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 #2 are listed below.

Anonymous Referee #2

Specific comments

Section 3.3. Evaporation estimation

1. The authors should indicate the time step used in the evaporation estimation using the mass transfer approach (hourly? daily? monthly?) and as to why this approach is chosen (eg availability of hourly data?). Normally, mass transfer approach works very well for instantaneous evaporation estimation where the dependence of vapor pressure gradient (es-ea) and wind speed in equation 1 is assumed to be nonexistent or at least low. Over long time integrated data the two parameters are interdependent and may not give good evaporation result (as in Dingman, 1994).

This is a good point. The averaging period used in the evaporation calculations was one day. This was chosen because averaging over this period does not introduce large error to the calculations (Dingman, 2008). Averaging over one month period would likely induce significant bias. As the Referee pointed out, vapour pressure differences and wind speed may correlate significantly in this case. This comment will be added to the revised manuscript.

2. What is the relation between the empirical constant n in equation 2 (which follows a power law) and the turbulent parameter 'n' implied in the Ck estimation-equation 3. Ck in the Isotope equation in this work is based on n = 0.5. For this reason the equation used in estimation of Ck needs to be elaborated (incorporating the turbulent parameter n) later in section 4.4- line 18 in addition to referring to Gonfiantini, 1986. The Ck (for d18O) values used in this work would be convincing if d2H has been used for the water budget computations using the Ck 2H proposed by Gonfiantini, 1986 and get comparable water budget estimation.

This is not the same "*n*". The term "*n*" (also "α" is used in the literature) used in eq. (2) is the friction coefficient linked to the vertical profile of wind speed within the boundary layer between the surface and the free atmosphere. In our case we calculated wind speed at 2 m height using the meteo data from 10 meters above the surface. We used in our calculations 0.15 which is the friction coefficient for grassland (Bañuelos-Ruedas et al., 2010). Friction coefficient for lakes and oceans is 0.10, but we estimated that 0.15 is more suitable for our situation since the lakes have relatively small surface areas and they are surrounded by forests which lowers the wind speed. If 0.10 is used instead, the calculated E values are ca. 8 % higher.

In the framework of the Craig-Gordon model describing isotope effects during evaporation of water the term "*n*" is introduced to quantify the impact of dynamic conditions at the interface on transport of different isotopic molecules of water through the layer of air above the water-air interface (see e.g. Craig and Gordon, 1965; Gat, 2010; Horita et al., 2008). It can be shown that kinetic fractionation associated with this transport can be expressed by the following formula:

$$\Delta\varepsilon \cong n \cdot (1 - \frac{D_i}{D})(1 - h_N) = C_k \cdot (1 - h_N)$$

Where D and D_i stand for molecular diffusivities of normal and heavy water molecules, respectively, in air, and h_N is relative humidity of the atmosphere normalized to the temperature of the evaporating water. The term "*n*" changes from 0.5 (rough evaporating surfaces, turbulent conditions) to 1 (maximum kinetic fractionation - transport accomplished only through molecular diffusion). The ratio D_i/D was measured in the laboratory (Merlivat, 1978). Wind tunnel experiments (Vogt, 1976) have shown that C_k is equal 14.2 ‰ for ^{18}O and 12.5 ‰ for ^{2}H. This corresponds to n = 0.5 i.e. rough evaporating surface and dominance of turbulent transport. Numerous field studies using isotopes to quantify lake balance have shown that these values are well suited for describing kinetic fractionation during evaporation of surface water bodies in continental settings.

Budget of Rokua lakes based on ^{2}H data is presented in the revised version of the manuscript and yields the results similar to the ^{18}O-based budget (cf. point 9 in the reply to Reviewer #1).

To avoid confusion, in the revised manuscript the term "*n*" in eq. (1) is labeled as "*θ*".

Section 4.2. Local isotopic composition

1. Please replace 'intersect' by 'intercept' in section 4.2 line 11

Done.

2. Line 16 What is the source of 'evaporation signal' and how is linked to the enrichment discussed in line 21. One major issue here is what is the role of these enriched groundwaters in feeding next downstream lakes? If this enriched groundwater enters into the next downstream lake it has a bearing on the interpretation of the I/E ratio of the next lake as this affects the _IT of the next lake downstream. To avoid this confusion the authors need to briefly provide groundwater table map (if available) or at least the geographic occupation of the different lakes and discuss if such lake interconnection exists via groundwater.

The expression "evaporation signal" and "isotope enrichment" have the same meaning.

We admit that the question of groundwater input to a given lake which seeps from upstream lake and thus is enriched isotopically, is an important issue. However we do not have sufficiently detailed observations to consider this explicitly for each individual lake. We took this into account only for lakes which have visible surface inflow originating from an upstream lake (cf. point 1 in the reply to comments of Referee #1). For the rest of the studied lakes we assumed constant isotopic composition of groundwater inflow equal the average isotope signature of groundwater measured in piezometers in the study area. The question of potential interconnections between lakes via groundwater is discussed in the revised version of the manuscript. In the revised version of the manuscript we also show the results of sensitivity study with respect to the assumed isotopic composition of groundwater inflow.

Section 4.4. Quantifying groundwater dependence of the studied lakes

1. A brief discussion about hydrology of the terminal lake Kissalampi (pond/lake) is needed so as to convince readers this lake can be used as an index terminal lake. The lake has been chosen as a terminal lake based on the assumption that it shows the highest enrichment. To substantiate this the authors need to give information about a) if the geology underneath the lake bottom allows this assumption –eg presence of clay deposits, b) if the lake occupy the lowest place in the region where all waters converge but little chance for leakage and c) compare the isotopic composition of the lake with respect to the hypothetical 'limiting isotopic composition- _ (Gonfiantini, 1986)' for the region and d) this is not a changing volume/shrinking lake during that time of the year.*

We appreciate this comment. Closer examination of the location of Kissalampi pond and calculation of the expected steady-state isotope enrichment of this lake for the case when it would indeed operate as terminal lake, revealed that the lake is in fact less enriched isotopically than expected (measured δ^{18}O: –5.6 ‰, expected δ^{18}O: –2.9 ‰). Therefore, we had to abandon this tempting idea of using Kissalampi lake data to derive h_N. Instead, we rely on meteorological information and measured temperature of the lakes in deriving this important parameter.

2. In section 4.4 line 26 reads "The lowest G value (27.8%) was obtained for Kissalampi pond, which is comparable to a terminal lake." This argument appears incorrect. The G reflect the inflow index not the outflow which governs whether a lake is terminal or not. This needs correction.

Correct, but not relevant anymore (see the previous comment).

3. Figure 8 and 9 add little value in the manuscript. If the authors claim this could add more value, elaboration is needed.

We agree. The figures were removed. The relations shown are described in the text.

Section 5. Conclusion

One or two lines of argument stating the presence or absence of any pattern in groundwater dependence by geographic position (upland, midland, center etc) may be interesting.

The groundwater dependence of the studied lakes does not follow any clear spatial pattern, since the existence of surface water outflow complicates the situation.

Other comments

Figure 3. In the line marks please use multiple of 5 in the vertical axis

Figure 3 has been modified.

Abstract- Line 3: please add 'and quantity' after 'role'

"and extent" was added.

In evaporation estimation, section 3.3, the authors used temperature of the surface part of the lake water body to estimate the saturated vapor pressure es (normalization has been applied to temperature of surface part of lake water body). In isotope section the humidity has been normalized to temperature of evaporating surface. The two (mass transfer and evaporative isotope fractionation) are based on similar theory and one temperature value should be used in both cases, in principle.

In fact the same temperature (temperature of the surface layer of the lakes) was used in both types of calculations.

References

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