



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

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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. We thank the reviewer for their positive and helpful comments on the manuscript. We have responded to specific comments in-line below

Specific comments: 1.1. 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. We agree that this section would benefit from more references to the literature and suggest including the following: i) For DOM composition (lines 48-60): Anderson et al 2019, Thurman 1985, Tipping et al 2009 ii) For chloramination (line 84): Norman et al 1980 iii) Regarding customer perception of coloured water (line 89) Ritson et al 2014 iv) Chow et al 2005 provides an example of a water industry specific reference regarding filter size and definition of DOC, which we think would be a useful addition to this paragraph. v) For DOM and bacterial regrowth in water pipes (line 87): Prest et al 2016 vi) The costing information (lines 93-94) has resulted from numerous discussions with water industry representatives as part of the NERC funded Freedom project that funded this review. The outcomes of these discussions are currently in the final editing process before publication as a series of briefing notes aimed at the UK water industry so we would suggest referencing Pickard et al 2021 (full reference: Pickard, A.E., Chapman, P.J., Williamson, J., Spears, B.M., Banks, J., Bullen, C., Leith, F., Gaston, L., Moody, C., and Monteith, D.: Rising concentrations of dissolved organic matter in drinking water supplies: can peatland restoration help? FREEDOM-BCCR briefing note I to the water industry. UKRI SPF UK Climate Resilience programme – Project no. NE/S016937/2. 2021.)

1.2. Section 2 is the strongest section of the paper. It is well referenced and written,

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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. We will move the location of Table 2 and refer to it within the relevant sub-sections of Section 2. We agree that logically it would be better placed earlier in the paper.

1.3. 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. This is a good idea, and we will modify Table 2 in line with the reviewer's suggestions

1.4. 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 processes 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. We will add a section on DOM processing in rivers and lakes into the introduction.

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. We agree with the reviewer that a complete review of carbon cycling in lakes is beyond the scope of this paper and would make an interesting paper of their own. We will change the structure of this section to cover evidence gaps specific to the water industry as suggested (and move some of the introductory information on in-lake processes to the introduction as outlined in point 1.4 above). This will improve the flow of the paper, and along with a conceptual diagram (see point 1.7 below) will make the paper easier to follow.

1.6. 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. On reflection, and although authors had access to the data contained within the Gaffney et al paper, we agree with the reviewer and will remove Fig 2 as it is based on limited data, and from one location, which may not be representative of other areas. We will also add reference to a new paper on this topic (Howson et al 2021) and compare their results to those of Gaffney in the text only.

1.7. 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. We have attached a potential conceptual diagram to this response. This will be further neatened provided it is felt that we have covered the main points. We would put this diagram in the introduction and add the aims up front to aid the reader in following the text through. This would be included with a paragraph as follows before section 1.1: The past decade has seen peatland restoration become an integral part of the UK’s environment policies, including the development of an English Peat Strategy, the Welsh Government’s commitment to the restoration of all semi-natural peatlands and Scotland’s national peatland plan, because of the potential role of restoration in improving biodiversity, carbon storage and natural water management. As nearly three quarters of the storage capacity of drinking water reservoirs in the UK is derived from water draining peaty areas (Xu et al 2018) the UK water industry has investigated whether peatland restoration and management could improve water quality at source in order to reduce treatment costs and risk of regulatory failure (See schematic). This review summarises the available evidence regarding the effects

of peatland catchment management on water quality, highlights the current evidence gaps and suggests priorities for future investigation. Then remove lines 152-159.

1.8. 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. This comment related to the magnitude of change seen in some sites reviewed in Evans et al 2017. We feel that this could be worded better and state that there is currently a knowledge gap with regards to the impact of catchment management on in-reservoir carbon cycling, and the extent to which measures which reduce in-reservoir DOM production and/or favour in-reservoir removal of DOM. This knowledge gap could be explored further by the UK water industry, particularly in areas where previous catchment management has not shown an improvement in water quality.

1.9. 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. See comment to Reviewer 2, in section 2.1

Technical comments: Line 45: sentence is missing a full stop. Agreed Line 56: suggest that dissolved organic matter should be in lower case, with abbreviation in capitals. Agreed Line 69: the heading here needs to be moved down a line. Agreed 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. Agreed, change as suggested. Line 164: I question the use of ‘most’ when $n = 4$; I suggest state for example ‘three of the four studies. . .’. Change as suggested

Line 212: this point needs a supporting reference. We will include Stimson et al (2017), and refer the reader to further references within this paper. We will also include further

reference to this paper in the section on revegetation.

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). Xu et al 2018 estimate that 72.5% of the storage capacity of UK reservoirs, or 1.56 billion cubic metres of drinking water per year, derive from organic soils in the UK, which supports 43% of the UK population. There are over 450 large dams for public water supply in the UK of which 80% are in upland areas (CIWEM 2011). In the UK surface water supplies 70% of drinking water (water.org.uk) Rather than bringing this information in here, I think that this information, alongside the suggested schematic, would be better as an opening paragraph showing the scale of the problem and why it is worth looking into. I think this would help with the comments that the structure needs improving and would assist the reader in following the article.

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. We agree that the catchment soils are likely to be the more dominant source, especially in the upland landscapes we are referring to in this review. We would therefore suggest “catchment carbon cycling” as a more inclusive term.

Line 345: differing loss pathways will differentially affect DOM treatability – this is an important point. Thank you. We will make sure this point is highlighted in the conclusions.

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. We suggest changing the sentence to: up to 85% of OC entering lakes & reservoirs can be recycled into biomass, held in sediments or emitted to the atmosphere, retaining the original references.

Line 355: change to ‘this hydrophilic DOM for clarity’. Agreed

Line 382: suggest this sentence come in the previous paragraph as it continues that point. On reflection, yes it does belong with the previous paragraph.

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. We will add further example references including Bracchini et al 2006, Carpenter 1998, and Karlsson et al 2009.

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

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? This point was included as water companies have looked into whether they can control in-lake processes, or use differing draw off depths, but to our knowledge these have been investigated for control of manganese and algae in the raw water entering the WTW rather than whether they make a difference to DOM concentrations. The part of the sentence referring to algae and Mn could be removed without changing the meaning.

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. Agreed, we will add these.

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. The reference should read Worrall, F., Clay, GD., Marrs, R. and Reed, M. (2010) Impacts

of burning management on peatlands. IUCN Peatland Programme Commission of Enquiry on Peatlands. We will amend.

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). We will change to BA CI rather than BACI. Primary should read primarily – as in I think they did do one measurement prior to ditch blocking but not an extended period of baseline measurements. It would probably be clearer to refer to this as CI only.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-450>, 2020.

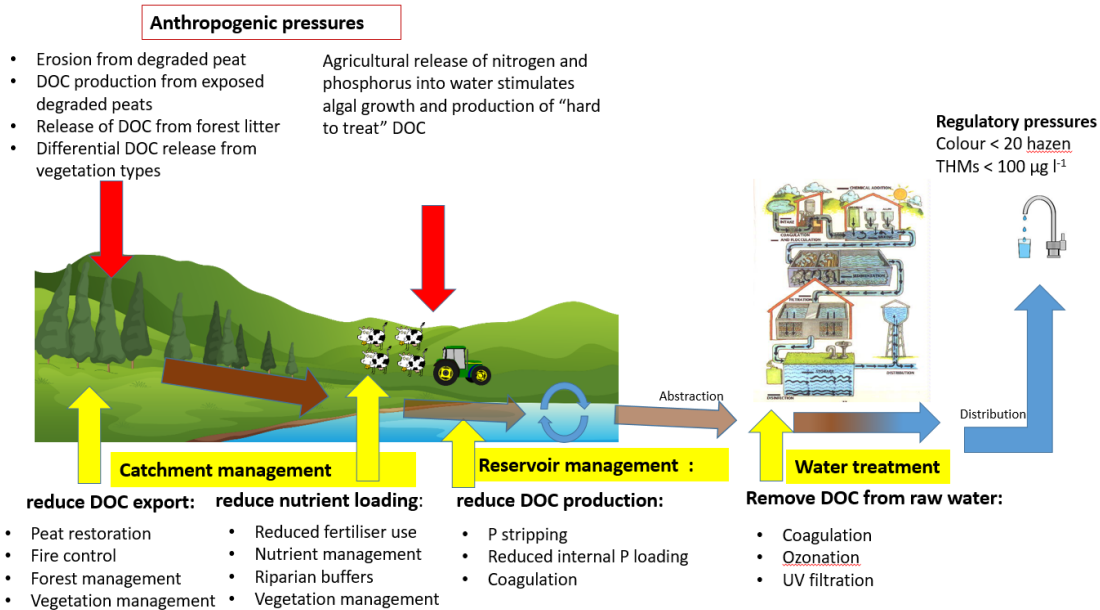


Fig. 1.