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Interactive comment on "Spatial distribution of environmental indicators in surface sediments of Lake Bolshoe Toko, Yakutia, Russia" *by* Boris K. Biskaborn et al.

Anson Mackay (Referee)

a.mackay@ucl.ac.uk

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General comments

This is a comprehensive study that looks at spatial variation of a number of biological, sedimentological, and isotopic indicators in a pristine lake (Lake Bolshoe Toko) in southern Siberia. Ultimately, the data will be used to inform on potential coring locations for long records, and to aid with palaeoenvironmental interpretations. The study has a number of strengths. Few studies critically consider the potential impact of coring location on the palaeolimnological indicators in the sedimentary record (although there are notable exceptions, see below). What makes this study stand out is

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the range of potential proxies / indicators that are covered. However, this also leads to potential weaknesses in the manuscript: (i) almost everything possible has been done, but (except for diatoms and chironomids) without the required reflection as to their palaeoenvironmental merits. This can be easily accommodated however. (ii) was a robust sampling strategy taken for each indicator – see comment below; (iii) I didn't see consideration of what role spatial autocorrelation may have played in the observed statistical relationships, which is likely to be important due to location of samples from the same lake. Given that this is a very interesting study, I hope that my comments help the authors to highlight more clearly which relationships might really be important.

I have made quite a few comments and spelling / grammatical corrections on the PDF. But I highlight some of the more important aspects in the specific comments below.

Specific Comments:

In the final line of the abstract, I don't know of any lakes that are not suitable for multi-proxy analyses. So I wonder if the abstract ought not end with a statement as to where the optimal coring location(s) is/are, and why.

Introduction: An assessment of lake heterogeneity has been tackled by many previous studies, e.g. I know of three done in our research group alone (e.g. Zhao et al. 2006). So a deeper consideration of what is being done here is warranted in light of studies that have gone before.

Lines 110-114: in terms of the diatom isotopes, one should also know catchment processes, such as the isotopic composition of inflows during both summer and winter. And I would suggest dissolution (Smith et al. 2016), which I'll come back to below.

Lines 147-151: great to see testable hypotheses here, but they are very vague. As phrased, you are almost certainly to find something that correlates, and we kind of know already from many studies that habitat properties will play a role in influencing bioindicators (e.g. coring from shallow or deep parts of lakes). It might be more useful

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to hypothesize about Lake Bolshoe Toko specifically?

Line 163: which is meant by a stressor in a non-impacted lake?

Study Site: Line 190-191: Would be useful to give recent productivity figures

Material and Methods:

These are all largely fine. What would be useful to know is (i) why not all 42 core tops were analysed for each proxy, ie give a rationale for looking at numbers of samples detailed for each indicator; (ii) with each proxy, state why it is being measured. For example, what are 18Odiatom values expected to reveal about the environment? Otherwise, there is the danger that the study becomes a bit descriptive.

Line 270: alkalinity?

Lines 410-412: dissolution and concentration calculations are not statistical analyses – more to section above.

Line 426: PCA may capture more variance, but do the data have a horseshoe shape when sample scores only are plotted for Axis 1 and 2? DCA is mainly done to get rid of this artefact. If this is present, then PCA is not appropriate.

Line 442-443: just a comment: given likely collinearity of many of the explanatory variables, p = 0.05 is quite high, and therefore significance easy to achieve. Might be better to consider a more robust p value of e.g. 0.005 or or even 0.001 to determine what is important (Colquhoun 2015).

Line 452: define Hill's N2 here

Line 460-461 – is this a form of PCA?

Line 465: given likely very strong autocorrelation in the spatial datasets, p = 0.05 is quite high, and therefore significance easy to achieve. Might be better to consider a more robust p value of e.g. 0.005 or or even 0.001 to determine what is important

(Colquhoun 2015). Or test significance once spatial autocorrelation has been taken into account.

Fig 2 & 3: It really would be better to show the distribution of the biological proxies using indirect ordination first of all, and then show the more advanced ordination (RDA). A biplot of samples ordered by indirect ordination, just on the basis of biological composition, can be immensely informative.

In the RDA, did you test the explanatory variables for normality before analyses? Which ones had to be transformed before the analyses? I can't see this information anywhere, yet this is very important.

Fig 5: In one sense these analyses are fine. But as autocorrelation will be so high here, I think a more robust p values is warranted, else there is a danger of getting lots of Type 1 errors

Line 534-535: Does it matter that virtually none of the samples actually lie on the GMWL? The lagoon shows signs of evaporation, but almost all the samples are below the GMWL, so are the isotopes influenced by ocean sources with high humidity?

Results: Table 1: It might be useful to show these relationships in a PCA, with variables standardised to take account of different units... Only include variables above detection limits.

Line 637: What does this index tell us?

Fig 7- 10 With all of these figures, I'm not convinced by the use of the green - red scale to represent low to high; how are scales chosen? Why do some maps have purple? It would be good to know how objective the choice of scales was.

Line 994-995: If this is the rationale for including Chrysophytes, then perhaps state this earlier. Are there any conclusions from their distribution in the lake?

Line 976-977: the authors should also consider the role that dissolution can play here,

especially for younger material, e.g. see Smith et al. 2016. DOI: 10.1002/rcm.7446

Line 1061-1062: This need not be the case. For example, in Lake Baikal, Aulacoseira species do very well under the ice, and I'm sure this could be the same for other non-shallow lakes. Eg see Jewson et al. 2009

Sections 5.4 and the conclusions are all good, but it might be also good to provide a concluding statement about potential for optimal coring location

References used in the review:

Colquhoun, D. (2015) An investigation of the false discovery rate and the misinterpretation of p-values. Royal Society Open Science. http://rsos.royalsocietypublishing.org/content/1/3/140216

Jewson, D.H., Granin, N.G., Zhdarnov, A.A., Gnatovsky, R.Y. (2009) Effect of snow depth on under-ice irradiance and growth of Aulacoseira baicalensis in Lake Baikal. Aquatic Ecology, 43, 673–679.

Smith, A.C., Leng, M.J., Swann, G.E.A., Barker, P., Mackay, A.W., Ryves, D.B., Sloane, H., Chenery, S.R.N., Hems, M. (2016) An experiment to assess the effects of diatom dissolution on oxygen isotope ratios. Rapid Communications of Mass Spectrometry 30, 293-300. DOI: 10.1002/rcm.7446

Zhao, Y., Sayer, C.D., Birks, H.H., Peglar, S.M. & Hughes, M. (2006) Spatial representation of aquatic vegetation by macrofossil and pollen remains in a small and shallow lake. Journal of Paleolimnology 35, 335-350.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-146/bg-2019-146-RC2-supplement.pdf

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