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Interactive comment on "Winter atmospheric nutrients and pollutants deposition on West Sayan mountain lakes (Siberia)" *by* Daniel Diaz-de-Quijano et al.

Anonymous Referee #2

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General comments

The manuscript topic falls within the scope of BG. It presents interesting data from an unexplored region. I think it is a valuable contribution on a relevant scientific topic i.e. pollutant/nutrient deposition in remote areas and the possible effects on the ecology of mountain lakes. The results are reported in a clear way but some sections could be shortened and presented more concisely. Some more information on lake features and lake chemical data could be provided (see specific comments).

Specific comments

Lines 47-48: There is no mention here and in the manuscript of the mod-

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elled deposition estimates made by EMEP (Co-operative programme for monitoring and evaluation of the long-range transmission of air pollutants in Europe; https://www.emep.int/mscw/index.html): I would suggest the authors to consider these estimates and possibly compare them with the measured deposition deriving from their snowpack analyses. I think that s could be an added value to the paper.

Line 54: “warmed”: do the author mean subject to global warming?

Some more information could be provided on the lake sites e.g. in Tab. S2, such as lake surface area and depth, land cover. This information could help in understand the differences in nutrient levels among the lakes. Deposition is indeed a relevant but not the unique driver of nutrients lake water.

Line 122: please specify sampling depths

Lines 122-124: the authors used data from a previous lake surveys: Were sampling ad analytical methods comparable with the present study? For instance, the sampling period was slightly different in the two surveys (June-Aug in 2011-2012, Aug-Sept in 2015-2017): could this affect the differences in water chemistry between the two surveys (see comment below about Table S2)

Lines 236-237: less than 50% of TN is in the form of NO3. Because NH4 and NO2 are negligible, the remaining part is organic N, Is there an hypothesis for such a high amount of the organic part? The comparison with deposition at other remote sites (lines 216-234) could consider also the relevance of inorganic vs organic N (if these information are available for the mentioned sites e.g. Pyrenees, Alps, Sierra Nevada).

Tab. 2: It should be briefly mentioned in the table caption that “$\sim$ time” and “$\sim$ precipitation” referred to different approaches for estimated deposition, and then referred to the text for the explanation.

Lines 238-243: SO4 values are indeed quite high. The authors stated that these values are possibly overestimated because referred only to the winter period: why deposition

should be “expectably lower during summer” (line 353)? Do the authors totally exclude long-range transport form large sources, which could explain this high SO4 deposition?

Paragraph 3.3 I would suggest reorganising this paragraph and shorten it. The comparison of the deposition estimates of the present study (Tab.2) with other studies or with global deposition models could be eventually summarised in a table in the SM.

Lines 284-295: Personally, I think this paragraph does not add any useful information on the estimate of P deposition and could be skipped. As the authors said, the use of pollen is an inaccurate method for the estimate: type and coverage by vegetation, meteorological features, and other factors should be considered. Furthermore, other sources than pollen could contribute to P deposition.

Lines 300-305: I agree that a seasonality in NO3 deposition could be scarcely evident at remote sites with very low deposition rates. However, precipitation amount is probably more important at these sites in shaping the seasonal pattern of deposition.

Lines 360-361: The cited site in the Alps was an example of a remote site affected by long-range transport of air pollutants from the lowlands. Furthermore, the SCP values referred to periods of markedly high pollutant deposition (1980s-ealy 1990s). This holds for many sites, at least in Europe, where deposition of air pollutants, especially SO4, decreased significantly in the last 3 decades. I would suggest considering this temporal discrepancy when making the comparison with other sites. Conclusions: this paragraph ca be shortened too, also because the content is partly already provided in the discussion. Conclusions can be maybe provided in the form of a few concise statements summarising the main outcomes of the study and the future research needs.

Tab. 1: I would speak about “local pollution sources” more than “local perturbations”

Table S2:

- SO4 is lacking. It could be interesting to see the SO4 level in lake water, considering the quite high atmospheric input of SO4 estimated form snowpack analysis.

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- Further, there are quite sharp differences in some variables (e.g. NO3, TP) between the 2011-2012 and the present survey e.g. NO3 in Oiskoe and Raduzhnoe was markedly higher in the first survey. On the opposite, TP seem to be significantly higher in the most recent survey. Could this be due to the different sampling procedure (composite vs grab surface sampling) or to the slightly different period of the year?

- TP values are quite high, especially in Oiskoe in 2015, pointing to a mesotrophic status of the lake: is there any hypothesis for that? Deposition is discussed in the manuscript as a P input, but these values lead to hypothesised other inputs (catchment sources)

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