Review of manuscript "Hydrological control of dissolved organic carbon dynamics in a rehabilitated Sphagnum–dominated peatland: a water-table based modelling approach" submitted to HESS by Bernard-Jannin et al.

The paper deals with the effect of peatland rewetting/restoration on DOC dynamics. The study combines field monitoring (DOC pore water concentrations [DOC] and water table depth) with hydro-biogeochemical modeling. Two monitoring sites were installed within the same acidic fen peatland, one defines the control site (disturbed, drier conditions) and one the rewetted case due to recent (3 years) blocking of a ditch. Field data indicate a strong effect of the rewetting on the water table depth (>20 cm shallower water levels during summer) and higher [DOC] in summer in the rewetted location compared to the control. Also the quality of DOC seems to change due to rewetting. Modeling results indicate very contrasting fluxes at the two sites, fast runoff with very low evapotranspiration at rewetted sites vs. slow "deep" drainage at control site, which is interpreted to control DOC composition and export.

The basic scope of the paper (topic, field data, model approach) is interesting, relevant for the community and suitable for publication in HESS. The authors dealt with a challenging site because fluxes were not observed. Calibration of a hydrological model only on water table depth data with the objective to interpret resulting fluxes is questionable although maybe not impossible when setting sufficient constraints on calibration scheme and parameters, and when providing reliable uncertainty estimates. However, as the modeling, and in particular the calibration, has been conducted and presented now, one cannot trust the highly questionable simulated hydrological fluxes (e.g. extremely low ET at rewetted site). Fluxes are crucial for the [DOC] dynamics, e.g. ET causes a 'physical' accumulation of DOC in the pore water, thus one can also not rely on the following implications for DOC concentrations and exports. In my major points, I thus focus on the hydrological part of the paper. The DOC part is commented with less detail, although it must be also fundamentally revised because I assume that results will change considerably with an update of the hydrological modeling.

In general, as mentioned before, fluxes of a hydrological model calibrated only on water table depth should be interpreted carefully. I present my major concerns and give some additional recommendations afterwards.

Major points

I see several major problems in the hydrological modeling part of the study. The hydrological model is of fundamental importance for the results of the manuscript. The authors intent "to identify the main hydrological processes and factors controlling DOC dynamics" with their model which is based on a conceptual approach based on reservoirs. Modeled are two monitoring sites that lie in the same disturbed fen peatland. I assume the two sites were very similar before 2014 (same vegetation, 60 cm peat thickness, … same peat properties?). The only that changed in 2014 was the boundary condition for the rewetted site, which is effectively a higher drainage level due to ditch blocking.

Separation into boundary-condition dependent and boundary-condition independent parameters:

Having repeated the field situation, it is only possible to rely on the simulated fluxes when all model parameters that do not depend on changed boundary conditions (i.e. here the changed drainage level) are the same for both monitoring sites. As far as I understood the three

discharge coefficients (overland, lateral runoff through peat, and "deep" drainage) are dependent on the raised drainage level due to ditch blocking. As the conceptual model does not include a drainage level as a boundary condition, these parameters have to be tuned that simulated water levels match observed ones. *(This is actually a major weakness of the presented conceptual model, as for a more process-type of model only a single boundary condition would need to be changed and/or calibrated if not known precisely. Emphasize this disadvantage.).*
In contrast to the three discharge coefs, I consider the ET crop coefficients and infiltration rate from the peat-macro to the peat-matrix reservoir (Imax) as "boundary-condition-independent". They should be thus calibrated to the same value for both sites.

In the study, however, all these parameters were calibrated separately at both sites resulting in extreme differences for the crop coefficients and Imax at the two sites. As a consequence, e.g. ET is extremely low at the rewetted site. The authors recognized that this flux is unphysical and explain it by additional open water reservoirs (not modelled) that are laterally coupled to the modeled 1D profile and buffer water table depth dynamics during high water levels. I think this might be an important process and a valid interpretation, but a dramatic reduction of ET for the rewetted site is an unacceptable result for the scope of the paper. If this process (open water storage) is really relevant, it needs to be accounted for.

To further prevent such an "overfitting effect", I recommend that authors think about reliable constraints during the calibration. For example, I suggest to calibrate all parameters simultaneously for both sites keeping the "system boundary condition independent" parameters the same for both sites ('multi'-site calibration approach)

Reliability of validation period:

Validation with independent data is crucial. The authors used the third year of data for validation. This is basically ok. However, Nash-Sutcliffe efficiency (NS) drops from 0.8 (calib) to 0.3 (valid) for one of the sites. The enormous drop of performance is a strong indication of overfitting. Authors argue that it might be related to the exceptional flooding event between the second and the third year. If they are convinced that this is the reason, then the whole validation period is useless because we learn nothing about the reliability of the calibrated model from it. In fact, from what I can read and see in the paper, I have the impression that the third year can be used for validating the hydrology, because the initial water table depth of the validation period seems to be quite consistent between model and data. Or were the initial condition of the model set to the observed data? If yes this must be stated clearly, and the validation period should be shortened as validation NS would be overestimated by this manual adjustment of the state variable.

Definition of reservoirs

Sm is 0 at the lower peat boundary, why? Did authors determine in the lab that there are no macro-pores at 80 cm? At the surface the total porosity (Sm+Se) of the peat is defined to be 1. Justify also this decision, as it is not obvious how a material can have a porosity of 1. Did authors want to prevent a discontinuity between soil and surface storage? But why?

DOC measurements

The definition of the reservoirs is of particular importance also for the DOC interpretation. Were the sampling pipes emptied before taking samples? If not, it is not clear what the [DOC] in the pipes actually is (mixture of recent rain water, [DOC] of soil pore water X days/weeks before sampling minux the losses due to being ‘open water’ in the pipe afterwards, etc.) . If yes, mainly the macro-pore water was refilling the empty pipe afterwards, i.e. authors need to compare the field data with modeled Sm [DOC], if I understood the approach correctly. Regarding the issue of sampling different soil pore spaces see also Zsolnay, 2003, Dissolved organic matter: artefacts, definitions, and functions, Geoderma.

Effect of flooding event on DOC validation period

What is the [DOC] in the river water? And do authors have any knowledge about how the flooding event affected the [DOC] of the peat profile in the third year? Authors need to discuss this. Till now, the third year is in particular questionable for validating the DOC model.

Recommendations:

Authors must establish much more confidence for their model. Besides taking into account my major and minor comments, I recommend that authors elaborate a sophisticated uncertainty analysis that provides uncertainty estimates for the flux components, i.e. that shows how variable the predicted flux components are for similarly performing models (similar fits of water table depth). Use probabilistic approaches like e.g. Bayesian modeling or GLUE. Authors can only generate sufficient confidence into the model results when authors are very critical with the calibration. For that, consider also the effect of optimizing the parameters for different objective functions, not just the daily NS or RMSE (e.g. different levels of aggregating the time series, daily, weekly, monthly averages) and add plausible errors to the forcing and water table depth data. These are ideas for directions authors need to look for.

Minor comments:

Page 4 - L15: “identify the locations” – I do not understand the sentence.

Page 5 – L4: What is Smax? Not defined after the equation but two paragraphs later.

Page 6 – L32: Why t+1 for PDOC and LDOC? Isn’t it dependent on the concentration of time step t. Clarify this also in eqs 7 and 8

Page 7 – L14: How can the maximum porosity of the peat layer be equal to 1?

Page 7 – L14: “flooded river” … wording. The river itself cannot be flooded.

Page 7 – L20: Give both NS and RMSE for both the hydrological and the DOC model. Not clear why different skill metrics have been chosen for the two variables. Would be interesting to know NS of the DOC model. It seems to be barely better than just the mean of the observed concentrations (i.e. NS~0), in particular for the validation period.

Page 7 – L22: calibrated over the calibrated hydrol. model. …. improve wording.

Table 1: correct table, reservoirs and discharge description are confused !

Table 1: How can Imax as a rate have the unit [mm]? A rate is always per time.

Table 2: Format table correctly.

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