

## ***Interactive comment on “Bacterial production in subarctic peatland lakes enriched by thawing permafrost” by B. N. Deshpande et al.***

### **Anonymous Referee #2**

Received and published: 14 April 2016

Overall, I found this to be a very comprehensive paper that will contribute useful information to our understanding of the effects of permafrost thaw on nearby lakes. Below are some overarching and specific comments that I hope will be helpful for clarifying and strengthening certain aspects of the manuscript.

Overarching comments:

The authors sample numerous lake and adjacent terrestrial sites, from a series of different geographic regions. Overall, I found it hard to follow the naming conventions used in the manuscript, and found that I often had to flip back and forth with section 2.1, where the site abbreviations were laid out. I would urge the authors to consider a way that they might make their naming conventions more intuitive throughout the manuscript. Simply taking care to always refer to a site as ‘peat thermokarst’ ‘mineral

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thermokarst' or 'rock basin' (or similar), in addition to the less intuitive abbreviations would be very helpful.

In addition, I wondered about the size fractionation results, and the fact that the fractionated samples (particularly at <35 and occasionally <3 microns) often show greater values than the total fraction. Of course, this is a concern, because (unless I am misreading the treatment) the various fractions represent just a subset of the 'total' fraction. The authors do spend a fair bit of time discussing some potential methodological reasons for these results in their Discussion. However, I wonder whether it would be best to limit the presentation of data to the 'total' and < 1 micron fraction, which ends up being the focus of the discussion, anyway. These results are also more intuitive, and seem more sensible overall. Alternatively, perhaps it is the difference in counting methods (flow cytometry vs. microscopy) between the total and fractionated samples that is leading to this offset in results. In any event, I would suggest a re-consideration of the presentation of this data.

Finally, I noticed a few typos and grammatical errors in the manuscript.

Specific comments:

Line 29-31: It wasn't clear to me, in the abstract, that you were sampling three lake types – until this point the abstract addresses the peat thaw lakes only. More clarity on this point in the abstract would be helpful.

Line 33: The enrichment experiment seems to out of nowhere a bit here. Perhaps just changing the wording slightly to say "An in situ experiment . . . relative to a control treatment" would make it clear to the reader that this hadn't been previously introduced in the abstract.

Line 85: "marine snow": This is true, but marine snow is not terrestrially derived, and so the carbon quality is likely quite different from what you're investigating. Perhaps re-work the text slightly to reflect the fact that the importance of 'snow' for microbes

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in marine environments doesn't necessarily suggest that particles (and permafrost-derived particles) will be similarly important in Arctic lakes. A good reason for this study!

Line 134: Six, rather than five sites? Or perhaps I am mis-understanding the site descriptions?

Line 140: Two sets of lakes, or two lakes in the SAS valley? If two sets, then perhaps change subsequent wording to read "the SAS1 set of lakes was to the south ..." or similar.

Line 163: 0.2 m? Units are missing here.

Line 168: Could you provide more detail on how the active layer was sampled? A full core to 60 cm? Or was a simple shovel used? Understanding the depth of sampling, and method of sample acquisition would be useful.

Line 172: Define CDOM.

Line 200: It would be useful to have a description of why these depths were chosen, relative to the active layer depth of 60 cm that is stated earlier. Related to this, as I read through parts of the Discussion, I felt it would be useful to have a better understanding of how thermokarst in this regions plays out. For example – how likely is it that soils (or, leachates) from 1.25 – 2.77 m depth are going to be available for transport to nearby lakes with thermokarst in this region? Is it reasonable to expect thaw or slumping to affect soils at this depth?

Line 201: Could you provide the soil: water ratio? Was it constant?

Line 248: Bacterial community specific growth rates. You're able to calculate a slightly different growth rate here than the more typical (per cell) value that's seen more commonly in the literature, because of the availability of average cellular carbon data. I might specify the units here to clarify that your cell-normalized growth rates are expressed in a slightly different way (d<sup>-1</sup>), and also state your abbreviation (BG). I also

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notice that through the text, you sometime refer to this as ‘specific growth rates’ or ‘community growth rates’ or ‘community specific growth rates’. I think you mean the same thing by all of these? If so, it would be good to standardize the wording, or – better yet! – just consistently use “BG”. In addition, provide your BP and BA abbreviations in this paragraph, and use these consistently in the text.

Line 328-338: Do you use the Mann-Whitney test for all of these comparisons? These look like comparisons between three groups to me, which suggests an ANOVA (or, non-parametric analog) followed by post-hoc comparisons. Is it possible to add these stats to Table 3?

Line 341: “the other thaw lake types”. Would “the mineral thaw lakes” be more accurate here?

Line 339-349: Were there interaction effects for any of these parameters? This might make the interpretation a bit more nuanced. It would be useful to state the presence (or absence) of significant interactions in the analysis, and if significant, provide some interpretation here.

Line 361-364: I’m unclear on the meaning of this sentence – can it be re-worked for clarity?

Line 422: “A rich potential source” would be more appropriate, given that you haven’t measured water flow across this landscape.

Line 430: “SAS waters”: clarify that you’re referring to lake waters here, rather than soil pore waters.

Line 439: Change “this element” to N, for clarity

Line 447-448: The relatively high P could also indicate P enrichment in local rocks / till? This is also suggested by the higher P concentrations that you find at depth.

Line 459-466: I found that this paragraph could use a bit of extra text to tie these

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findings from the literature back to your own work.

Line 497 and onwards: These concerns about particle-based bacteria are certainly well-founded. It's worth considering these caveats when thinking about how to best present the fractionation data (as discussed in the overarching comments). I certainly agree with the assertion, however, that particle attachment is important in these systems.

Line 562 – 563: Lakes in this region would be more similar to your mineral thermokarst lakes, I believe.

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