

## **Response to editorial comments on bg-2015-195:**

We wish to thank the editor for her thoughtful comments. Below we have listed the comments (blue and italic) and our answers.

### **Comments on manuscript**

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*Title: "waterways" - This does not seem to include lakes and ponds... In Wikipedia, a waterway is defined as "any navigable body of water"... I was puzzled when I first started to read the ms as I thought this analysis excluded lakes but end of Intro says the opposite.*

We have changed "waterways" into "aquatic systems"

*Abstract: "shorter flow path lengths" - Shorter than what?*

We have changed "shorter" into "short". Further information on flow path lengths can be found later in the manuscript, we can unfortunately not elaborate on this in the abstract.

*Line 79 - "much" of which will be vulnerable to .. - do we already know enough to say "much" or would it be more cautious to use "part" taking into account that you use "by the end of the century"?*

We have changed "much" into "part"

*Introduction: line 129: "carbon source"*

We have removed carbon source from this sentence.

*Section 2.1: "all aquatic incubations were performed oxic" - was this always proven/followed? as even though we might think it was oxic (at start), it might have become anoxic over the course of the experiment*

Yes, all oxic incubations were regularly shaken to ensure sufficient oxygen in the incubation vials. Also, we have removed spaces, or added them where highlighted.

*Section 2.2: line 217: "using separate vials, in triplicate, for each time-step" - what was the volume recuperated from the original 30 ml, and what filtration system did you use to overcome the limited-volume problem? (did you use a GFF in a capsule with a syringe?)*

*Maybe worth specifying in the final protocole*

The volumes did not change significantly. We have added "(using pre-combusted glass filter tower units with 25 mm GF/F filters or a cleaned syringe filter assembly)" to this sentence.

We have added the "=" sign to equation (1). Thanks for noticing.

*Section 3.1, line 289-290: "after addition of an inoculum" - coming from where?*

This was different between the 14 aquatic and soil studies that were included in this synthesis. We have added "as described in the individual studies" to this sentence.

*Section 4.2: is this normal that the word "lake" does not appear much above and never again below? cause your dataset does include lake results right?*

Yes, our dataset includes lakes but they are almost all from one region in boreal Canada (Olefeldt et al., 2013). Therefore we have not discussed this further in the manuscript.

*Section 4.2.1. second sentence. could this long sentence cut into 2?*

Yes, we have split this sentence into two parts.

*And from the second sentence part, should readers understand that all results obtained from permafrost regions were on Yedoma type soils? (and grouping discontinuous and continuous pmf?)*

*cf the mention of Yedoma in the Abstract too. A confusion remains I think (and this could be clarified in Fig. 3 caption as well).*

No, not all permafrost region results were from the yedoma region. As part of the results were from the yedoma region, this could still have affected the differences in BDOC losses we observed. We have rephrased one sentence in the introduction to explain that "yedoma permafrost derived DOC ..... **contributed** to higher DOC loss" instead of "**explained** the higher DOC loss".

*Line 412, "still shows signs of pre-processing" - you mean during the frozen period?*

We mean during a combination of previous summer/shoulder seasons (where the OC was still in the soil), or during previous thaw, or during aquatic transport prior to sampling. This subsentence cites two papers, so readers can refer to those if they are interested in further information.

*Line 413-416: isn't this depending on where we take the soil sample (active layer versus permafrost)?*

This sentence presents the general results that followed from our statistical analysis. There are likely multiple factors that determine these results (as are also explained later in the manuscript), and this point raised may be one of them.

*Line 425: is it DOC that is thawing or soil which leaches DOC upon thawing?*

We have changed thawed DOC into leached DOC. Thanks for pointing this out.

*Line 425: again this Yedoma specification, telling me that only Yedoma were studied in this meta-analysis (for the permafrost data subset)*

We have added the subsentence ", representing part of our continuous permafrost soil data (Fig. 1)" to point out that yedoma data are only a subset of the continuous permafrost dataset.

*Section 4.2.2: line 457: "more rapidly" - it is still not clear to me "more rapidly" than what? - but the above result underlined does not refer to pmf vs non-pmf, does it?*

More rapidly than in discontinuous permafrost regions and in regions without permafrost, as explained in 4.2.1. We have rephrased this sentence, it now reads:

"Nevertheless, this suggests that continuous permafrost regions may release DOC that degrades more rapidly with the movement from headwaters to larger rivers in the fluvial network than DOC that is released from discontinuous permafrost regions or regions without permafrost."

*Section 4.2.3, line 468: can this be generalized, i.e. do the 3 cited studies covered all large arctic rivers? or shall you say that it was shown to be more biolabile in the 3 cited studies covering X Y Z rivers?*

These cited studies cover five rivers in total, so we have changed this into "several" Arctic rivers. We think it is not worth citing all the river names, the citations are given and the readers can look it up.

*Section 4.2.4 You may want to explore the paper by Tietjen et al. 2005 on clay – organic matter aggregates that have been found to enhance bacterial production (through photochemical degradation?) by providing a surface for attachment and concentrating DOM. (Tietjen, T., Vahatalo, A.V., and Wetzel, R.G. 2005. Effects of clay mineral turbidity on dissolved organic carbon and bacterial production. *Aquat. Sci.* 67: 51–60. doi:10.1007/s00027-004-0753-2.)*

We have added this references to this section.

*Section 4.2.4, Line 512: This is confusing; I think you might want to distinguish plant exsudates (would we say "plant-derived" then?) from OM that originates from plant materials that has been transformed into DOM in the catchment (allochthonous DOM).*

We have added "allochthonous" to this sentence, to clarify.

*Section 4.2.5 there is a bit of redundancy with what was mentioned above (notably with 4.2.2 and 4.2.3). Was this done on purpose? (to wrapup).*

Yes indeed, we wanted to use this section to emphasize the main trends once more.

*Section 4.3.2, first sentence: I guess this is the basic principle underlining the work by Cole et al. 2007 (and Tranvik et al. 2009 for lakes and reservoirs only) on global C cycle. Would these be worth citing? You also might want to mention C burial by the fluvial network? And again no mention of lake processes in this section...*

We use this sentence to highlight that river mouth sampling is "ignoring" within-watershed processing. It does follow Cole et al. etc. but our point is different. Burial is certainly a factor that is important in fluvial networks, but this is less relevant for DOC processing. Regarding lake processing: see our earlier answer discussing the scope of our study.

*Section 4.3.2, line 575-576: why gaseous losses per se? you mean at the landscape scale? harder than anything else? This start of paragraph seems strange especially as we go on reading.*

We have removed this sentence.

*Section 4.4, title: one thing I thought you did (reading the end of your intro) but now realise you did not, was to test with your own experiments several protocols. This would be powerful...*

We only tested for protocol differences regarding inoculation (with 1% or 10%) or no inoculation (see 3.2). We have added this to the end of the introduction.

*Section 4.4. first sentence - May I suggest that you come back to the ultimate goal here as well, i.e. to the use of getting BDOC accross the Arctic, considering the limitations of "simple" incubations. What will be the meaning of all this information if a whole bunch of scientists follow your recommendations? I use myself this type of experiments and I believe they are useful, but explicit considerations may be more convincing?*

Good suggestion. We do not want to repeat ourselves too much, but have woven in the goal a bit better here.

*Standardized protocol, first sentence: This is suitable for aquatic DOC only. People can face a major technical problem during this step, particularly when generating soil leachates: filter clogging!!! This can become a major drawback when you are in the field trying to start your experiments and your GFF completely clogs... Can you include a step for when this becomes an issue?*

We assumed that this potential problem would be anticipated on by future protocol users. Also, we do not see this as a problem per se, it just requires more filters for the filtration. Nevertheless, we have added a note on this in the detailed protocol in the Supplementary Information.

*Line 626: use caps with silicone or teflon septa - How do you recommend washing these?*

There are multiple ways to do this, or just use new septa. We have added "clean" to this sentence, to be a bit more specific.

*Line 645-647: This would thus be a "potential" BDOC, as such a rise in T will accelerate microbial processing (ex. by 15degC as water is commonly close to 5degC at the bottom of lakes, and in flowing rivers maybe?). So this would generate an overestimation in that sense? I know you mention below ambient T incubation as an additional step, but I do not recall you discuss this problem and significance in the paper.*

Correct, this would be a potential BDOC. Note that some Arctic streams and rivers can be well over

5 degrees in summer, for example the Kolyma and streams in her catchment may approach 15 degrees during July-August. Nevertheless, 20 degrees is warmer than ambient temperature. We have briefly elaborated on this. Incubations at ambient temperature are recommended to run as additional protocol steps, but we emphasize the importance of a standardized temperature for comparability between studies.

*Line 651: remove "most" biomass (regrowth over the course of the experiment), since as you mentioned above, this filter type leaves enough bacteria to pass (do we know anyway what proportion is passing?)*

We have added "most". To determine the proportion that is passing goes, in our opinion, beyond the scope of this study.

*Line 683: I suggest you mention that acidified samples as preserved for DOC is not adequate for CDOM optical characterization, but if filtration is done with GFF (0.7 nominal), bacterial regrowth can quickly change the optical properties, thus analyses should be done very quickly.*

I think we can assume that people that are familiar with CDOM measurements know that acidified samples cannot be used for this type of characterization. However, we have added a short sentence on sample storage times.

### **Comments to response letter**

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*Revisions based on comment #2: please consider reversing the order of the added sentence? (text about small streams before large?)*

We prefer to here first present the systems in which we observed a change (large streams and rivers), and then state we did not see it elsewhere (smaller streams and soil leachates).

*Last sentence in answer to comment #5: Not sure what you are referring to.*

We are referring to the first paragraph of section 4.1

*Revisions based on comment #8: OK, but the corrections done by authors are not yet addressing the differences between DOC in pmf vs non-pmf regions. And indicate if you mean that the unique composition of Yedoma DOC, "containing high levels of aliphatics and carbohydrates, allowing for more rapid degradation" is more rapid than non pmf DOC or non-yedoma DOC, that is, IF Abbott and Spencer studies cited do support a comparison of this.*

Abbott and Spencer et al do not specifically address the difference. We have added another sentence to clarify this: " Yedoma permafrost occupies a part of the continuous permafrost domain and its unique composition will therefore contribute to the composition of the DOC release from continuous permafrost."

*Comment #9: see comments to manuscript on this confusion; I think one might read the paper with the impression that only Yedoma soil experiments are included in the meta-analysis, but apparently this is not the case. Cf lines 439-448 in ms (just before section 4.2.2)*

We hope that our answers and edits to the editorial comments on the manuscript will alleviate this confusion.

*Answer to general comment of referee#2: Even though the paper by Laurion & Mladenov 2013 does not specifically address BDOC but the effect of light on thaw lake DOM, it discusses the implications on biodegradability. Would this be relevant citing in this part of the paper? I also think it could be cited elsewhere (Introduction, section 4.2.4, protocol; sorry for this partisanship, but there are so few studies that have looked at this behavior).*

We have added this reference to section 4.2.4.