Biogeosciences
Discussions

Interactive comment on "Seasonal dynamics of organic carbon and metals in thermokarst lakes from the discontinuous permafrost zone of western Siberia" *by* R. M. Manasypov et al.

Anonymous Referee #2

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1) GENERAL COMMENTS

This paper reports on water chemical composition (mainly DOC, Fe and other major/trace elements) changes during a complete annual cycle in shallow thermokarst systems of western Siberia. Lake ice formation, affecting dissolved element concentrations and their transfer from the water column to the sediments, is also described.

The manuscript presents an impressive quantity of hydro-chemical data (11 figures + supplement material). It provides new insights into chemical element dynamics during lake-ice formation and development throughout the year. It also presents the effect of lake water residence time on its chemical composition (DOC and Fe) during the ice-free

season, especially during mid-summer. To my knowledge, this has not been reported very often (if ever) elsewhere.

One central question in this study concerns the seasonal patterns of DOC and metal concentration in thermokarst systems throughout the year. This question is important for a better understanding of high-latitude aquatic systems biogeochemistry, including the carbon cycle and greenhouse gas emissions, at the regional and global scales. This is the impression that the reader has by reading through the abstract and the introduction.

However, it appears that the sampled ponds and lakes were not the same from a season to another (Table 1, Fig. 1). Apart from 3 lakes out of $\sim$ 130, in each sampling season the sampled sites were thus different, and in very contrasting abundance (June: n = 64; August: n = 31; October: n = 31; February: n = 3). How can we talk about seasonal trends if the data do not come from the same sampling sites? Moreover, only 3 sites are presented for winter, which seems a poor number compared to other seasons.

Without further explanation on why the authors chose different lakes in different seasons (instead of coming back to the same sampling sites to be able to compare from a season to another), and without a demonstration that comparable lakes (of the same size) will show comparable hydro-chemical trends throughout the year, the inferred seasonal trends are not well supported. This should be further explained or justified.

One way to provide more information would be to display correlation lines, slopes and coefficients (R^2) on the Figures that present separate seasonal data (Figs. 2 to 6, and 10). At several places in the main text, the authors mention slopes and correlations, but do not provide such information on the graphs.

I provided many specific comments and technical corrections below, which I hope will be useful to the authors for improving the manuscript.

2) SPECIFIC COMMENTS

P= page number, L = line number.

P1977, L2-6. Except only two words ('and vegetation', L4), this sentence is identical to the first sentence of the Introduction (P1978, L10-14). One would expect the abstract to recapitulate, summarize, condense the main points of the manuscript. Not just routinely copy-paste a few sentences.

P1977, L2-3 (same as P1978, L10-11). The authors should provide a reference to support the assertion that thermokarst lakes 'extend over a territory spanning over a million km2'.

P1977, L10-12. This sentence is hard to understand. First, elements cannot 'change their concentration' (by their own), but their concentration will change because of external drivers (hydrology, climate, etc.). Second, I do not understand the last part of the sentence: 'with the lake size increase above 1000 m2 during all seasons'. Lake size increasing, I guess? And why would lake size increase during all seasons (not just during spring floods)? This is confusing, please reword.

P1977, L16. Is the observed increase 'between spring and summer' on the one hand, and 'autumn and winter' on the other hand? If yes, maybe put a comma between the two? Otherwise, the reader has the impression that the max increase is all year-round! Secondly, when is the highest increase: spring-summer, or autumn-winter? Strictly speaking, there cannot be two 'maximal' increases during the year. Please choose and amend the text.

P1977, L20-21. When reporting depths, I don't think we can use 'at the beginning'. Better to use 'near the surface', 'towards the bottom', etc. Also, at the end of the sentence: 'to the depth'... of what? Do the authors mean 'to the bottom'? Please reword.

P1977, L21-23. The sentence is confusing. 'A number of elements' showed the ac-

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cumulation? (instead of demonstrated). Also: the accumulation of what? Of the elements? If yes, please reword. It's as if there is something missing at the end of this sentence, as currently written.

P1977, L24. Do the authors mean 'implications' (instead of 'consequences')?

P1978, L1. For the non-specialist: what is a 'kDa'?

P1978, L6-8. How can a higher DOC concentration automatically mean a strong heterotrophic status and thus elevated CO2 flux to the atmosphere? This assertion, although possible, is not strongly supported by the results form this study.

P1978, L10-17 (first paragraph). The authors do not provide references here. Where do statements like 'a territory spanning over a million km2' or 'permafrost thaw being heavily intensified in Western Siberia' come from?

P1978, L24. Only in 'boreal lakes'? I would say subarctic/arctic lakes as well.

P1979, L1. Maybe I am too strict on words here, but if these shallow aquatic systems fully freeze to the bottom during winter, shouldn't we name them ponds instead of lakes? If so, these western Siberia systems would not be that particular or different from other ponds from elsewhere around the Arctic (e.g., Laurion et al., 2010; Negandhi et al., 2013).

P1979, L2-9. To my knowledge, Laurion et al. (2010) and Negandhi et al. (2013) do not report on thermokarst lakes, but rather on shallow ponds (< 2 m deep, mostly ∼ 1 m deep) that do freeze to the bottom during winter.

P1979, L9-13. Again, thaw ponds from Canada (Laurion et al., 2010; Negandhi et al., 2013) also lack a groundwater network because they are either overlying impermeable silty clays (subarctic ponds) or located in the continuous permafrost zone (arctic ponds). So these systems are also connected to the hydrological network via surface flows only, and thus the 'uniqueness' of western Siberia systems might not be true in that regard.

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P1979, L17-24. The specific questions are clearly mentioned and interesting, but what about the general question(s), the main goal(s) of the study? The reader feels that it is referred to in the next paragraph (P1979, L25 to P1980, L4), but not explicitly stated. Please clearly mention the general objective(s) of this study.

P1980, L5. Why specify 'analytical and statistical methods'? If so, the authors should also add 'sampling' methods. Why not just 'methods', in general?

P1980, L12. What are 'normal precipitation/evaporation conditions'? What are the variations during drier or wetter years? (in terms of mm of precipitation/evaporation, for example).

P1980, L19-22. We understand that precipitation and evapotranspiration data are provided by the nearby meteo station, but what about 'the annual runoff of the territory? Where do these data come from? There should be references for that. And finally, I am guessing that lake water volumes were calculated from the depth mapping (echosounder)? If yes, please make it clear.

P1980, L23. From the 'PVC' what? PVC tube, container, platform? This is not clear.

P1980, L24. Again, if we talk about small and shallow water bodies, shouldn't we refer to them as ponds? (see my comment above, P1979, L1).

P1981, L19. [...] except for B and P (30 %)'. Were these elements discarded for further analyses? If yes, please mention. B is still in Table 3.

P1981, L28. 'The ice of the lake water column'... Do the authors simply mean 'lake ice'?

P1981, L29. Ice cores were cut using a Ti saw. How could contamination of samples by Ti be avoided? Ti appears in Table 3.

P1982, L22-25. Where can we see the data? These do not appear in Table 1 or Fig. 1, mentioned earlier in the paragraph.

P1983, L1-9. Same comment as just above: where can we see the data? A figure or a table, with the mentioned correlations, would help in following the text.

P1983, L10-11. As mentioned above and below, if all the lakes (except 3, labelled with * in Table 1) were sampled during different seasons, then such interpretation is not fully supported. For example, in Figure 2 the DOC concentration shows a maximum for micro-depressions (< 1 m2) in spring, but the same concentration was not measured in these ponds during the rest of the year. Indeed, we might expect DOC in small ponds to be even more concentrated in summer and autumn, which would strongly modify the general trends of Fig. 2 (and the other mentioned relationships). Same for the maximum at 1000-10000 m2 surface areas: how can we know that DOC concentrations were not higher during other seasons for these particular lakes?

P1983, L11-14. This sentence apparently refers to Fig. 3, not Fig. 2. This is a bit confusing with the preceding sentence.

P1983, L12. The slope or the values (y-axis) on the graph? It would help to have such slopes/lines on the graph.

P1983, L14-16. Again, such seasonal trends are inferred from data that were not obtained in the same lakes (except 3 lakes out of 130). Moreover, there are only 3 samples from the winter campaign, so it would be better to remove this season from the whole year.

P1983, L21-22. Again, since the pH was not measured in the same lakes during all seasons, this statement is not supported by the data, unless the authors can demonstrate that comparable lakes of comparable area show comparable pH values throughout the year. Such demonstration is not provided in the manuscript as currently written.

P1983, L22. The small-size lakes were sampled ONLY in spring, according to the figure. So why mention the season here?

P1985, L9-12. This is interesting, but it's not presented in the figures. Where can we

find these data?

P1985, L11-12. What do the authors mean by 'some redox stratification'? A drop in O2 concentration? Fluctuations in the Eh? Please be more specific.

P1985, L26-27. This is important information that explains why only 3 lakes were sampled for the winter season. It should appear in the methods section, otherwise the reader assumes that all the lakes could be sampled throughout the whole year.

P1986, L1-11. This paragraph is really interesting, but it is valid for only 3 lakes out of the ∼ 130 lakes sampled. That should be acknowledged in the discussion.

P1986, L18-10. Where do these results come from? I might have missed something, but I don't think XRD was mentioned in the methods.

P1987, L20-21. This is a long list. Report as a table or remove this sentence. Some examples are provided in Fig. 9 anyway.

P1988, L8-14. I don't fully understand the relevance of this paragraph. What does it bring to further understand the data?

P1988, L15-22. This paragraph is more Discussion than Results material.

P1990, L10. Winter values are for only 3 lakes. This should be acknowledged.

P1992, L8-9. Where do these results come from? Was this mentioned in the methods?

P1992, L14-16. This is interesting, but it should be supported by data or a reference.

P1994, L25. Leaching of DOM from plant litter is very fast. How fast? Hours, days, weeks?

P1995, L12. The title is hard to follow. Maybe a shorter title would be more efficient.

P1996, L4-15. This paragraph is interesting. I am wondering if the authors have explored other mechanisms of CO2 enrichment during spring melt (CH4 oxidation, for example?). Also, what local conditions in/around these shallow ponds and lakes would

preclude CO2 build-up under ice, as observed in deeper boreal lakes (Karlsson et al., 2013)?

P1996, L17-24. Again, the text as written suggests that each individual lake was sampled 4 times during the year, which is not the case (only 3 lakes out of 130 were sampled in each season). So this first conclusion, although interesting 'per se', is not clearly supported by the results. The authors state that in spring, there is a clear correlation between chemical concentrations and lake sizes, but this is the only season when lakes of all sizes have been sampled. Who knows what the results for other seasons would have been if small lakes had been sampled in summer, autumn and winter? The lack of repeated sampling in the same lakes during successive seasons weakens this part of the conclusions. Maybe this can be explained by logistical reasons, but the authors do not give them.

P1997, L7-11. To my knowledge, this is the first time that ice formation and related changes in the composition of many chemical elements via pressure cracks is reported. Really interesting, and as far as I can tell, supported by the data (Figure 7).

P1997, L12-14. Were these 'macroscopic and organic- and Fe-rich amorphous particles' observed in the lake bottom sediments?

P1998, L2-3. This is interesting, but speculative. Can the authors provide any support for that (measurements, reference)? That would make the statement substantially stronger.

P2003-2004 (Table 1). I don't fully understand here. The 'June lakes' are labelled from Z-43 to Z-107; the August lakes, from Z-1 to Z-31; the October lakes, from OZ-1 to OZ-31, and the February lakes, from LF1 to LFS. So they are all different lakes? Besides the only three lakes labelled with an asterisk (Z-96, Z-2, and OZ-17), do I understand that the other lakes were not sampled during the 4 sampling seasons? If so, then how can we compare seasonal trends if the data don't come from the same lakes? Also, I guess that 'R' (μS cm-1) stands for conductivity? Please specify. In fact, all acronyms

and letters (column headings) should be defined somewhere.

P2007 (Figure 1). For people not familiar with western Siberia, it would be helpful to provide a map of the regional context of the study area, showing other northern regions (at least Siberia as a whole). Also, the figure resolution is not high enough to distinguish individual lakes. Maybe a higher-resolution file was submitted to the journal?

P2009 (Figure 3). I don't really see increasing UV280/DOC here. Maybe the authors could add regression lines (for each season) on the graph?

P2010 (Figure 4). Is this figure absolutely necessary? (or could be included as Supplement)

P2011 (Figure 5). Please display the correlation lines, with their slope and R2 values. Otherwise, except for summer we don't really see 'increase in lake water pH with the increase in the lake size'.

P2012 (Figure 6). Is this figure absolutely necessary? (or could be included as Supplement)

P2013 (Figure 7). I presume 'LF-1' and 'LF-5' refer to sampled lakes?

3) TECHNICAL CORRECTIONS

General comment for the whole text: ice and snow melt. Permafrost thaws.

P= page number, L = line number.

P1977, L7. Suggestion: Change 'of the size range' for 'ranging in size'.

P1977, L10 and the following (abstract results). Please choose between past or present verbs, not both.

P1978, L2. Suggestion: 'oxyhydroxide' (without the parentheses).

P1978, L18. 'In contrast to THE relatively good understanding'. . .

P1978, L25. When referring to ice = melt(ing). When referring to permafrost = thaw(ing).

P1979, L6. North of CANADA (not just Quebec).

P1979, L8. 'freeze TO the bottom'.

P1979, L17. 'seasons': June, August, October and February (colon instead of a comma).

P1979, L20. 'a broad range of LAKE SIZES'.

P1979, L29. 'and metal micronutrient CONCENTRATION' in these. . .

P1980, L6-9. This sentence is really long and hard to follow until the end. Maybe split it in two separate sentences would help.

P1980, L10. All the lakes [. . .] ARE located. . . (present might be more appropriate here).

P1981, L11. I guess the authors mean 'with an uncertainty LOWER than 3 %?

P1982, L2. 'In three instances'. . . Not clear. In 3 locations? 3 depths? 3 replicates? Please clarify.

P1982, L3. 'was collected' (instead of could be collected).

P1982, L14. 'each zone utilized a different number of samples'. Not clear. Each zone contained? Used? Please clarify.

P1982, L17. Remove 'the' (= 'Effect of lake size'. . .).

P1989, L10-11. 'The highest concentration of DOC observed in spring in < 10 m2 depressions'. I guess the authors refer to Figure 3 for that statement? If so, add a mention to Fig. 3.

P1989, L13. Snow and ice melt.

P1989, L14. '. . . a trend IN DOC and metal concentrations'. . .

P1989, L16. Replace 'thaw snow' by 'snowmelt'.

P1989, L22. Replace 'this' by 'the' (the source of the solutes).

P1992, L5. Replace 'thaw' by 'melting'.

P1993, L18. Add a colon after period (= '. . .open water period: photo- and bio-degradation').

P1995, L14. Replace 'demonstrates' by 'shows'.

P1995, L18. Remove 'by thaw snow' (= '. . .vegetation leaching occurring right after the snowmelt').

P2004 (Table 1, continued). The last lake is labelled 'LFS', but from Figure 7 I guess it should be labelled 'LF5'.

P2018 (Figure 10). A 'separation line' at 8 months is mentioned in the text (P1989, L1). Such a (vertical) line could be drawn on the graphs.