

General comments:

The manuscript "Dissolved organic matter composition regulates microbial degradation and carbon dioxide production in pristine subarctic rivers" by Saarela et al. provides a nice comparison between DOM and CO₂ production in the a clear water vs brown water systems in the high latitude watershed. Studies linking DOM and CO₂ has become very crucial in recent years with increasing amount of greenhouse gas emission from inland waters, and this manuscript provides valuable findings on the topic. The manuscript includes a number of advanced techniques including CO₂ measurement, FT-ICR-MS, and qPCR, which combined with appropriate statistical analysis seems adequate to support the major findings. The manuscript will be valuable addition in the field of aquatic biogeochemistry and will benefit the readers of Biogeosciences. I have a few suggestions for the authors to consider before the final publication of the manuscript.

Specific comments:

1. In lines 108-109, the author mention about adding an inoculum from the surface sediment. Since river water samples are usually added as inoculum for incubation experiments, please provide a brief explanation for adding inoculum from the sediment.
2. Also, a previous meta-analysis study on BDOC measurement method (Vonk et al. 2015) reported no significant difference between the BDOC measured with or without inoculum when a 0.7 µm filter like GFF is used. The author could have avoided adding inoculum since enough microbes pass through the filter required for microbial degradation.
3. The incubation experiment for measurement of CO₂ is quite interesting. A little more details on the calculations methods or showing the actual data on a SI table would be helpful for the readers. Also, why did the author use a one-point calibration when at least two point is more usual for calibration.
4. Please add a relevant reference for the compound classes assignment criteria (line 181-184).
5. In the results, I see a lot of statistical analysis results (i.e., p values); however, I miss seeing the actual values of the major parameters particularly in comparison between seasons or water types. Including some actual values for DOC, CO₂ etc. in the result section would improve the readability of the manuscript, whereas adding the statistical results in the figure would also be helpful.
6. The discussion is well supported with references, but you may add the following reference to support the findings on lability of molecular composition (example: line 344-349). Begum et al. 2022 (<https://doi.org/10.1016/j.watres.2022.119362>).