Anonymous Referee #1

We would like to thank reviewer #1 for the review and the helpful comments.

This study is showing very well how DOC/N concentrations are changing in ditch and groundwater of a drained peatland depending on water table changes at different temperatures and offers highly interesting data of DOC/N losses of a small catchment characterized by organic soils over a period of three years. Most favourably the sampling period was characterized by a very dry and a very wet year so that the importance of hydrological conditions on hydrochemical changes could be investigated fairly well. I completely agree with the authors that such data can be hardly found in literature and therefore I strongly believe that the scientific community will highly benefit from a publication of this manuscript. For me it was highly interesting to learn that DOC concentrations in fen groundwater are not necessarily increasing at rising water tables and also that DOC losses must not play significant role for the total carbon budget.

Likewise from the management perspective data of this study are highly interesting. However, I have some doubt that the land use changes occurred within the time of sampling can be discussed seriously here.

> We agree with the reviewer that under optimal circumstances the effects of the land use change would have been measured directly. However, we had no prior information on the renovation of the ditch network or the ploughing of the grassland. In the discussion, we state that these changes are a "plausible explanation" (page 19, line 8) for the observed changes of the concentration patterns, and overall, we believe that this wording is rather careful, and that we can indeed discuss land use change here as there are no other factors which could plausibly explain the measured decoupling of the groundwater and ditch concentration levels.

Apart from this, all parts of the manuscript are written in a clear and consistent manner. Overall, the text is very well structured and organised, making it easy to follow the narrative flow of the text. All figures and tables are informative and substantiate the text.

I have identified only few flaws which should be removed by the authors before I would recommend a publication of the manuscript. The two major concerns I have is that it remains obscure for me:

1) which portion of the catchment under investigation is really covered by peatland. Missing this information it complicates the comparison with other studies.

> Around 50 % of the catchment is covered by organic soils. This number was added to the site description (page 4, line 29). Additionally, the boundary between organic and mineral soils estimated from augering (Blume, 2005) was added to Figure 1.

2) to which extent the considered ditch (= “catchment outlet”) in the study is representative for the DOC and nitrogen losses of the total catchment. Can you exclude that ground water export is an important pathway?

Regarding the first point I am not sure if all nitrate and also all DOC found in the fen groundwater is solely originated from the peat soils? From my own experiences (shallow) groundwater received from mineral soils can not only characterised by high nitrate concentrations (» 50 mg N/L) but also by high DOC-concentrations (» 30 mg/L).

> The discharge in the ditch is certainly composed of both (tile) drainage water and groundwater, and given the heterogeneous geological situation of the area, possibly even deeper groundwater (page 9, line 31). Unfortunately we did not sample any deeper wells below the peat or in the part of the catchment with mineral soils.
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> Nitrate concentrations in tile drainage water at a field site approximately 2 km away have indeed been found to be very high (Tiemeyer et al., 2008). Soil properties and land management are similar to the mineral soils within our catchment. However, the mineral soil in our catchment is not tile-drained. Therefore, the residence time in the groundwater will be much longer. A study by Janssen et al. (2013) at the above-mentioned field site has shown that nitrate concentrations under undrained buffer strips fell to, on average, 0 to 1.0 mg/L at 4 m depth. This reduction took place within buffer strips of only 1.3 to 7 m width, and therefore we assume that denitrification will be at least as effective during the much longer passage from the arable land to our ditch or to the fen peat.
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> Regarding the DOC concentrations, we did sample the ditch draining the same experimental field site as mentioned above. DOC concentrations were as low as 5.4 ± 1.4 mg/L (n = 247, unpublished data). Furthermore, the DOC concentrations in the shallow groundwater (peat) are clearly higher than in the ditch sampled in the study presented here. These two observations point to the peat as the main source for DOC.
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> Therefore, we assume that the peat is the main source of DOC and especially of nitrate, but we added the statement that we cannot quantify a possible contribution of the mineral soils to the total nitrate and DOC losses (page 19, lines 30-31 and page 20, lines 1-3).

There are some more formal things which I would change.

1) I suggest including a “sampling” section after Line 14, Page 3029. Within this new section it should be explained more detailed how sampling of dip wells was performed. In particular if or how samples were filtered and fixed in order to minimize hydro-chemical changes due to aeration of samples (redox-change) and degassing of carbon dioxide (pH-change).

The suggested sub-chapter was added. We did not acidify the samples as this could, in our experience, cause an underestimation of DOC (page 6, lines 21-22). A loss of carbon dioxide is limited by immediately and completely filling the sample bottles (page 6, line 19).

2) At the end of the statistic section (Page 3032, Line 16) I recommend to explain more detailed the "Nash-Sutcliffe coefficient (NSC)". In Wikipedia I found the following which might be useful: "Nash–Sutcliffe efficiencies can range from −1 to 1. An efficiency of 1 (E = 1) corresponds to a perfect match of modeled discharge to the observed data. An efficiency of 0 (E = 0) indicates that the model predictions are as accurate as the mean of the observed data, whereas an efficiency less than zero (E < 0) occurs when the observed mean is a better predictor than the model".

The Nash-Sutcliffe (or model) efficiency can range from -∞ to 1. A more detailed explanation was added as suggested (page 8, lines 27-30).

And, finally some more minor things.

1) Abstract (Page 3024). I suggest showing also the range of the concentrations of nitrate and DOC.

Done as suggested. We also added the DOC concentrations in the groundwater which were so far missing in the abstract.

2) So far I was always translating N-mineralisation with ammonification (formation of ammonium) and nitrification as separate (subsequent) process, however I do not know if there is a strict definition (e.g. Page 3038, Line 27).

This is correct. Megonigal et al. (2003), for example, use the term mineralisation for the formation of ammonium from organic nitrogen. To be more precise, we replaced "mineralisation" with "mineralisation and subsequent nitrification" at several places (page 14, line 12; page 14, lines 23-24; page 15, line 21 and page 18, lines 5-6).

3) For me it is new that the EU Water Framework Directive is using 2.5 mg N/L as "target value"?! (are you sure?)

This expression was indeed too ambiguous. In Germany, the LAWA ("Working Group on water issues of the Federal States and the Federal Government represented by the Federal Environment Ministry") defines 2.5 mg/L NO_3-N as "moderately polluted"(LAWA, 1998) and as the target value for all surface water bodies. In

Germany, this definition is also used in the context of the WFD (UBA, 2010). The text has been clarified as suggested (page 17, lines 13-15).

4) Page 3045, Line 13. I would write "a net ecosystem exchange (NEE) of carbon dioxide between around 4000 and 9000 kg C ha^{-1}".

Changed as suggested (page 20, line 10).

References

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