Answer to the comment by anonymous referee #1

We thank the reviewer for the deep review that will help to improve the quality of the paper and we explain below how we plan to address each of the points raised.

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. In 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 exist. 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.

We will add a paragraph in the introduction explaining the expected results of restoration on DOC dynamics based on existing literature. Air Temperature and WTD are considered to be good descriptors for conceptual DOC models (Birkel et al., 2017; Lessels et al., 2015). Indeed these two parameters determine the microbial activity and peat decomposition that are the producing and consuming processes of DOC in peat water. Concerning studies on the impact of rewetting and drain blocking in peatland on DOC dynamics have shown opposite results with increasing concentrations (Hribljan et al., 2014; Strack et al., 2015) and decreasing concentrations (Höll et al., 2009; Wallage et al., 2006). In this study we propose to use a modelling approach to simulate DOC concentrations in a rewetted peatland and identify factors of control that can explain differences in DOC dynamics.

In addition, we will specify the objectives of the study as follow: “The objectives of this study were to 1) identify the dominant hydrological processes in both the rewetted and undisturbed peatland locations, and 2) to understand how these hydrological processes affect the DOC dynamics in each of the two locations.

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).

The spatial variability of the biogeochemical or hydrological components is included in the range of the model parameters defined for the model calibration. Then the model is calibrated for both sites. It is therefore the values of the calibrated parameters that give an indication on the behavior (hydrology and DOC) of each site in order to explain the differences observed between both sites of the peatland.

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.

Unfortunately we don’t have the dataset as asked by the reviewer with in the same point WTD and DOC values. However it is clear that we have two locations with different water table and DOC dynamics, and that the model is able to explain these differences by differences in the partition between slow and rapid drainage. We will discuss this in the revised version of the ms by explaining that the different dynamics in the rewetted area might be explained by others factors than the restoration

The main reason of using a water table based model instead of a discharge based model is that it can be challenging to monitor discharge in peatlands (flat areas with many outlets varying in time and space). Using a water table based model allows to simulate hydrology when discharge data are missing as it is the case in our study site. Discharge based model would be more robust when simulating fluxes but water table based model has one advantage as it allows the application of the model in different locations within the same peatland. Therefore the use of a water table based model has to be seen as an alternative to the discharge based model when discharge data are not available.

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?).

In this study we mainly focus on the impact of hydrology on DOC dynamics therefore other factors such as redox are not discussed but their potential effects will be added in the new version of the ms. The plant cover is very homogeneous all over the site with a mixture of Graminoids (Mainly Molinia caerulea and in a much lower extent Eriophorum angustifolium and Ryhchospora alba), ericaceous shrubs (Erica tetralix and Calluna vulgaris) and sphagnum species. In addition, the site is an oligotrophic fen that has not developed any abundant hummock and hollow microtopography, such as in a typical ombrotrophic site

Specific comments:

- Please check if all abbreviations are explained when mentioned the first time

We'll check and correct after the final modifications.

- P3 mention trophic state of fen

It's oligotrophic. It will be added.

- 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).

We will remove this statement.

- P6 L 26 . . . What is DOC loss here, mineralization or export from the peatland or both?

Here, DOC loss corresponds to mineralization and sorption, it will be add in the ms.

- P6 L32 . . . what is the DOC concentration in rain water?

DOC concentration in the rain was measured in rain water samples and is 2 mg/L

- P7 L 5 expand on how exactly the DOC model was calibrated

The calibration of the model is explained in detail in the section 2.2.4

- 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.

The model is water balance model, it is therefore impossible to account for water coming from exceptional flood events. One have to keep in mind that we speak about very exceptional events (return period of about 50 years). Regular flood events can be simulated by the model. Moreover, the exceptional flood affected only Sr reservoir because peat profile was already fully saturated with rain water when the flood occurred. Therefore the impact of the flood on peat water DOC should be very low. We will add this comment in the ms.

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.

This was indeed a weakness of the model. However, following the comments and suggestions of the 2nd reviewer we were able to improve the model calibration. And the model is now able to simulate with the same efficiency wet and dry condition (except exceptional rainfall events as discussed above)

- Please avoid “good results”, “satisfactory results” etc. as these are undefined terms or define what this means.

We will define these expressions and assess the quality of the model based on the value of the efficiency criteria. In addition and following comments of the second reviewer, an uncertainty analysis have been performed to better assess the confidence we can have in the model.

- 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.

Upstream is control and downstream is rewetted, we will make the changes.

- 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!

We will change export to release. DOC release at the rewetted site is episodic because it is linked to the hydrology which is controlled by rapid drainage resulting from rain events. On the opposite DOC release in control site is linked to the hydrology which is controlled by slow drainage and is therefore less impacted by rain events. The observation is consistent with Birkel et al (2017) that found an episodic release of DOC (60% of release in 30% of the time). This is an analysis of the model results and is independent of the use of a water table or a discharge based model (both models simulate DOC release).

- 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.

We will improve the explanation of the links between biogeochemical module and hydrology. We believe that DOM will be more aromatic in deep layer because it is “older” than in surface where the input of fresh organic matter would lead to less aromatic DOM as discussed in Strack et al (2015).

- I suggest a language polishing by a native English speake

We will send the paper to a corrector after the final changes are made.

References:

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