

## ***Interactive comment on “Carbon dynamics in highly heterotrophic subarctic thaw ponds” by T. Roiha et al.***

**Anonymous Referee #2**

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General comments:

The paper addresses the carbon dynamics of thaw ponds. The study is motivated by the poor knowledge about these ecosystems, not at least when it comes to food web dynamics and biotic processes controlling carbon cycling, i.e. issues that this study addresses. Weak parts of the manuscript include quite poor temporal and spatial resolution (5 ponds, 2 sampling occasions, 2 points (shallow and deep) per pond). I suspect that the ability to sample with high resolution may be limited for these sites but it still implies that it is not possible to draw too many conclusions about temporal and spatial variability based on the data. Sometimes the ms do that. I also lack clear aims and hypothesis to test. Without this the study tends to be rather descriptive and unfocused. There are also unclarities in some of the methods and data treatments,

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implying that some results and conclusions may not be supported by the data. The discussion is long and I suggest the authors try to make it more focused/structured (which would be easier with more clear aims and hypothesis).

Specific comments:

11708 L2-3. This is not correct. Super saturation is no evidence of net heterotrophy. Net heterotrophy is defined as when community R exceeds GPP. Ponds could be supersaturated without being net heterotrophic. L25-26. It is unclear what you mean with ‘production of new carbon’. Please clarify. 11709 L6-8. Here and at other places. Do you need all these references? I suggest you reduce and just leave key references/examples. 11710 L28. ‘another habitat’ is unspecific. Specify that you mean hypolimnion. 11711 L2 Here you can be more specific on how C cycling will be affected. L4-6. The aim should be rewritten to better reflect the study. I think it could be more specific and also combined with hypothesis/predictions. It is difficult to demonstrate variability in space and time with only 2 sampling occasions and 2 sampling points per pond so this part should be reformulated. L8-9. ‘make an attempt’ does not sound very scientific. Reword. L14. I suggest you write that the study region is in N. Quebec, Canada. 11715 L25. I believe this technique only capture  $^{14}\text{C}$  fixed in particulate OM and not C allocated to exudates. Would be good to explain what PP (net vs gross, part vs. tot) the data represent. L11716 L3-4. It is not clear how you derived PAR. Please explain in more detail. L5. How do you know that there were no light if it was not measured? L12-The lack of algae  $\delta^{13}\text{C}$  data is a weakness. Although these data are very hard to obtain it would be good with complementing the modelling approach with some sort of uncertainty analysis to show how the results could vary with variation in  $\delta^{13}\text{C}$  algae. This could be accomplished by using literature data on the fractionation factor. L15. Provide details on method for  $\delta^{13}\text{C}$   $\text{CO}_2$ . L26. The test with variable C:chl a ratios is good. Please show the results from this test (now you just mention that there were no change). 11717. L20-22. Please provide info if/how you accounted for trophic fractionation for  $^{13}\text{C}$  in consumers? Results. There is a general lack of

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statistical support when stating that there are differences between seasons or depths. 11718. L4. Yes, it is likely that ice and snow absorbed all light but since light was not measured this is an assumption. Reformulate. 11719 L11-12. As written it is unclear if this is a general presence or if it applies for summer or winter only. Rewrite and clarify. 11720 L18. Give stats when you state something is significant different L25. '...cover most likely prevented...' 11721 L27. Add results from uncertainty analysis. This may show that there are some uncertainty around these numbers from mixing model. If so I suggest you express the resource use by the consumers less strongly than is now the case ('...clearly dominated by...'). 11722. L4-5. You do not have the detailed temporal and spatial resolution to be able to make these general statements about variability. I suggest you specifically write surface vs. deep and late winter vs. summer (or mid-summer) instead of trends over seasons and down the water column. L9. Super saturation does not imply a dominance of energy flows through heterotrophs. 11723 L9-10. How was the C accumulation calculated? There are only 2 time points, one in winter and one in summer so it is hard to understand how accumulation rates could be calculated. Also, winter concentrations are affected by freezing per se. 11724 L7. It is likely that SDOM decreased during winter but you actually do not have any time series data to show this. L9-13. This part is unclear. It is not obvious why high DOC suggest dominance of carbohydrates or protein like compounds. Also, interruption of allochthonous input plus degradation of CDOM would decrease DOC conc. Part of the increase in DOC conc. should also be explained by outfreezing during ice formation. 11725 L17-18. You should discuss the correlations between biological variables and environmental variables. 11726 L14, L20. This is somewhat confusing. You first write that BP were strongly linked to CDOM but then that DOC did not control BP. L21-23. This has already been mentioned in the discussion. 11728 L5. I assume you mean 'input' rather than 'sources' of carbon. Table 1 and 2. Could these be combined to save space? Table 2. Why is KWK23 summer surface data included twice? Table 4. Are the statistics for summer or winter data? Give n values. Figure 2. Do you need to show this figure? Fig 5. What does the error bars represent?

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