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Biogeosciences Discussions

Interactive comment on "Methane dynamics in different boreal lake types" *by* S. Juutinen et al.

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General comments:

1. First, this manuscript is based on a fantastic dataset! I have never seen anything even close to this regarding CH4 concentrations in water. Obviously I am very much in favor of publication and my comments below should not be taken as criticism. Instead the comments are primarily driven by an interest to learn as much as possible from this outstanding material and to contribute to this as much as possible. This means I may still recommend publication even if not all comments result in changes in the manuscript, but I would of course appreciate if all comments are addressed in some way so I understand how they were handled. Please let me know if some of the comments below are unclear. Overall I congratulate the authors to a very interesting

manuscript.

2. The aim to use the lake typology and lake variables used for environmental monitoring and link those to lake CH4 is valuable and definitely should be tried. However, given the outstanding dataset it would be sad to not explore if other variables or other ways to analyze the data can yield even better understanding of lake CH4 dynamics. It is not clear if this alternative analysis, focusing not on the environmental monitoring perspective but rather on extracting the maximum knowledge about CH4 from the data, have been done and some specific questions below address this.

Specific comments:

3. P3458 L9-13

Somewhat unclear if statements regard NRC and shallow lakes only or all lakes. Surface water concentrations are mentioned in two different sentences partly having the same meaning (?)

4. Section 2.4 and elsewhere

I think the estimates of the diffusive flux is uncertain. If I understand it right k was kept constant for all alkes at all times regardless of lake size. An average wind speed (U10) of 3 m/s yields a k600 of 3.46 cm/h or 0.83 m/d according to the equation 2. I fear that this is an overestimate for the smallest probably wind sheltered lakes while it is almost certainly an underestimate for the large lakes in the dataset. However, I have no better alternatives to offer at this point, but please consider (1) to clearly discuss these problems, and (2) to focus more on the storage fluxes, which are more robust and for which you obviously have the most extensive data in the world so far. Also, please point out more clearly earlier in the text that ebullition, representing a very large open water flux component was not included. This sometimes gets confusing, e.g. when expressions like "total flux"; or "annual flux" is used. For example, in section 3.4 it is not always clear that ebullition was not included.

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5. P3466 Section 2.5 and Table 4

The variables explored to explain CH4 concentrations and mentioned in the text are O2, Ptot, TOC, lake area, and depth. What other variables or combination variables were tried but not reported? Potentially several other variables could show stronger relationships with CH4 concentrations, particularly for surface water showing relatively low r2 in Table 4. See some examples below:

(a) How about mean depth (assuming that only max depth was tried; unclear in the text)?

(b) Was Ptot tried alone? It does not seem so in Table 4. In general it is unclear if Table 4 only show significant relationships or all that were tried. With such a large n many relationships with low r2 will still be significant, so is may be better to select what relationship to show based on a r2 treshold. It is also unclear to what extent the order of the variable in the stepwise analysis makes a difference or not.

(c) Figure 3 indicates that summer surface concentrations vs log lake area for all lakes should yield a much stronger relationship than relationships with depth as the primary variable, but no such relationship is presented in Table 4.

(d) Surface water CH4 during stratification periods seems to come from epilimnetic sediments and is lost by oxidation and emission. Hence it could be expected that a large epilimnetic sediment area relative to the epilimnetic volume would favour high concentrations while a large lake area resulting in a higher piston velocity should increase emissions lowering surface water concentrations. Could this be expressed by combination variables in some way? Maybe a first test could be done with a simple "area to mean depth ratio" and/or "area to volume ratio".

(e) One interesting problem with all this is that the concentrations are a result of processes occurring at different scales. For example, the volumetric concentrations of different variables used in Table 4 rely on not only what happens in the volume itself

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but also from import and export to the surroundings across surfaces. The concentrations of different variables are obviously regulated by very different processes and this may reduce the strength of relationships where the concentration of one variable (e.g. CH4) is related to concentrations of other variables (Ptot, O2, ...). One way to handle this could be to compare whole lake integrated variables. For example instead of comparing bottom concentrations of CH4 and O2 one could compare total lake storage of CH4 and O2, or whole lake average concentrations, or as previously tried anoxic volume fraction and total lake CH4 storage. Similar approaches could be tried for other variables as well.

(f) Was residence time tired as a variable? I have never seen this tried but it could be interesting given this data set. Residence time should partly affect TOC but could also be related to ground water inflow. Perhaps it can say something about possible groundwater CH4 contribution which is largely ignored in many studies.

(g) Were simple ratios between the tested variables tried. E.g. an intermediate Ptot does not necessarily mean intermediate productivity fuelling CH4 production if most P is bound to TOC and the Ptot to TOC ratio could be used to explore this.

(h) The data cover a relatively large latitude gradient. Was any climate variables such as average temperatures tried for all lakes or within the different groups?

The above are just examples of how I think and if such approaches have not been tried already, I am sure other more clever/elegant approaches can be identified with greater knowledge about the data than I have.

6. P3466 L15.

I do not understand the use of "production and oxidation". Was this measured? From my understanding concentrations were measured - not processes?

7. P3466 L31

Please, clarify what is meant with "angular transformation". Arcsin of the square root

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of x?

8. P3467 L1-10

The classification according to Ptot or bottom O2 based on environmental monitoring is interesting and fine, but could potentially also (1) result in a classification possibly being irrelevant for some aspects of the CH4 cycling (e.g. surface concentrations if lake physical properties are more important) or (2) limit the use of the findings for readers in countries where lake classifications are made in different ways. I write this comment not to criticize the classification but rather to stress the importance of the parallel efforts to find and present general results valid for all lakes.

9. P3469 L14-15

Please, consider moving the explanation about the choice of variables to the methods section (and also to consider additional variables according to above comments).

10. Results and discussion

Would it be possible to have sub-headings (e.g. summer surface concentrations, summer bottom concentrations, winter surface concentrations, and winter bottom concentrations, or equivalent to enhance readability? I frequently got confused and mixed up various parts of the text having similar statements for different seasons and depths. A conceptual table showing the strongest predictors could also help. This table would need two columns for summer and winter and rows for surface and bottom. The table text could be: Variables being related to CH4 concentrations at different depths and seasons (linear regression; r2 $>$ "selected threshold value". Plus or minus signs indicate positive and negative relationships, respectively. Details about relationships are shown in Table 4. (I can give a better example by e-mail since the web formatting does not allow me to put a table here).

11. Section 3.4 (P 3471) and Figure 5.

When there is the option to determine lake integrated flux per m2 I think this is always

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preferable relative to flux per m2 sampled water column at the deepest point. The latter is not representative for whole lakes and makes comparisons between lakes much more difficult. In the case of CH4 this is particularly important since concentrations depend on sediment surface area to whole water layer volumes, and not on what happens in just one m2 of the water column. Hence, a storage flux valid for only 1 m2 of the total lake area does not seem relevant for this excellent material enabling outstanding between lake comparisons and this is also mentioned in the discussion. Hence, I suggest that only lake integrated values should be regarded. In line with this I suggest panels A and C in figure 5 should be omitted. (At the same time I would like to thank the authors for clearly discussing these two different areal units and their difference. In far too many papers it is unclear which was used making interpretation very difficult.)

12. P3474 L4-6

If the shallow lakes were also the smallest they should have been more wind sheltered implying a lower k600 which also could contribute to higher surface concentrations.

13. P3477 L19-20

The flux information form the 8 Swedish lakes represent whole lake integrated values and should perhaps not be compared to sampling point areal values. If lower values were found for the Finish lakes this could depend on differences in lake morphometry. The Swedish lakes were likely most comparable to the smaller lakes in the Finish dataset and not to all the lakes.

14. P3477 L23-29:

The Wisconsin lakes had higher CH4 concentrations and I partly agree with the explanation regarding their small size implying large sediment area to water volume, but also think this has connections to climate. Due to a more continental climate the Wisconsin lakes warm up quickly and develop stable stratification during longer periods of the year. This results in a higher CH4 storage in the water column and hence a leager

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storage flux. On top of this the epilimnetic sediment was warmer than in Sweden (and presumably Finland) which could explain higher CH4 flux into the water and higher diffusive emissions.

15. P3478 L3-7

I am not sure that lake area integrates other variables particularly well. More likely it more directly reflects the surface area across which CH4 is exchanged. For example, lake integrated areal flux range from 1.6 to 727 mmol m-2 a-1 (stated on P3477 L15-17). This represents 2-3 orders of magnitude while lake area ranges over more than 5 orders of magnitude (e.g. fig. 6). This explains why lake area should be more important for whole lake fluxes than variables showing less between lake variability (e.g. depth, Ptot, O2 ...). However, these other variables should do a better job explaining the areal flux (flux m-2).

16. Table 1

Are there some minor errors in the table? HSm: Color 30-60? VHSh: Color as for VH?

17. Table 3

It would be interesting to know the fraction of the lakes having bottom water CH4 concentrations $>$ 2 μM. The data gives the impression that just a few lakes had very high bottom water CH4 concentrations while the majority had very low bottom water concentrations.

18. Table 4

- Does "a" denote the intercept of the relationships? - See also above comments

19. Table 5

- Are some words missing in the text explaining the table? - I cannot find the notes, a, b, and c.

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20. Table 7

"Total flux" can make the reader think all flux pathways were included. Please, clarify that estimates are based on diffusive and storage flux. Does it make any sense that flux was best correlated max depth or does depth just reflect something else that could provide a better explanation? I could not find much about this in the discussion. I also wonder if it may be better to look at diffusive and storage fluxes separately since they depend on different factors. Considering them together may result in more diffuse patterns.

21. Figure 2

Please consider having different scale for different panels to make all data visible. It would also be good to give more information of the relative sizes of diffusive and Storage fluxes, respectively. At the moment This is not easy to access from Fig 2 and the texts primarily mention that storage flux was more than 1/3 of the flux in half of the lakes. This is an important issue for how to prioritize in future efforts measuring fluxes and providing percent contribution of storage at least for each lake type would be very valuable.

22. Figure 3

How about having open symbols for clear water lakes, grey for humic and black for very humic to get a more logical connection between symbols and lake characteristics? NRC could have X symbols. How would a contour plot look like? Right now it is difficult to comprehend the density of lakes in certain areas of the graphs since the symbols may overlap.

23. Figure 4

I only had open and black symbols in my version, while grey symbols are mentioned in the figure text. There is also a peculiar tick mark on the error bars.

24. Figure 5.

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Something looks strange with the HM lakes in my copy. I can see the quartiles but no median or mean values.

25. Figure 6.

I. The series "All" and "Statistic sample" seem to overlap almost completely. Is it necessary to have them both in this figure?

II. I assume the Bastviken et al values were obtained from Table 1 in Bastviken et al 2004. I wonder about some of these values:

(a) The star at 1200 mmol m-2 a-1 looks like Lake Mendota studied by Fallon et al 1980? If so and it is preferred to keep the point in spite of being representative of a system with high anthropogenic nutrient load, I suggest mentioning the original reference.

(b) I could not identify all the points. E.g. one of the stars at about 500 mmol m-2 a-1 should be Illersjön, but where comes the other star at this level from? Why are not all 11 Wisconsin lakes for which there could be triangles present? (Where are Morris and Peter Lake?) For all of these lakes there is also the option of selecting diffusive and storage flux data only and make a corresponding star.

(c) When discussion data including ebullition (triangles) it could be mentioned that ebullition should not be taken as long term averages but rather as an indication of its potential contribution on top of diffusive flux and storage, since ebullition is highly variable over time but was based on short term measurements.

(d) I wonder if the stars that seems to correspond to the areas of Swedish lakes Mårn and Lillsjön should not be at a level of 139 - now they look like being below 100.

(e) Is Crystal Lake from Michmerhuisen & Striegl 1996 included among the stars? If so it would be fair to mention the original reference as for Fallon.

Interactive comment on Biogeosciences Discuss., 5, 3457, 2008.