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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.

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We thank the reviewer for their comments on our manuscript and will attempt to address these issues. Below is a detailed response to each of the six issues the reviewer has highlighted.

Point 1: 'The study is based on the analysis of only 100 water samples for easily measureable parameters'

Response: Although it would be desirable to include as many samples as possible in the experimental design this was limited to 5 replicates per vegetation/treatment for practical considerations. Similar studies looking at decomposition have used similar,

or indeed lower, numbers of replicates. Soong et al. (2015) used three replicates per substrate in a laboratory decomposition experiment concerning DOC from fresh and pyrolysed litter. Fellman et al. (2015) also used three replicates per substrate in a litterbag study of litter decomposition whilst Cleveland et al. (2004) performed six DOC extractions per litter type and then bulked them to create three replicates. In a laboratory study on the decomposition of Calluna Vulgaris, Van Meeteren et al. (2007) used five replicates per treatment in a similar approach to our own study.

We feel five replicates, giving 100 samples in total, is a good balance between capturing natural variability in the samples and practicality given the samples filled three climate control cabinets and that manual irrigation of the samples (to ensure even wetting of the vegetation/soil) was necessary. In the subsequent analysis 200 samples were analysed for TOC and UV properties (pre- and post-coagulation) as well as 100 fluorescence samples. The coagulation experiments themselves were time consuming as commercial jar-testing apparatus is limited to six samples per run and each run takes $\sim$2hrs to perform. Although we were unable to present the data due to quality concerns resulting from instrument failure, 100 samples with two replicates (200 total) were also chlorinated, quenched and then extracted to assess disinfection by-product formation.

The reviewer notes the lack of any supporting water chemistry or litter chemistry data in the paper. We apologise for this oversight as we did measure pH of the extracts and will include these data in an updated version of the manuscript. We also measured the carbon and nitrogen content of a sub-sample of the starting soil/litter, however we did not include this in the manuscript as correlations between C:N and extractable DOC were shown in out 2016 paper in Scientific Reports. We instead referred to this paper in the discussion section. We will include the C:N data in an updated version of the manuscript. Although measuring CO2 production during the experiment would have been desirable we already acknowledge the lack of these measurements as a weakness in our ability to confirm the cause of changes in extractable DOC between drought treatments (line 363).

Point 2: The degree of desiccation after and during the 6 weeks was not measured, nor the biological status of the samples. Only for the peat samples some data on water contents at the end of irrigation (unit?) are given in line 251.

Response: The unit of the final water content is grams and is given in the text. Data on final water content for the peat soil and Sphagnum litter are available and confirm the efficacy of the irrigation treatment in causing degrees of desiccation in the treatments. This can be included in the updated manuscript.

Point 3: The different intensity of irrigation should induce different leaching rates and different DOC fluxes from the samples. No information is given on that.

Response: Unfortunately, DOC from each irrigation event was not recorded. We note in the method section that:

'Previous work has shown that the amount of water used to extract DOC and whether one extraction is performed or sequential extractions to simulate multiple rainfall events gives no significant variation in DOC quality (Don and Kalbtiz, 2005, Soong et al., 2014), only changes in the total amount of carbon'.

We would therefore suggest that any differences in DOC quality are captured by our approach. Rewetting following drought of interest as this has been highlighted in the literature (and discussed in our introduction) as a period of increased riverine DOC concentrations. One of the goals of the experiments was to ascertain whether litter layer DOC flux played a role in the increased DOC concentrations post-drought or whether this was entirely due to processes within the peat, hence our focus on rewetting.

Point 4: Following the 6 weeks of irrigation, all samples were air dried before water extraction (line 148) which does not make sense to me: If all samples were air dried before extraction, the pre irrigation to induce different degrees of desiccation seems meaningless. The rewetting of air dried soil samples cause specific effects (Birch effects) that my override the aimed irrigation effect.

Response: Air-drying was performed so that accurate estimates of extractable DOC could be determined on a mgC g-1 basis. Whilst air drying the samples after the irrigation simulations may have increased the homogeneity between the sample treatments, we feel this is likely to be minor as this occurred for approximately 2-3 days compared to a 42 day simulation. Also, during the simulation all samples will have been exposed to periods with no irrigation multiple times due to the number of days of rainfall being fixed across all treatments. The differences between treatments, therefore, are the extent of decomposition and DOC production during the 42 day simulation due to desiccation rather than the final water content.

Point 5: a) The data presentation needs substantial revision: The content of tables 1 – 6 and the main message can easily be given in text form (tables 1-6 can be omitted).

Response: We can reduce the number of tables in the manuscript if the editor feels this is necessary. Table 1 can be described easily in the text so may be removed, however Table 2 contains twelve p values as well as eight $\omega 2$ values so we feel a table is appropriate to summarise this information for the reader. It would also be possible to incorporate Tables 5 and 6 into the text if necessary.

Point 5: b) Fig. 1 gives DOC release from the 5 sources, Fig. 2 gives drought effects on only peat samples, Fig 3 gives SUVA only for Molinia, Fig 4 gives removal efficiency for the 5 sources, but without drought effects. Hence, the presentation is confusing and inconsistent.

Response: The reasoning for this is explained in the text as treatment and/or interactive effects are interrogated. In Fig 2 drought effects were shown only for peat as this was the only DOC source with significant drought effects. Similarly, only Molinia was included in Fig 3 as this was the only source with a significant drought effect on SUVA. Finally, drought effects were not included in Fig 4 as there were no significant drought effects for removal efficiency.

Point 6: The conclusions on effects of climate and vegetation change on peatland biogeochemistry are highly speculative in view of this short term laboratory study.

Response: Below is a sentence by sentence justification of our conclusions section.

"Climate projections for the UK vary, however most agree the likelihood of droughts in the future is set to increase."

This point is not from our data yet is supported by research cited in the introduction.

"The results of this research suggest the dominant effect of drought on peatland DOC sources is to increase the amount and decrease the treatability of DOC from peat soils" This is supported by our data. The drought treatment only effected the peat soils, increasing the amount of extractable DOC. The repetition of the control group showed that exposure to oxygenation at any level decreases the SUVA value and humification (measured by peak C) which are both commonly used as indicators of the treatability of DOC.

"This is likely due to the 'enzymatic latch' mechanism increasing decomposition when oxic conditions prevail" Although we present this as a likely hypothesis we have acknowledged in the earlier text that as CO2 production was not measured this cannot be confirmed, hence use of the word 'likely'.

"No drought effect on different vegetation litters was found, suggesting that the greatest effect of drought for vegetation may be facilitating shifts to drought-tolerant species dominance rather than altering decomposition processes in the short term."

Here we note that no effects were found for drought on DOC production from vegetation, whilst qualifying that this was a short term study. We fit our findings into the wider literature which suggests drought may facilitate shifts in species dominance in peatlands and there may be, therefore, longer term effects from droughts. Comparison between the typical peatland species (Sphagnum and Calluna) and the more drought tolerant ones (Molinia and Juncus) in our study sheds some light on these longer term

effects.

"Oxygenation of peat appears to greatly increase extractable DOC whilst also decreasing the aromaticity and humification, which may mean it is more difficult to remove at the treatment works." Here we restate our primary finding which is supported by the data we present.

"These results provide support for catchment management programmes seeking to increase resilience to drought by raising peatland water tables as a strategy for mitigating against high riverine DOC concentrations following droughts."

In this final statement we extrapolate our findings into the context of peatland restoration occurring in the UK and elsewhere. As our primary finding is that oxygenation of peat leads to more DOC which is harder to treat, we suggest that management that seeks to limit the extent and/or frequency these conditions occur is positive from a water treatment point of view. We do not make any other conclusions regarding these schemes.

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