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Interactive comment on "Effects of anomalous high temperatures on carbon dioxide, methane, dissolved organic carbon and trace element concentrations in thaw lakes in Western Siberia in 2012" *by* O. S. Pokrovsky et al.

Anonymous Referee #2

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Review of: Effects of anomalous high temperatures on carbon dioxide, methane, dissolved organic carbon and trace element concentrations in thaw lakes in Western Siberia in 2012 Authors: Pokrovsky et al. Biogeosciences Discuss MS# bg-2013-98

The authors make use of a natural warm period during the summer of 2012 and previously collected aquatic chemistry data to examine the effects of high temperature on aquatic chemistry. I find this work to be of broad interest; with some relatively minor refinements this manuscript will be suitable for publication in Biogeosciences. General comments on the manuscript, and specific comments with accompanying text citations,

are provided below.

General comments:

For a study that examines differences in water chemistry between two time periods that have distinctly different temperature regimes, I find the overall description of regional temperature means, temperatures for the two study periods, and when, exactly, sites were sampled during 2010 and 2012 to be sparse. More information than that provided in Figure 1a is required for the reader to assess the uniqueness of the heat wave event that is being documented. As outlined in the specific comments below, I would find it very helpful to have a figure that presents a long-term mean of daily temperatures in this region, at least for the summer or spring-summer-fall period, contrasted with daily temperature means for both 2010 and 2012. The period over which lakes and ponds were sampled during both 2010 and 2012 could then be overlain on this data. This would enable the reader to assess when sample collection occurred relative to the heat wave event, and gain a more nuanced understanding of the progression of the heatwave.

Following on the comment above, more information on the sites that were sampled should also be provided. Were the same sites sampled in 2010 as in 2012? If so, was this true in all cases, or only some cases? One way to show this information might be to color code Figure 1b to show sites sampled in 2010 only, 2012 only, and both years. Adding specific sample dates to Table 1 (see also comment below) would also help with this point.

Finally, although the authors do acknowledge that multiple mechanisms are at work to bring about the changes in water chemistry that they observe, I find that they could do a much better job of providing an integrated discussion along these lines. This is also touched upon in the specific comments below. For example, changes in DOC concentration could be caused by (1) evaporative concentration increasing concentrations, (2) increased microbial activity in the water column decreasing concentrations, and (3)

increased C release from permafrost increasing concentrations. It seems that all of these processes are at work, and the authors have some information that would allow them to discuss the relative importance of these three mechanisms. A more fine-tuned discussion for this, and other constituents would be useful.

Specific comments:

p 7258 ln 29: TE (trace elements) abbreviation not previously defined

Section 2.21: A more thorough description of the soil depression that were studied would be useful

Section 2.1: Greater detail on sampling times would be useful, and a description of temperatures during the two periods. Is it possible to add a figure that shows daily temperatures for the summers of 2012 and 2010, compared to the long term average, with the sampling period for the two years overlain on the temperature figure? See also general comment above.

p. 7267, ln 8-10: This strong relationship between DOC and conductivity indicates evaporative concentration as an important mechanism for the increase in DOC concentration between 2010 and 2012. Could this be referred to specifically in section 4.1?

page 7268, line 19-21: The methodology referred to in this sentence is unclear ("we used the percentage of colloidal . . .")

page 7270, line 24: A 50% decrease? This is not the same thing as a two-fold decrease.

Page 7272, lines 21-24: Is there evidence to support the claim that trace elements are limiting in this region? Typically nutrients such as P (perhaps N) would be assumed to be limiting for primary production in freshwater systems.

Page 7274, line 25-26: If what you're seeing is an increase in DOC concentration as

a result of evaporative concentration, then the total stock of DOC in the lakes is not changing. As such, it's not clear to me that the overall flux of DOC from lake to river will increase – this will depend on how the hydrology of the larger system does or does not change.

Page 7276, line 5-14: There are multiple processes at work here, and I would find a more nuanced discussion of these processes to be useful. DOC release from peat, increased heterotrophic respiration (potentially as a result of increased DOC consumption) and evaporative concentration will all affect the concentration of DOC in this system, and all seem likely to be occurring. If the two-fold increase in DOC concentration can be entirely accounted for by the decrease in lake volume (as discussed on page 7269, lines 20-25), does this mean that the increased DOC release from peat is at most equivalent to the change in heterotophic respiration?

Page 7278, lines 5-6: An increase by a factor of 5 to 10 for CO2 and CH4. This is higher than the means that you provide in the abstract and in previous sections. Are these values for individual lakes that you have measured?

Table 1: Sample dates for each site should be included in this table. Are these data for 2012, 2010, or both?

Figure 2 and others: Please provide a more complete description of the difference between the sites labelled "2010" and "2010 Khanymey". From section 2.1, it appears that all sites were in the Khanymey region?

Interactive comment on Biogeosciences Discuss., 10, 7257, 2013.