

General comments

The manuscript «The effect of drought on dissolved organic carbon (DOC) release from peatland soil and vegetation sources» addresses relevant scientific questions related to water quality, climate change, drinking water and catchment management. The work builds onto previous work by these authors and others, but adds new aspects and brings the understanding forward. The experimental setup is adequate, although extractions also before treatment would have been beneficial (hence the add-on experiment would not have been needed). The method is usually clearly explained, and the manuscript is well structured. The figures and tables are adequate, and although the number of samples varies between figures, this is clearly explained. The data analysis is generally sound. The major objections are related to the limited explanation of the how the add-on experiment relates to the main experiment, including how the add-on experiment can explain the different results for control samples in this experiment and the previous Ritson et al. (2016) - and part of the discussion of the drought effect for peat soils, including the wider consequences of the findings. However, I find these kinds of controlled experiments valuable, and after considering carefully the points below I regard it as worth publishing.

Specific comments

A drought effect is observed in peat soil, but the interpretation could be elaborated/adjusted:

- 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 long-term effect, in that they will be occurring more frequently, potentially giving a steady increase in DOC concentration.
- 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?
- 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.
- 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 short-term effects. It is true, in the sense that only one single drought event is mimicked. But are there arguments that long-term 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.

The conditions of the control group were repeated, but this is not clearly justified/explained, and the interpretation can be questioned

- The title of section 2.5 should rather be “Repetition of the control group conditions”

- 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?
- Line 337: I would change “without any experimental treatment” to “at control conditions” (which are certainly experimental in some sense)
- Line 344-347: Delete this type of discussion text from the results chapter
- 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 follow-up 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
- Line 463-464: It is claimed that drought (oxygenation) decreases aromaticity, while the drought experiment itself did not show effects on DOM quality for peat soils. You argue why this may be so in section 4.2, but please repeat it briefly here and modify the conclusions (make them less firm).

Line 76: You may mention what kind of programmes/how Sphagnum dominance is promoted

Line 139: I assume the intervals between the rainfall simulation were the same, but please specify this

Line 152: I assume the extracts were filtered before further analysis? Please explain

Line 167: Was the coagulation performed on filtered samples? In case, please justify this

Line 207-208: Move and merge into 3.1 to avoid repeating this information here

Line 219-222: Move the more detailed explanation of the method to section 2.4

Line 230-231: Specify that you are talking about the peat soil

Line 276-278: Specify that you are talking about the Molinia samples

Section 3.4: Move the sentence in line 296-297 (on drought effects) to the beginning of the section.

Line 299-300 simply repeats line 293-294 – move and merge.

Line 308-310: Move to the discussion (section 4.2)

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?

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

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

Technical corrections

Line 139: Space between “applied” and “eleven” missing.

Line 151-2: “Kalbitz” incorrectly spelled, and both references missing in the reference list. And why is the reference not put at the end of the sentence? Is the latter part the author’s own interpretation?

Line 155: Unit misspelled, should be mgC l^{-1} .

Line 266: Delete “group two Calluna and”

Line 278: Replace “medium” with “moderate”, as this is the term used elsewhere

Line 323: Add DOC before “removal efficiency”

Line 344+line 346 and similar places: When talking about the properties of the actual molecules in question, use DOM, not DOC. DOC is just a notation for what is actually analysed and for which we can talk about changes in concentration etc, but DOC cannot be more or less aromatic or humified.

Line 433: Add “SUVA” before peat

References: Sometimes access date is added, sometimes not. In general web page and access date should not be necessary for published papers, but at least be consistent.