

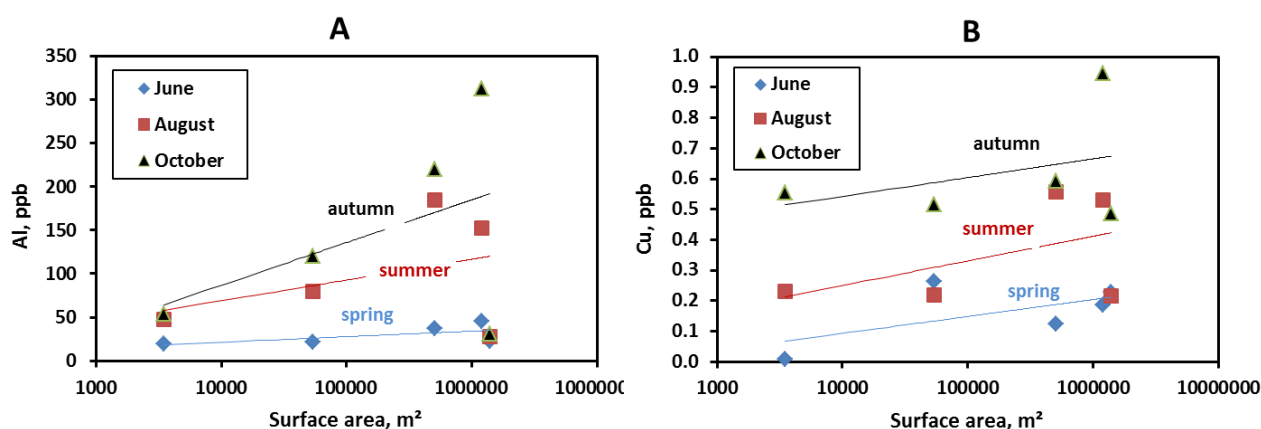
## Responses to Reviewer No 2

*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 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.*

Highly variable number of sampled lakes during each season was basically due to the difficulties in sampling logistics, and the access to the site. Only five lakes (now labelled A, B, C, D, E in the Table ESM) could be collected during all three open water seasons. In winter, we could not sample more than 3 lakes (both ice core and bottom water), because out of  $> 10$  lakes we drilled in February, only 3 had any water at the bottom. All small lakes ( $< 1000 \text{ m}^2$ ) were frozen solid in October (autumn period). We did not focus in this work on small size ( $< 500 \text{ m}^2$ ) lakes in summer, because the small water objects were thoroughly studied in our previous work (Shirokova et al., 2013). Besides, many small ponds ( $10\text{-}100 \text{ m}^2$ ) were dried in August 2013. For this reason, the size range of the sampled lakes is different among seasons.

In addition to particular features of these shallow water bodies, the main reason why we discuss full data set is that we demonstrate, using rigorous statistical treatment, that the effect of the lake size on all measured chemical parameters is absent ( $p > 0.05$ ). In other words, given very similar depth of these shallow, non-stratified water bodies, all located in the same watershed divide, they are chemically indistinguishable. The seasonal trends shown in Fig. 9 of the manuscript are based on rigorous statistics of many lakes (except winter) and as such can be considered as highly representative for the territory.

To further illustrate the validity of our approach, we plotted in **Figure R1** below two selected elements, also present in Fig. 9 of the ms, Al and Cu, for the same 5 lakes sampled in spring, summer and autumn. For a large range of lake surface area, the trend spring  $<$  summer  $<$  autumn is clearly visible and statistically significant ( $p < 0.05$ ). The same treatment can be performed for all other dissolved components; however we believe that only simultaneous treatment of all lakes provides most reliable results.



**Figure R1.** Al (A) and Cu (B) concentration in five lakes of various size. Each lake was sampled three times during the year (June, August and October). The symbols are the measured values and the solid lines are the log fit to the data. The difference in element concentration between 3 seasons (spring < summer < autumn) is statistically significant at  $p < 0.05$ .

*One way to provide more information would be to display correlation lines, slopes and coefficients (R2) 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.*

In response to this comment we provided **Table R1** below that lists all the equations and correlation coefficients for the data shown in Figs 2 to 6 and 10 of the ms. In the revised version of the paper, we will include this information either in the graphs or as Supplementary Material.

**Table R1.** Equations, regressions and coefficients to the data shown in Figs 2-6 and Fig.10 of the manuscript.

Figure No	Variables	Season	Equation
2	DOC, ppm/ Lake surface area, m <sup>2</sup>	Spring	$y = 24.609x^{-0.066}$ $R^2 = 0.4952$
		Summer	$y = 57.095x^{-0.116}$ $R^2 = 0.3271$
		Autumn	$y = 48.106x^{-0.073}$ $R^2 = 0.177$
		Winter	$y = 0.4785x^{0.2984}$ $R^2 = 0.6807$
3	UV <sub>280</sub> /[DOC]/ Lake surface area, m <sup>2</sup>	Spring	$y = 0.0251x^{-0.007}$ $R^2 = 0.0267$
		Summer	$y = 0.0284x^{0.0014}$ $R^2 = 0.0002$
		Autumn	$y = 0.0301x^{0.0135}$ $R^2 = 0.0163$
		Winter	$y = 0.0005x^{0.3178}$ $R^2 = 0.9896$
4A	Ca, ppb/ Lake surface area, m <sup>2</sup>	Spring	$y = 206.82x^{-0.028}$ $R^2 = 0.0447$
		Summer	$y = 184.92x^{0.0412}$ $R^2 = 0.0326$
		Autumn	$y = 297.31x^{0.0109}$ $R^2 = 0.0015$
		Winter	$y = 0.6376x^{0.5271}$ $R^2 = 0.37$
4B	Si, ppb/ Lake surface area, m <sup>2</sup>	Spring	$y = 106.48x^{-0.055}$ $R^2 = 0.0864$
		Summer	$y = 85.569x^{0.1047}$ $R^2 = 0.109$
		Autumn	$y = 393.67x^{-0.031}$ $R^2 = 0.0054$
		Winter	$y = 1.3992x^{0.459}$ $R^2 = 0.5696$

5	pH/ Lake surface area, m <sup>2</sup>	Spring	$y = 4.0403x^{0.0067}$ <b>R<sup>2</sup> = 0.0965</b>
		Summer	$y = 3.846x^{0.0254}$ <b>R<sup>2</sup> = 0.3087</b>
		Autumn	$y = 3.0401x^{0.0374}$ <b>R<sup>2</sup> = 0.4421</b>
		Winter	<b>N.D.</b>
6A	Fe, ppb/ DOC, ppm	Spring	$y = 41.808x^{0.2403}$ <b>R<sup>2</sup> = 0.0195</b>
		Summer	$y = 23.408x^{0.7182}$ <b>R<sup>2</sup> = 0.311</b>
		Autumn	$y = 199.48x^{0.0383}$ <b>R<sup>2</sup> = 0.0013</b>
		Winter	$y = 20.462x^{0.9299}$ <b>R<sup>2</sup> = 0.7804</b>
6B	As, ppb/ Fe, ppb	Spring	$y = 0.2476x^{0.1053}$ <b>R<sup>2</sup> = 0.0872</b>
		Summer	$y = 0.1255x^{0.2998}$ <b>R<sup>2</sup> = 0.5305</b>
		Autumn	$y = 0.0567x^{0.4327}$ <b>R<sup>2</sup> = 0.3301</b>
		Winter	$y = 0.0003x^{1.2984}$ <b>R<sup>2</sup> = 0.99</b>
6C	Cd, ppb/ DOC, ppm	Spring	$y = 3E-05x^{1.9488}$ <b>R<sup>2</sup> = 0.2105</b>
		Summer	$y = 0.0003x^{1.3096}$ <b>R<sup>2</sup> = 0.4001</b>
		Autumn	$y = 0.0071x^{0.3962}$ <b>R<sup>2</sup> = 0.0863</b>
		Winter	$y = 0.034x^{0.0428}$ <b>R<sup>2</sup> = 0.0026</b>
10A	DOC, ppm/ Residence time, years	Spring	$y = 9.4848x^{-0.589}$ <b>R<sup>2</sup> = 0.2374</b>
		Summer	$y = 14.774x^{-0.859}$ <b>R<sup>2</sup> = 0.3856</b>
		Autumn	$y = 15.425x^{-0.413}$ <b>R<sup>2</sup> = 0.2928</b>
10B	Fe, ppb/ Residence time, years	Spring	$y = 73.814x^{-0.037}$ <b>R<sup>2</sup> = 0.0006</b>
		Summer	$y = 160.03x^{-0.448}$ <b>R<sup>2</sup> = 0.086</b>
		Autumn	$y = 198.79x^{-0.421}$ <b>R<sup>2</sup> = 0.1799</b>

## Responses to specific comments of reviewer No 2

*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.* We agree. The first sentence of the revised abstract will be the following "Despite relatively good knowledge of biogeochemistry of Siberian thermokarst lakes during summer base flow, their seasonal dynamics remains totally unknown."

*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 km<sup>2</sup>'. We corrected this sentence as "Western and central Siberia's thermokarst (thaw) lakes extend over a territory spanning over a million km<sup>2</sup> (half of Western Siberia Lowland, 0.5 million km<sup>2</sup> and all North Siberian Lowland, 0.84 million km<sup>2</sup>)"*

*P1977, L10-12. This sentence is hard to understand.* We revised as: "The lakes larger than 1000 m<sup>2</sup> did not exhibit any statistically significant control of the lake size on Dissolved Organic Carbon (DOC), the major and trace element concentrations over three major open water seasons."

*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?* We agree and completely revised this sentence as following: "The concentration of most trace elements (TE) increased in the order spring > summer > autumn > winter."

*P1977, L20-23. 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.* We revised these two sentences as following: "As a result, the DOC and metal (Mn, Fe, Ni, Cu, Zn, As, Ba and Pb) concentrations were the highest near the surface of the ice column (0 to 20 cm) and decreased by a factor of 2 towards the bottom."

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

*P1978, L1. For the non-specialist: what is a 'kDa'?* Low molecular weight here means < 1 kDa, or < 1-2 nm.

*P1978, L6-8. How can a higher DOC concentration automatically mean a strong heterotrophic status and thus elevated CO<sub>2</sub> flux to the atmosphere? This assertion, although possible, is not strongly supported by the results from this study. In this part of the text, we do not claim the elevated flux of CO<sub>2</sub> to the atmosphere from thermokarst lakes. Arctic thermokarst lakes may as well absorb CO<sub>2</sub>. Later in the text we do discuss the heterotrophic status of lakes from the Khanymey test site of this study and we reference our previous work in this region (Shirokova et al., 2013, Biogeochemistry).*

*P1978, L10-17 (first paragraph). The authors do not provide references here. Where do statements like 'a territory spanning over a million km<sup>2</sup>' or 'permafrost thaw being heavily intensified in Western Siberia' come from? We revised the sentence on geographic coverage with numbers taken from the Encyclopedia which do not require specific reference. We did add the reference to works of Frey and Smith (2005).*

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

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? Yes, these water objects could be called ponds. However, for consistency with previous works, we would like to maintain the use of term "lakes" in this study. We do agree that a 10 m large, 40 cm deep water object is hard to call "lake" and thus prefer a more general name "water body".

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.

This is only partially true. Our point here is that 0.5 to 1.5 m deep, non-stratified western Siberian lakes having  $4 \leq \text{pH} \leq 6$  and  $10 \leq \text{DOC} \leq 40$  mg/L are still different from much less acidic, less organic and most importantly, redox stratified,  $2 \pm 1$  m deep thermokarst ponds of Quebec and NW Territories, despite the fact that both type of lakes freeze solid in winter. In most recent compilation of studied circumpolar ponds (Rautio et al., 2011, *Ecoscience*), only one region among 16 (thaw pond of Boniface, Quebec) exhibits a pH of  $5.4 \pm 0.6$ , Cond. of  $18 \mu\text{S cm}^{-1}$  and a DOC of  $13.4 \pm 4.7$  which is comparable to western Siberia water bodies (Table 3).

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.

We agree with this remark and modified the text as following: "Another important difference of thermokarst western Siberia lakes from well-studied river delta / river valley lakes is the lack of connection to the underground network in the formers."

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. Our primary goal was to better understand the thermokarst lake biogeochemical functioning which should allow to constrain the impact of lake water metal and carbon cycling on river water composition and greenhouse gas exchange with the atmosphere in the course of year. On a larger perspective, we aimed at the understanding seasonal pattern of dissolved organic carbon and metal micronutrients in these shallow but highly abundant water bodies, different from previously studied glacial and deep thermokarst/yedoma lakes. This knowledge should allow predictions of phytoplankton activity, sedimentation and microbial respiration on the annual scale, necessary for evaluation of the net ecosystem exchange under various climate change scenarios.

P1980, L5. Why specify 'analytical and statistical methods'? If so, the authors should also add 'sampling' methods. Why not just 'methods', in general? Agree and corrected accordingly: 2. Study site description, sampling, analytical and statistical methods.

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). The

normal precipitation in this region is close to 450 mm. During dry summer, the lakes can decrease their depth two-fold (Pokrovsky et al., 2013, Biogeosciences).

*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.*

The annual runoff of the territory is between 200 and 250 mm (Novikov et al., 2009 and Frey et al., 2007).

*And finally, I am guessing that lake water volumes were calculated from the depth mapping (echosounder)? If yes, please make it clear.*

Yes, a GPS-echosounder was used for the depth mapping.

*P1980, L23. From the ‘PVC’ what? PVC tube, container, platform? This is not clear. Water samples were collected from the PVC boat for large lakes; we corrected accordingly.*

*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). Agree and added a term “ponds” in this line.*

*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. While P was discarded for further analyses, B concentrations in most lakes are a factor of 3 to 7 higher than those in the SLSRS-5 and thus were retained for presentation. In addition to Agilent ICP MS, the Element XR ICP-MS measurement allowed to better assess the B concentration.*

*P1981, L28. ‘The ice of the lake water column’: : : Do the authors simply mean ‘lake ice’? Yes, corrected accordingly.*

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

Note that Ti is covered (passivated) by highly inert TiO<sub>2</sub> (rutile) oxidized layer which is extremely stable at low temperatures, whose solubility in the water film formed during drilling is several orders of magnitude lower than the Ti concentration we measured in the samples. Ti pressure devise is widely used for extracting interstitial soil solutions for TE analysis (Pokrovsky et al., 2006, GCA). In this study, before and after fieldwork, blank samples were run by filling the pre-cleaned PVC container with MilliQ water and submerged Ti blades, at neutral pH and letting it to react for several hours. No detectable contamination of Ti, any major and trace elements was observed.

*P1982, L22-25. Where can we see the data? These do not appear in Table 1 or Fig. 1, mentioned earlier in the paragraph. Good point. See **Table R1** above and we will include these data in the final manuscript.*

*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. The correlation parameters are now presented in **Table R1** (see above).*

*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. Now the detailed answer can be found in **Fig R1** and a description above. Actually, five lakes of various size were sampled during all open water seasons.*

For example, in Figure 2 the DOC concentration shows a maximum for micro-depressions (< 1 m<sup>2</sup>) in spring, but the same concentration was not measured in these ponds during the rest of the year. Right, small depressions were dry in August and fully frozen in October and February.

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). Detailed study of small depressions, thaw ponds and small lakes is presented both for normal (Shirokova et al., 2013) and extremely dry (Pokrovsky et al., 2013) summers. Same for the maximum at 1000-10000 m<sup>2</sup> surface areas: how can we know that DOC concentrations were not higher during other seasons for these particular lakes?

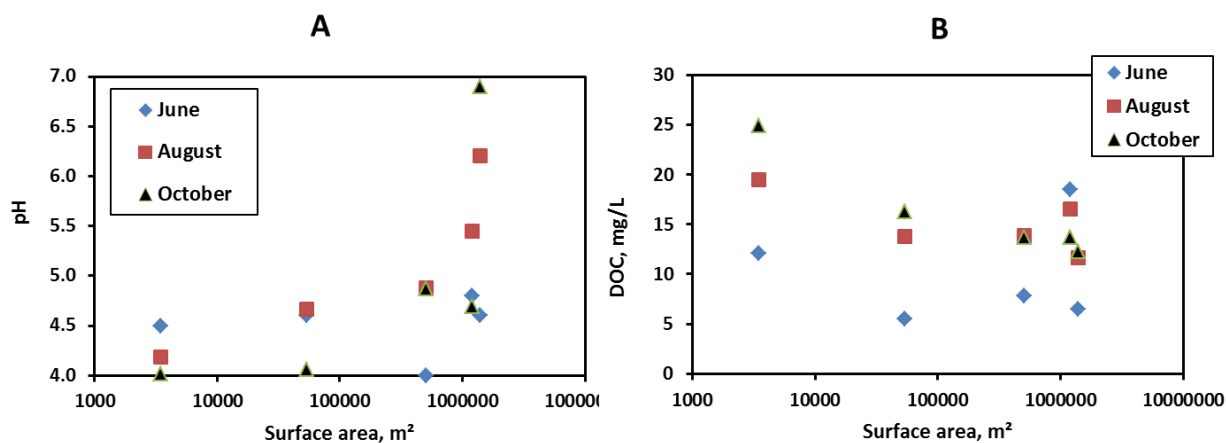
Unfortunately, these ponds were frozen during baseflow. As described in the beginning of our reply, given very similar depth of these shallow, non-stratified water bodies are all located in the same watershed divide, and they are chemically indistinguishable

P1983, L11-14. This sentence apparently refers to Fig. 3, not Fig. 2. This is a bit confusing with the preceding sentence. We agree and revised this §.

P1983, L12. The slope or the values (y-axis) on the graph? It would help to have such slopes/lines on the graph. The slopes are given now in a separate **Table R1** (see above)

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. We agree with the reviewer to remove winter data from this trend. However we believe that 30 to 60 similar lakes sampled during 3 open water seasons provide better statistics on the chemical evolution than that of 3 single data points for each individual lake. Besides, all trend described in this work on numerous lake analysis are confirmed by the data of 5 lakes sampled throughout the 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. This is now illustrated by the regression parameters listed in **Table R1**. In addition, we plotted pH and DOC concentration as a function of lake surface area for 5 selected lakes, sampled during all three open-water seasons (**Figure R2**). The trend of pH increase with the lake size, the DOC increase in small size lakes, and the overall sequence spring < summer < autumn are visible on these plots.



**Figure R2.** pH (A) and DOC (B) concentration as a function of lake surface area for 5 selected lakes, sampled during all 3 open-water seasons.

*P1983, L22. The small-size lakes were sampled ONLY in spring, according to the figure. So why mention the season here?* The pH trend is mostly pronounced for large lakes, which were sampled during all seasons (see **Figure R2** for 5 selected lakes samples during all 3 open-water seasons)

*P1985, L9-12. This is interesting, but it's not presented in the figures. Where can we find these data?* A stated in the Footnote of Table 1 of the manuscript, oxygen concentration in samples OZ-13 to OZ-31 represents surface/bottom values.

*P1985, L11-12. What do the authors mean by 'some redox stratification'? A drop in O<sub>2</sub> concentration? Fluctuations in the Eh? Please be more specific.* There was a ~50% drop in O<sub>2</sub> concentration at the sediment-water interface relative to the bottom of the ice core. These measurements were not quite reliable due to low temperatures and instability of the electrode potential in muddy bottom water. For this reason, they were not reported in Table 1.

*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.* We agree and modified the sampling section accordingly. Only 3 out of 10 sampled lakes were sampled for both ice and liquid water.

*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. We fully agree and amended the text accordingly.

*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.*

As it is stated in L 7-9 of p. 1982, "The freeze-dried precipitates were characterized by scanning electron microscopy (SEM) using a Jeol JSM840a, and by X-ray diffraction using an INEL CPS 120 CoK $\alpha$ ."

*P1987, L20-21. This is a long list. Report as a table or remove this sentence. Some examples are provided in Fig. 9 anyway.* We generally agree; however, the results of this statistically-based treatment are important to present in the main text

*P1988, L8-14. I don't fully understand the relevance of this paragraph. What does it bring to further understand the data?* This result might be empirical, but it clearly distinguishes two groups of elements: elements whose concentrations are affected by the seasons by a factor of 2 or higher and elements do not significantly increasing their concentration in summer relative to the spring seasons. For example, the increase of Si and metal micronutrients (Zn, Co, Ni, Cu, Cd, Ba and Mn) might indicate some preferential release of these elements during active plant and upper moss litter leaching in summer. High concentrations of B, Na, Mg, Ca, Cs, Pb in early spring relative to the end of summer may indicate their input with atmospheric precipitates.

*P1988, L15-22. This paragraph is more Discussion than Results material.* We believe that this information is novel and important for the reader. In fact, the small study site presented in



this work can be a good surrogate for much larger region of western Siberia (described in Manasyrov et al., 2014) in terms of seasonal evolution of thermokarst lake chemical composition.

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

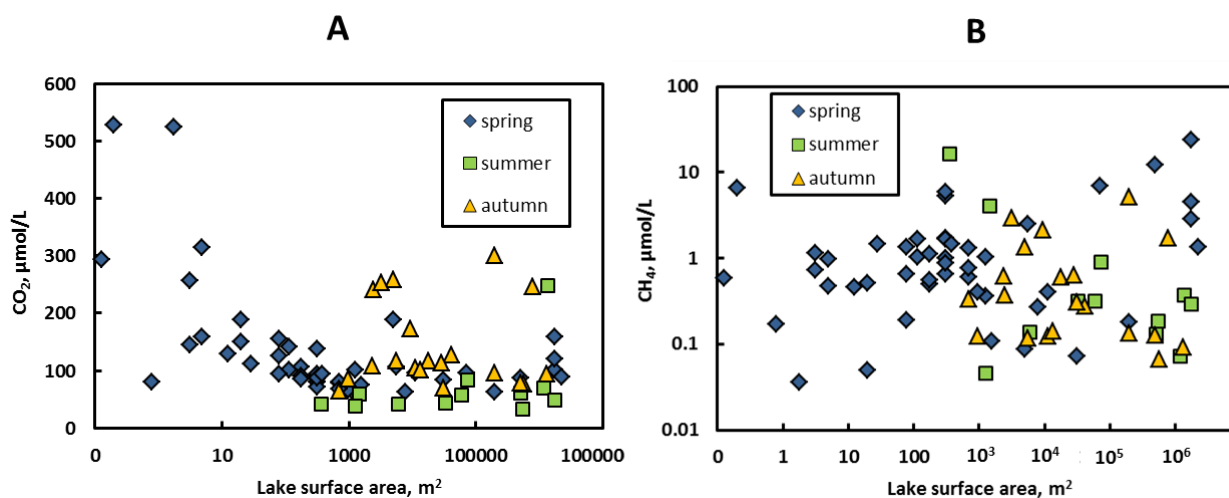
*P1992, L8-9. Where do these results come from? Was this mentioned in the methods? Good point. Here we refer to previously elaborated technique of Fe(II) analysis using dialysis membranes deployed directly in the lake water (Pokrovsky et al., 2012 Aquatic Geochemistry).*

*P1992, L14-16. This is interesting, but it should be supported by data or a reference. Analysis of bacterial activity in the ice and bottom water was beyond the scope of this work and will be a subject of another manuscript. Here, we moderated our suggestion stating the possibility of such a heterotrophic consumption. Highly non-conservative behavior of DOC during the winter time demonstrating significant depletion in February (Fig. 9 A) may suggest some heterotrophic respiration of DOM under the ice.*

*P1994, L25. Leaching of DOM from plant litter is very fast. How fast? Hours, days, weeks? The time scale is hours as confirmed by laboratory experiments.*

*P1995, L12. The title is hard to follow. Maybe a shorter title would be more efficient. We renamed this section as “4.4. Seasonal evolution of stock of carbon and TE in thermokarst lakes”*

*P1996, L4-15. This paragraph is interesting. I am wondering if the authors have explored other mechanisms of CO<sub>2</sub> enrichment during spring melt (CH<sub>4</sub> oxidation, for example?). This could be a possibility. At present, we do not know of any seasonal measurements of CO<sub>2</sub> and CH<sub>4</sub> in thermokarst water bodies of western Siberia. Our unpublished data (**Figure R3**) do not demonstrate any significant enrichment in CO<sub>2</sub> or CH<sub>4</sub> during spring flood. Only in very small depressions (< 10 m<sup>2</sup>), the CO<sub>2</sub> level does increase. Given that the concentration of methane is 1 to 2 orders of magnitude lower than that of CO<sub>2</sub> and [CH<sub>4</sub>] is independent on the depression size, we doubt the significant role of methane oxidation in CO<sub>2</sub> enrichment. This work is in progress.*



**Figure R3.** CO<sub>2</sub>(A) and CH<sub>4</sub>(B) concentration in depressions, thaw ponds and lakes of the Khanymey test site sampled in May, August and October 2013 (Pokrovsky et al., unpublished).

*Also, what local conditions in/around these shallow ponds and lakes would preclude CO<sub>2</sub> build-up under ice, as observed in deeper boreal lakes (Karlsson et al., 2013)?* The reasons for this contrast could be low volume of the thermokarst lake water and relatively short period suitable for this accumulation in western Siberia, since already in February, there is a lack of liquid water under the ice, or very low fraction (between 10 and 20%) of this unfrozen water stock.

*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?*

Our conclusion “Most of dissolved elements and organic carbon decreased their concentration following the order June < August < October, regardless of the lake size range, from 2·10<sup>2</sup> to 2·10<sup>6</sup> m<sup>2</sup>” is supported by statistics (see **Table R1**) and also confirmed in the individual lakes sample during all three open-water seasons (see **Figures R1 and R2** as examples for some components). In revised version, we will specify the season.

*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.* Now it is all explained in the first page of our reply, together with pertinent figures. Such small ponds disappear in summer due to evaporation and they are subjected to full freezing at the very beginning of the glacial seasons.

*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). We believe that this is very general mechanisms operating across hundred thousands km<sup>2</sup> of frozen lakes in western and northern Siberia. From the helicopter flight in the beginning of the winter, most lakes in western Siberia permafrost zone exhibited this yellowish color at the surface, certainly linked to cracks and DOM-rich water seeping, see **Figure R4** below*



**Figure R4.** Typical thermokarst lake surface from the helicopter in the beginning of the winter taken 300 km NW from the study site. Cracks of ice produced OM-rich water seeping.

*P1997, L12-14. Were these ‘macroscopic and organic- and Fe-rich amorphous particles’ observed in the lake bottom sediments? Presumably, yes, since the sediment is enriched in Fe (Audry et al., 2011). Analyses of sedimentary cores sampled in shallow lakes of this site in August 2014 is in progress.*

*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. We would like to avoid adding references in the Conclusion section. However, supports of this increased stocks are from seasonal observations of lake coverage in western Siberia (Zakharova et al., 2014), described in L15-25 of p. 1995 (section 4.4).*

*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? Detailed answers to these questions are given in the first § of our reply and illustrated by **Figures R1, R2**. Five lakes were sampled during all open-water seasons as now shown in revised Table 1 by A, B, C, D, and E letters.*

*Also, I guess that ‘R’ ( $\mu\text{S cm}^{-1}$ ) stands for conductivity? Please specify. In fact, all acronyms and letters (column headings) should be defined somewhere. In Table 1, R stands for specific conductivity and N.D. for non-determined.*

*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? Yes, we submitted HR image to the journal. Revised Figure 1 is given below (**Figure R5** at the end of this reply)*

*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? We listed now in **Table R1** the*

regression coefficients. The average values of  $UV_{280\text{ nm}}/[\text{DOC}]$  are equal to  $0.024\pm 0.0037$ ,  $0.030\pm 0.0072$ ,  $0.035\pm 0.0069$ , and  $0.0354\pm 0.0068$  in spring, summer, autumn and winter, respectively.

*P2010 (Figure 4). Is this figure absolutely necessary? (or could be included as Supplement)*  
We agree and will move it to the Supplement.

*P2011 (Figure 5). Please display the correlation lines, with their slope and R2 values. These values are given in **Table R1** of this reply.*

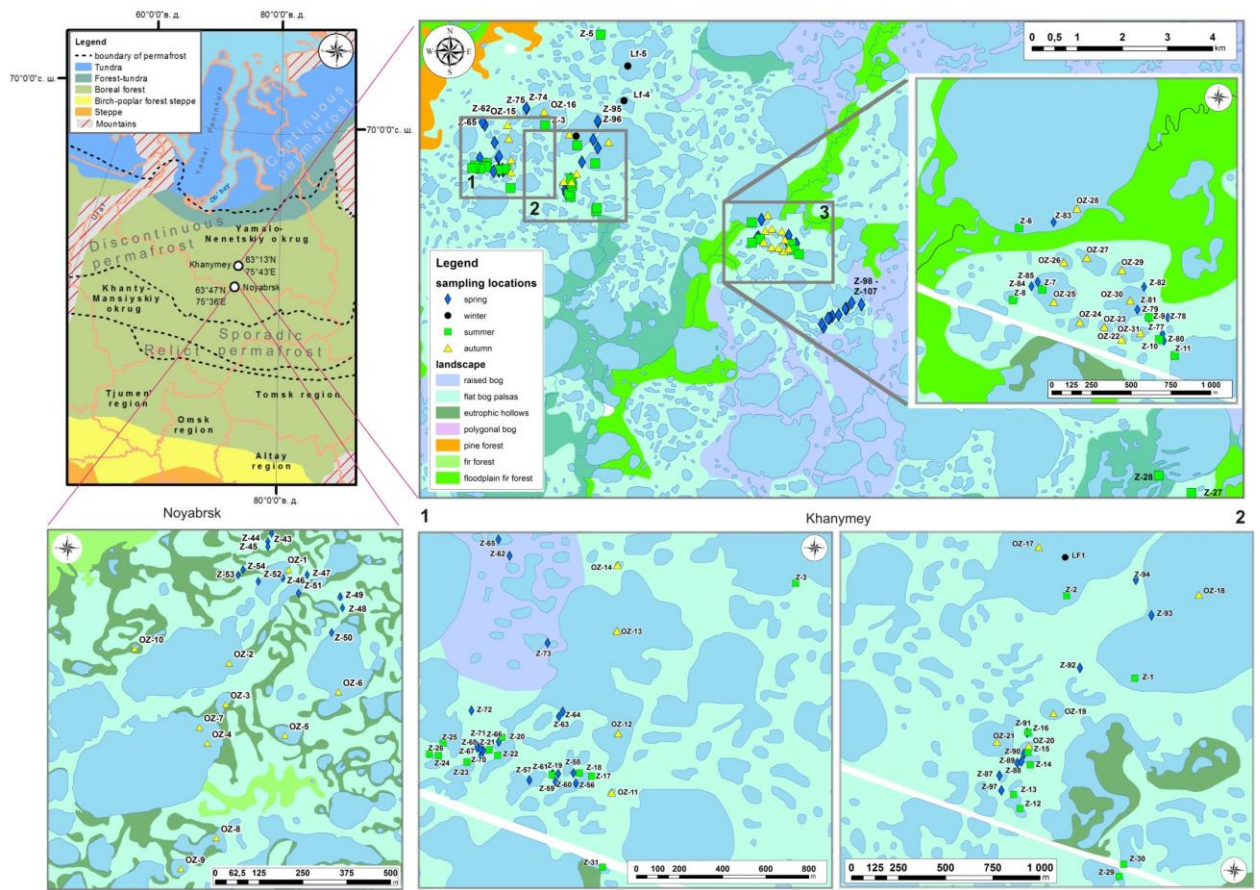
*Otherwise, except for summer we don't really see 'increase in lake water pH with the increase in the lake size'. We agree, this increase should be visible only during summer due to the increase of primary productivity and occasional underground feeding. The increase of the pH in the cycle of thermokarst lake maturation based on previous summer period observations is fairly well documented for western Siberia (Pokrovsky et al., 2011, 2014).*

*P2012 (Figure 6). Is this figure absolutely necessary? (or could be included as Supplement)*  
We agree and will move it to the Supplement.

*P2013 (Figure 7). I presume 'LF-1' and 'LF-5' refer to sampled lakes? Yes, we added this explanation in the figure caption.*

**We took into account all technical corrections of this reviewer**

*General comment for the whole text: ice and snow melt. Permafrost thaws. Thank you for this very useful remark. We edited the text accordingly*



**FIGURE R5.** Study site area with symbols showing the position of sampled lakes and small water bodies in different seasons. Different colors correspond to different elementary ecosystems.