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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Will UK peatland restoration reduce dissolved organic matter concentrations in upland drinking water supplies?" *by* Jennifer Williamson et al.

Eleanor Jennings (Referee)

eleanor.jennings@dkit.ie

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General comments:

This review paper deals with the potential for reduction of dissolved organic matter (DOM) concentrations in potable water sources due to the implementation of catchment restoration measures. The authors note that such measures are now starting to be implemented by water treatment companies, but that the evidence for their success in reducing concentrations is still lacking. The paper, therefore, is of immediate relevance and of use to water managers and policy makers. It also has the potential to highlight new research areas which could address knowledge gaps on this topic. Overall, it

is generally well written and achieves its aims in terms of the review of the literature on catchment management in peatlands and effects on DOM concentrations. It is very informative and thought-provoking. However, while I consider that it is worthy of publication, there are points that do need to be addressed. I outline these below, and also note other more minor corrections for the authors.

Specific comments:

1. Section 1.1 has much relevant information but needs to be better supported by references to inform the reader, especially as the paper is based on a review of the existing literature. For example, the section in lines 48 to 60 on DOM composition is not currently supported by any literature. Similarly the sentences at lines 84-89 and lines 93-94 need supporting references.

2. Section 2 is the strongest section of the paper. It is well referenced and written, and argues its points well. It provides an excellent assessment of the available studies. Table 1 should be referred to more throughout this section to guide the reader. The information contained in this table is central to the points raised but it is referred to only once in the text. I also suggest that Table 2 be brought up into the later part of Section 2. It currently appears only in Conclusions, but logically would come after the review of the studies has been presented.

3. Table 2 also needs some amendments. I strongly suggest that the authors confine themselves to three colours and omit any indication of confidence. As they note, there are still a limited number of studies on this topic and I do not consider that they have enough evidence to infer a confidence level as indicated by the darker red colour. This table should also be expanded to include a column that showed key supporting references. These are not so great in number that they cannot be included. A numbering system for references in Table 2 could also link back to Table 1 where appropriate.

4. Overall, I consider that structure of the paper could be reordered to better lead the reader through the complexities of the issues. If the paper aims to also deal with pro-

cesses in lakes, there is currently some material in Section 3 on in-lake processing of DOM which would be more useful earlier in the paper. I suggest that it be incorporated into a new short section on processing of DOM in both fluvial and lacustrine systems that comes after 1.1.

5. The focus in Section 3 on in-lake processes is not reflected in the current title and therefore the implied scope of the paper, although it is included in the aims. This was a little confusing on first reading the paper. I strongly consider that Section 3 should be restructured and retitled to focus on knowledge gaps in general. In-lake/reservoir processing of DOM in relation to water treatment should be included as one of these gaps, together with topics that need to be addressed in catchment processing of DOM, catchment management and indeed processing of DOM in river networks. One current issue with the paper is that the section on in-lake processing reviews only a small section of the vast literature on carbon cycling in lakes. The implications of in-lake processing for water treatment and in particular carbon cycling could be argued to actually merit a separate review paper. The number of papers on C cycling in lakes and reservoirs has increased hugely in the last decade, and the DOM cycling will be affected by a range of additional processes not referred currently to by the authors. Changing this section to address gaps in general in relation to management of DOM would allow this material to remain, and would give a better structure to the paper.

6. Figure 2 is one of the weaker parts of the paper. It is based on four data points extracted from another publication (Gaffney et al., 2018) and presents two 'new' data points based on an equation using those data. However, I consider that the approach is not sound. The relationship presented is based on the mean porewater DOC concentrations, abstracted from the original chronosequence study. The current authors then projected the two new values for 20 and 30 years post-restoration for that site apparently using an exponential equation based on those four summary values alone. The original paper indicated larger datasets that were summarised as boxplots, where these mean values were also indicated. Those original plots indicated a relatively wide

degree of uncertainty which is not taken into account in their use in Figure 2. There is no indication here that the original full datasets were available to the authors, nor is the equation used presented. I do not consider that the authors should take these mean values and extrapolate future trends, especially 1. without taking account of any uncertainty in the original data, and 2. given the range of processes that could influence those future trends, processes that they describe comprehensively in this paper. Even in the original paper, Gaffney et al. (2018) were only willing to state that their results 'suggest that at least >17 years is likely required for complete recovery of water chemistry to bog conditions'. Figure 2 should be removed and the text amended, but should include the point that this study showed that restoration may take at least 17 years i.e. multiple decades.

7. The paper would, however, benefit hugely from a conceptual figure that could illustrate the mechanisms that control DOM concentrations and quality, including those related to catchment restoration. This figure could then be used to guide the structure of the revised paper.

8. Line 390: the authors state that 'Overall, these results suggest that measures which reduce in-reservoir DOM production, and/or favour in-reservoir DOM removal, may be as – or perhaps more – effective than measures aimed at reducing DOM export from the terrestrial catchment.' This is a strong statement and could be supported a more concrete way, for example a table that compares the published reductions in DOM concentrations in reservoirs due to the cited measures.

9. I also consider that the authors do not currently highlight their own analysis and conclusions based on Section 2 enough in the abstract, and should rewrite with this in mind.

Technical comments:

Line 45: sentence is missing a full stop.

Line 56: suggest that dissolved organic matter should be in lower case, with abbreviation in capitals.

Line 69: the heading here needs to be moved down a line.

Line 80: I suggest that these lines follow on directly after the introduction of THMs in the paragraph before, rather than in a new paragraph.

Line 164: I question the use of 'most' when n = 4; I suggest state for example 'three of the four studies. . .'.

Line 212: this point needs a supporting reference.

Line 335: the heading, or subheading title (subheading if this section is changed as suggested in the Specific comments), used for this should indicate that it applies to lakes and to reservoirs. This section should start by making a point on how many sources of potable water are lakes/reservoirs, or what % of the population get their water from lakes/reservoirs (information on whether this true for the UK would support this).

Line 336: 'Lakes play an important role in fluvial carbon cycling'. I suggest that the term 'fluvial carbon cycling' should be changed. This carbon may be exported from a river to a reservoir but catchment soils are likely to be the more dominant source. This term could be taken to imply that the carbon originated in a river/stream.

Line 345: differing loss pathways will differentially affect DOM treatability – this is an important point.

Line 337: the authors use the term 'can be lost'. Please be more specific in this sentence on the processes that you are referring to. You do go on to give more detail further on, but I would expand this sentence here for clarity.

Line 355: change to 'this hydrophilic DOM for clarity'.

Line 382: suggest this sentence come in the previous paragraph as it continues that

point.

Line 395: the point supported here by Birk et al. 2020 has long been recognised. I suggest that you refer to some of the other literature on this effect.

Line 402: suggest this should read 'increases in concentrations and changes in the quality of DOM. . .'.

Line 420: here they refer to 'algal and manganese control by a number of UK water companies'. I wonder why this point is here in Conclusions and is this not included with detail in the in-lake processes section?

Line 807-809, Figure 1 legend: clarify where on image the dams are visible for 'individual dams can be seen crossing the ditches in image on the left'. Some arrows could be used here to indicate a dam.

Line 796: The reference 'Worrall, F., Clay, G. D., Marrs, R., and Reed, M.: Impacts of burning management on peatlands, 2010' is lacking some of required detail.

Table 1. I suggest that BA be separated by a space or comma from CI where used together in the column 'Experimental design'. Also the term 'Primary CI' is not clear to me. For the line O'Brien et al., clarify that you refer to 'DOC fluxes' in the last column. For the line Urbanova et al. add a comma or semi-colon after NA (note that NA should also be defined in the legend).

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