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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydrological control of dissolved organic carbon dynamics in a rehabilitated *Sphagnum*–dominated peatland: a water-table based modelling approach" *by* Léonard Bernard-Jannin et al.

Anonymous Referee #1

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The ms describes a modeling approach to predict the relationship between DOC production and hydrology, especially water table levels, in a fen which partly experienced draining and restoration. The authors used a parsimonious approach including a hydrological and a biogeochemical component. The model was calibrated directly at the site through monitoring of the peatlands water table level at the two measuring sites and by DOC analyses. I addition the authors carried out fluorescence measurements to determine changes in the DOM quality. In principal, several parsimonious models to predict the release of DOC dependent on hydrological changes from peatlands ex-

ist. Different from these models which commonly use discharge data to characterize the hydrological component, the authors use changes in water table to predict DOC release. The authors concluded that their model predicts DOC release at the two sites reasonably well (whatever this means). As an advantage compared to other models they argue that the use of water table changes instead of using discharge measurements allows predicting local changes in DOC release esp. if parts of a peatland underwent draining and restoration, which might have strong influence on DOC production and release. I don't have an emphasis on hydrological modeling of peatlands but on the biogeochemical processes of DOM production on peatlands, so I will not comment on the quality of the model itself. I had a bit a hard time with the structure of the paper. One reason might be that the mentioned objectives of the study are very general .... to identify hydrological processes and factors controlling DOC dynamics .....the impact of rewetting on DOC export. It is known that water table changes is the major driver of DOC release in peatlands, because surface wetness determines the redox conditions on the mire's surface which in turn control microbial activity and peat decomposition. In this sense, it would be helpful if the authors could first describe in more detail what they expect the effect of peatland restoration on DOC release could be, especially what is expected how the amount and quality of DOM changes as result of drainage and subsequent rewetting. Based on this it would be easier to understand the chosen approach. Moreover, I think that the comparison between the WTD and the stream discharge approach should be discussed in more detail. On page 12 the authors argue that the advantage of the WTD approach is that this model can predict DOC release at different areas in the same peatland in contrast to the stream discharge approach which integrates the entire peatland. However, the authors have only selected a single site with is under restoration and did not define which biogeochemical or hydrological component/factor is specific for the rewetted site. Due to this, it remains unclear how the specific behavior of the rewetted site compares to the overall variation of DOC release and DOM quality during water table changes in the entire peatland (compare the discussion in Birkel et al 2017 and Broder et al . 2015 cited therein).

This means that the observed changes at the single rewetted site provide a small data set only, so that it remains somehow speculative if the shown changes and effects are specific for rewetting sites. A larger data set which shows the general variability of WTD, DOM quality and DOC export at the selected peatland compared to the specific rewetted site would convince me that the chosen approach is of advantage compared to the one using the stream discharge approach. I suggest that the authors discuss this in more detail including effects of spatial varying redox conditions (also hummocks and hollows), plant cover (what means Sphagnum dominated in this context), mineral redox barriers esp. iron-oxides; which are important for DOC dynamics in fens (is this site minerothrophic or ombrothrophic?).

Specific comments: - Please check if all abbreviations are explained when mentioned the first time - P3 mention trophic state of fen - P6 L20, ...the higher the soil moisture, the more DOC is produced. . .. I doubt this very general statement. Permanent water logging reduces DOC production (anaerobic conditions). - P6 L 26. . .. What is DOC loss here, mineralization or export from the peatland or both? - P6 L32 . . .what is the DOC concentration in rain water? - P7 L 5 expand on how exactly the DOC model was calibrated - P7 L11 the period . . . was not simulated . . .. because exceptionally high rainfall. . .. . . Water coming from flooded rivers. I think it is probably a major weakness that the model cannot simulate heavy rain events because those are very important for DOC concentrations and release (connecting pools etc.) please comment on this how this affects the overall quality of the model. See also P8 L28 . . . decrease in model efficiency. . ... explained by exceptional events. Please explain why you think how the model is still useful if it cannot simulate rain/drought events and related preconditions. - Please avoid “good results”, “satisfactory results” etc. as these are undefined terms or define what this means. - P10 L27 and Figure 5, what are upstream and downstream locations? This is discussed on page 10, but I could not find a description in the methods section. - Fig. 6 difference in DOC variation between control and rewetted site and discussion on page 9. I think this observation needs further discussion. I don’t understand why DOC export (I would prefer “release” here)

is episodic at the rewetted site and I don’t understand why this observation is consistent with the observation of Birkel et al., 2017 who used a stream discharge approach. Please explain! - Regarding a detailed explanation of the biogeochemical module, the authors refer to a previous paper. I think the authors should expand a little bit on this especially how draining and rewetting alters the quality of organic matter in comparison to the control site (density etc.). I also wonder why the authors believe that deeper peat is more aromatic. Please explain and include appropriate reference. - I suggest a language polishing by a native English speaker

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