Referee comments on "Discontinuity of the concentration and composition of dissolved organic matter at the peat-pool interface in a boreal peatland" by Antonin Prijac et al.2022

MS No.: bg-2022-71

General comments

This paper examines the difference in DOM concentration and composition between peat soil and pools, and discusses the factors leading to this discontinuity. I mostly enjoyed the paper. The design and analysis are understandable, and various methods were used to provide information on DOC composition and source. Furthermore, peatland management is currently an important topic of research, but the C cycling dynamics in boral peatland pools is still largely unknown. Thus, papers on this topic should be welcomed. However, there are some relatively major problems with the paper as it currently stands, about the methods and discussion. Some mistakes including typos should also be addressed to improve the paper quality.

Specific comments

Materials and methods:

In general I think the writing of method is a bit lengthy. While it's good to provide such information for readers who want to replicate the method, there are too many details which are not necessary to be included in the main text of the paper. I'd like to suggest the authors to refine this part in a concise manner, combining with references and supplementary materials.

In addition, as various sampling trips, analyse methods, proxy indices are used in this study, I'd like to suggest using a table or diagram to summarise this information, which would make it much easier for the readers to understand the research design and interpretation. For example, how many samples from what sites on which dates, and which were analysed for what. It'd also be helpful for the readers to understand why dot plots were used in Fig. 2.

Discussion:

1. **Difference in DOM concentration and composition**

Firstly, when comparing the DOC concentrations in porewater and pools in this study with those in other climatic zones, seasonality should be considered, as here DOC samples were collected in the growing seasons which would tend to be higher than other seasons.

Secondly, the authors only explained the good correlation between DOC:DON and 13C in porewater, but didn't try so with the pool DOC. The absence of this correlation in the pools could well lead to the discussion in 5.2, and highlights the discontinuity of DOC composition.

Lastly, while the photo-degradable DOC might have been quickly degraded in the first few days, I'm not entirely convinced that it would be the main reason. DOC from porewater was not exposed to light before being collected so there should be minimal effects from photodegradation. In pools, new DOC inputs would be expected with the increased precipitation, which was observed especially during the summer and autumn seasons. Therefore, there could be continuous supply of photo-degradable DOC during those periods of time. Furthermore, in boreal and arctic areas, the amount of sunlight is less abundant than low latitude areas, limiting the photodegradation of DOC, although I realised it would be less so in summer. Did you have any data on the light? Was any incubation conducted when it was rainy or cloudy? Did the glasses/vials filter out certain wavelengths which cause photodegradation? As it stands now I don't think there is enough evidence to make the argument that there was no photodegradation process in the samples.

2. Processes leading to the difference

In general, I think the authors can refine this section by focusing on the more likely processes with a bit more in-depth discussion. It reads like the authors were only including different reasons (e.g. hydraulic conductivity, structure of peat pores, water storage coefficient) that could possibly happen in pools and porewater, but lacking the strength to combine them together. In addition, a few arguments that the authors made were not clearly explained and need to be clarified.

Firstly, the difference in DOC composition, e.g. $SUVA_{254}$, E2:E3, could be a result of that DOC in peat soil is 'older' than those exported to water, which was not considered in the paper before thinking about the more instant changes caused by hydrological, chemical and biological processes as the authors focused on. Also, the aromatic DOC should be more hydrophobic rather than hydrophilic. If aromatic compounds are hydrophilic, it should be easier for them to flow from peat to pools leading to higher DOC aromaticity in the water, which is contrasting to what was observed. In addition, in the abstract the authors pointed out the transformation of DOC at the interface led to the production of low molecular weight compounds, which is contrasting to their suggestion that microbial-processing would cause the increase in aromatic DOC which is often larger in molecular weight.

Secondly, DOC does interact with different materials or minerals in peat. For example, the oxidation/reduction of Fe have been observed to be mediated by microbes and affect the solubility of DOC (Mladenov et al., 2010), and tend to coagulate with high molar mass DOC (Ritson et al., 2014). At the interface between peat and pools, particularly when water table is higher in peat, the change from anaerobic to aerobic environment could affect the reduction/oxidation of certain relevant minerals (e.g. Fe) and reduce mobility of certain group of DOC (Nierop et al., 2002).

Lastly, it was great to see that the authors were trying to explore the biogeochemical processes from porewater, interface and pools, which could be a highlight of this paper. However I'm not entirely sure how big role microbial processing is at the interface as the authors claimed. Indeed, biodegradation could happen within a couple of days, but at the interface I tend to think the physical and chemical interactions, e.g. precipitation and binding via the changes in the physical environment from soil to water is more instant and faster than bio-processing, and might have played a more important role. The soil C is still the dominant input for the pools despite the higher level of microbial activities in water. While the authors did a good job highlighting the difference in DOC concentration and composition, but as the paper is about the discontinuity between pools and peat, it's important to better explore how the water being transported between peat and pools (even vertically), what happens at the interface, what kind of DOC is exported and why.

Technical corrections

21: Please change "If" to "While".

39: Please delete "net".

49: What processes of organic carbon do you refer to?

77: This paper presents a study about DOC lability from boreal peatlands with porewater sampling (https://doi.org/10.1139/cjss-2019-0154), so the authors may want to change the argument that no insight about changes in DOM composition in boreal peatlands.

141: It's not clear what monitoring "among others" refers to.

178: Both UV and fluorescence are optical analyses.

3.2.2: Is it better to shorten this part and highlight the key information, as it's effectively repeating what's in each of the graphs in Fig. SI.3.

185: What calibration was conducted after observing the difference in Abs_{254}?

190: It's not clear which samples were analysed with Duetta in 2019. Just 2019 samples or both years?

185-230: the description of the method details can be simplified, and information of each index presented more systematically. It's a bit lengthy with much detailed information.

233: Can just use DOM as being introduced already. Please check throughout the manuscript.

238: I'm not fully convinced this mixing was necessary. The variability can be considered in the statistical analysis. And why did the authors only mix the porewater but not the pool samples?

214: Was there additional cover for the amber glasses to completely block out the light? Did you test the light penetration through the vials?

246: Why was the porewater samples placed at the outlet instead of inside of the wells? Was it because the authors wanted to monitor the hourly temperature? In addition, the authors didn't provide information on if there was headspace in the glasses/vials, if they were open during the incubation for gas exchange.

253: Do you mean both DOC and TN were measured, or a ratio of DOC/TN was examined directly?

Table 1 and Figure 2: Is it necessary to have both in the results, as they present mostly the same results.

Fig.2: Why were there seasons with <5 samples? In the methods, it says 6 pools in 2018, 11 in 2019, and 6 wells in 2019.

Fig.3: The negative relationship mainly existed in the porewater samples, while the correlation for the combined samples was not that good with cor = -0.53. Maybe it would make more sense to look at the relationship separately, which would help highlight the different C dynamics between the two C sources.

334: There are several cases throughout the paper saying e.g. "As for $SUVA_{254}$", or 'As for the FI". Do you mean compared to the changes observed in $SUVA_{254}$? Can you refine this please?

375: DOC:Cl does not seem to be mentioned in the method. I understand the authors may have more data than presented in the paper, but please check and avoid mistakes like this.

380: Can you include the PCA analysis for the seasonal effect, maybe in supplementary information?

Fig 4: Caption was repeating the text in the results so could be shortened. Does DOC:Cl actually refer to DOC: DON? Information on R package for ellipses is not needed here but can be in methods.

387: Could delete “Statistical tests also revealed that” and replace with “In addition, the..”.

391: Was the absence of filtered samples in August considered, as this could skew the difference between the two treatments?

412: The sentence needs some changes.

493: What do you mean by “low apparent degradation rate”?

505: Is this 136350m3 the volume of the pools in this study? While it’s small DOM degradation in these sites, what would it be if scaling up for the whole Bouleau peatland? It may not only be ‘slight effect’ if considered collectively. In addition, seasonal variation in DOC concentration and degradability could also mean that in some months, the pools may act as ‘hotspots’ for GHG emission, which would be important information for peatland management along with global warming.

515: I may suggest an alternative next step to explore the effects from pools and peat morphology on DOC transport from peat to water, as it is not so clearly assessed yet but could be important as regulating the water and DOC sources.