <u>Assumption: Isotope steady-state of the studied lakes</u>

This assumption was behind all our calculations, although it was not discussed in the text. In the revised manuscript we address this problem. We think that this assumption is in fact well-justified. The reason is that natural systems such as Rokua lakes had a lot of time in the past to come up to an isotopic steady-state, reflecting their mean hydrological status and local climate. What we have observed in our study were seasonal fluctuations around these steady-state values induced mostly by seasonal ice cover. Three months following disappearance of ice cover in mid of May 2013 apparently represent sufficiently long time period to bring the lakes close enough to the steady-state values, to justify the steady-state assumption used in the calculations.

2. Assumption: Completely mixed lakes

We were aware of the problem related to the isotopic homogeneity of lakes. Therefore, for larger and/or deeper lakes several water samples were collected for isotope analyses (cf. section 3.1., lines 13-18 of the original manuscript). Deeper lakes showed expected enrichment of the shallow layer with respect to the bottom waters (0.7‰ on the average for $\delta^{18}O$). For balance calculations presented in the manuscript we used average δ values obtained on the basis of all water samples collected in the given lake including depth profiles. We consider this simplified approach justified in the light of methodology presented in the manuscript, tailored to single sampling campaign of a large number of lakes. See also comment 5. p. 9191, lines 8-11 about completely mixed lakes.

3. Assumption: Isotopic composition of the total inflow to the studied lakes

We admit that our assumption setting identical isotopic composition of the total inflow for all studied lakes was perhaps a too-simplistic approach. This was corrected in the revised version. Iterative procedure of calculating I_{TOT}/E ratios was adopted. In the first step groundwater fluxes to the lakes without visible surface inflow were calculated assuming that the isotopic composition of the total inflow equals the coordinates of the intercept of LMWL and LEL (–14.1‰, –100.0‰). In the second step, the calculations of I_{TOT}/E ratios were repeated, this time with δ_{IT} calculated individually for each lake as a flux-weighted average of local precipitation ($\delta^{18}O_P$= –14.1 ‰ and δ^2H_P = –100 ‰) and groundwater input ($\delta^{18}O_{GW}$= –13.1 ‰ and δ^2H_{GW} = –95 ‰) calculated in the preceding step. The procedure was repeated until the change of δ_{IT} was smaller than the adopted uncertainty of isotope measurements.

For lakes with visible surface inflow a slightly different approach was adopted to calculate δ_{IT}. The first step was identical as in the case of lakes without visible surface inflow. In the second step, the total outflow (groundwater plus surface water) from the upstream lake was determined as a difference $I_{TOT} – E$. Then, the isotopic composition of the total inflow to the downstream lake was determined as a weighted average of three components: (i) local precipitation, (ii) surface inflow from the upstream lake, and (iii) groundwater inflow of constant isotopic composition (–13.1 ‰). Since discharges of surface inflows were not measured, it was assumed arbitrarily that they constitute 25 % of total outflows from upstream lakes. Surface inflows determined in the previous step were assumed to have the isotopic composition of upstream lakes. The calculations were repeated until the change of δ_{IT} was smaller than the adopted uncertainty of isotope measurements.

2. C3857, lines 11-13: *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.*

Accepted. Appropriate corrections/additions were made in the revised version.

Detailed comments:

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

Has been corrected.

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

Done.

5. 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).*

Stets et al. (2010) used Priestley-Taylor method to calculate evaporation flux. In the framework of this approach the average temperature of the lake is needed. In our calculations of *E* we adopted mass transfer method which exploits the vapor-pressure gradients above the evaporating surface. Therefore, the surface water temperature is of importance here. However, we were concerned with the mixing of the studied lakes from the isotope perspective. This is the reason why we haven't even considered the mixing of the lake in this context.

6. 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?*

See assumption 1 above.

7. 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)*

Correct. In the revised manuscript we refrain from this simplifying assumption and calculate δ_A assuming isotopic equilibrium between local precipitation and atmospheric water vapor at ground level temperature for the period in question (June-August 2013).

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

Done.

9. 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.*

In the revised version we present the results of isotope balance calculations also for deuterium. The total inflow-to-evaporation ratios derived from ^{2}H-based balance turned out to be ca. 5 % higher on the average than those derived from ^{18}O-based balance. For the *G* index this difference is approximately 0.8 %. The MTT values were ca. 4.3 % higher for ^{18}O-based balance.

10. 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.*

Yes, this is true. The revised text has been modified to reflect that.

11. 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.*

See point 1 above and the revised text.

12. 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 h_N 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.*

Not relevant anymore. See point 1 above and the revised text.

13. 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.*

Corrected. In the revised version we provide also the sensitivity analysis with respect to possible changes of $\delta^{18}O$ of groundwater.

14. 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.*

In the revised version δ_{IT} is calculated individually for each lake (cf. point 1 above). Still, δ_{GW} was maintained constant (–13.1 ‰, –95 ‰), simply because we do not have sufficiently detailed knowledge of this parameter with respect to each studied lake. Sensitivity analysis included in the revised version highlights potential impact of changes of this parameter on the *G* index.

15. 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?*

Not really.

References

Stets, E. G., Winter, T. C., Rosenberry, D. O. and Striegl, R. G.: Quantification of surface water and groundwater flows to open- and closed-basin lakes in a headwaters watershed using a descriptive oxygen stable isotope model, Water Resour. Res., 46(3), n/a–n/a, doi:10.1029/2009WR007793, 2010.