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Discussions

Interactive comment on "The effect of drought on dissolved organic carbon (DOC) release from peatland soil and vegetation sources" *by* Jonathan P. Ritson et al.

Jonathan P. Ritson et al.

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We thank the reviewer for their comments on our manuscript and will attempt to address these issues. Below are the main points raised by the reviewer and our responses to them. The reviewer also made a number of minor comments and technical points which we do not address here but will be happy to incorporate into the updated manuscript.

Point 1: The abstract states in line 29-30 that "more immediate effects are observed in peat soils". This is correct, but if drought events will be more frequently observed in the future, these pulses of DOC can also be regarded as a longâĂŘterm effect, in that they will be occurring more frequently, potentially giving a steady increase in DOC concentration.

Response: We agree with the reviewer's comment and will amend the manuscript to highlight this.

Point 2: It is somewhat surprising that drought effect was only observed with the mild treatment. This is explained by large variability in the other treatments, possibly because some samples became drier than intended (line 244-261). The arguments are mainly repeated in lines 359-363, but I miss a discussion of the implications of this. Do these results indicate that there is an "optimum" drought frequency for DOC release, i.e. that DOC release will not increase with increasing drought frequency and severity, but will increase to a certain point and then decline?

Response: Yes, we hypothesise that this is due to 'water scarcity limiting microbial activity (Toberman et al., 2008) and/or increased hydrophobic protection decreasing the extractable DOC on rewetting'. We would suggest that at very severe levels of drought DOC production is limited by water scarcity, however this would not stop oxygenation of peat and therefore greater potential for increased DOC production in the future due to the enzymatic latch mechanism. We will add a more detailed explanation of the implication of this finding to the amended manuscript.

Point 3: Line 423-426: Are you suggesting that drought causes permanently altered biogeochemical controls so that the released DOM becomes gradually more aromatic? The literature usually argues that more aromatic DOM is released after single drought events, but that increased frequency of these will give increased aromaticity over time. Please explain in more detail in which way you suggest your single rewetting differs from field studies and how this may have affected the results.

Response: In this section we discuss the lack of an increase in SUVA value for peat from the drought simulation, in contrast to field studies. The literature often suggests that DOC is elevated for many years after drought events. As we were monitoring a single rewetting event we suggest that one of the possible explanations for conflicting results could be that many of the longer term processes involved in increased DOC concentration and aromaticity (enzymatic latch, recovery from sulphate acidification from oxygenation) may not have had time to occur. We will explain this more clearly in the updated manuscript.

Point 4:In line 431-435 the results on both DOC and SUVA seem to be summarized. Do you consider that there was a "lack of drought effect for peat" or are you here only talking about SUVA? And again, you argue that the experiment simply investigates shortâĂŘterm effects. It is true, in the sense that only one single drought event is mimicked. But are there arguments that longâĂŘterm effects of drought go beyond the sum of many single events, that there are more permanent changes going on? This is what you indicate, but you do not explain or express it clearly.

Response: Yes, as this is in the section headed 'SUVA' we were only referring to effects on SUVA in this statement. We will clarify this in the updated manuscript. The reviewer is correct that our intention was to suggest that frequent droughts could create long term changes in peatland biogeochemistry, but that our experiment did not cover this. Again, we would be happy to clarify this in the updated manuscript.

Point 5: Line 186-192: Please explain why peat samples for this additional test were collected at a different site. And explain more clearly why this extra experiment was performed? Was it simply because in the main experiment there was no extraction prior to treatment, so you did this to look at changes over the course of the experiment?

Response: The reasoning behind performing the extra experiment was to interrogate the possibility that any oxygenation of peat could affect DOC quantity and quality and thus explain differences between the results found here and our previous work. The reviewer is correct that this could have been achieved by extracting DOC from a sub-sample prior to the start of the original experiment, however as this was not done we performed this short experiment. The samples were collected from an ombrotrophic peatland with a comparable mixture of vegetation (Juncus, Molinia, Sphagnum, Calluna, Eriophorum) and were of the same level of humification (von Post scale). Al-

though not identical to the peat collected in the original experiment, we feel these samples are similar enough to test the hypothesis that the control conditions used in the original experiment give enough oxygenation to alter DOC properties.

Point 6: Line 439-447: The discussion comes here, but it is not clear. Yes, you show that DOC removal may decline with time due to change in DOM properties, but it is not clear why this suggests that DOC removal was lower in this experiment than in Ritson et al. (2016). As far as I can see the control samples in the current experiment underwent exactly the same treatment as the peat samples in the previous experiment. Figure 4 shows DOC removal across treatments, but the results for the control group given in the supplement should be directly comparable to Figure 1 in the 2016 paper – which shows a big difference in DOC removal. I cannot see that this followâĂŘup experiment explains why there is such a big difference. This is important, as you argue (e.g. in the abstract) that DOC from peat is harder to remove, but in Ritson et al. (2016) it is the easiest to remove. Please elaborate.

Response: The confusion here lies in the experiment we are referring to in Ritson et al. (2016) as there are multiple experiments in this paper. The control group of this paper is directly comparable to the experiment entitled 'Litter decomposition in the laboratory' in the Ritson et al. 2016 paper where only data on amount of DOC extracted were presented. The comparison we were intending to make, however, is to the first experiment from the 2016 paper entitled 'Ease of DOC removal during the treatment process for different peatland sources'.

In the 2016 coagulation experiments DOC was extracted from fresh peat which had had minimal exposure to oxygen. We suggest a reason why the peat DOC in the 2017 paper showed poorer removal by coagulation was that it had been exposed to oxygen over the length of the simulation and this may have altered the treatability of the extracted DOC. The repetition of the control group conditions provides evidence for this as it shows exposure to oxygen causes a decrease in Peak C and SUVA, both of which have been correlated with ease of removal via coagulation in the literature. We will explain this in greater detail in an updated version of the manuscript and make it clear that when we say in the abstract that that peat DOC is harder to remove we mean peat that has been exposed to oxygen compared to peat which has not.

Point 7: Section 3.6: The fluorescence data are only presented in connection with coagulation. But what about difference in fluorescence properties related to drought treatment or vegetation type? Why are these results not presented and discussed?

Response: These data are available and can be included in the updated manuscript. The data suggest a drought effect on Peak C (humic-like) fluorescence for both Molinia and Juncus and an effect on Peak T (protein-like) fluorescence for Juncus.

Point 8: Line 367-369: This probably relates to the results given in table 6, but it does not fit with the lack of drought effect on peat SUVA. I suggest just briefly mentioning this here, but refer to the lack of drought effect for peat discussed in section 4.2

Response: The statistical subset of peat and Molinia had the highest SUVA values and we link this to our previous work suggesting this is likely to mean higher environmental persistence. We will rephrase this to make this clear and mention the lack of a drought effect on peat SUVA.

Point 9: Line 460-462: You could mention the drought effect on SUVA for Molinia, which may partly counteract the oxygenation effect of peat (lower aromaticity).

Response: Agreed.

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