

Interactive comment on “Dissolved organic carbon concentrations vary with season and land use – investigations from two fens in Northeastern Germany over two years” by M. Schwalm and J. Zeitz

Anonymous Referee #2

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General comments

This manuscript describes two years of DOC data, with accompanying water level/discharge and meteorological data, for one drained (agricultural) and one rewetted (former agricultural) site. The questions of whether DOC loss is a significant component of the carbon loss from fen peats, and of how these DOC losses respond to drainage and other forms of management, are important and topical – for example this was recognised as a source of uncertainty in the recent IPCC Wetland Supplement. Therefore, this study should provide a useful addition to a currently small evidence

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base, supporting understanding of managed fens and informing their management.

Unfortunately, however, some aspects of the study seem to have been either undertaken or described in a rather amateurish way, in particular the description of sites and methods used is completely inadequate, leaving me far from convinced about the validity of the results. For me, the greatest problem is the estimation of DOC fluxes. By their nature, fens are flat, receive large groundwater water inputs from uncertain sources, and can lose water via subsurface pathways as well as via drainage channels. As a result, defining the catchment area of a fen can be extremely difficult, yet the authors simply provide a couple of roughly drawn outline maps with no information on topography, flow directions, land-use or hydrological management. How the catchment boundaries were defined is not explained, and yet the calculation of DOC fluxes per unit is critically dependent on these terms, and so therefore are the conclusions about the effects of land-management on DOC export. I find it surprising that fens in drained landscapes should have such natural catchments, and am quite doubtful whether these really define the boundaries of water input to the sites. In most drained peatland landscapes, ‘catchments’ are largely defined by the configuration of drainage channels, not just the surface topography, and in fens it is particularly complex due to the subsurface movement of water. From Table 1, 35% of the agricultural site is not peat, but which areas, and how do these areas influence results? In a groundwater fed system, alkaline (and low-DOC) water will enter from adjacent mineral soils/bedrock, diluting the DOC concentration of water in the fen to an uncertain extent. Additionally, the authors refer to the use of pumps in the agricultural catchment, without explaining what the pumps are doing (removing water? If so how much?), where they are located, or whether pumped water was included in the flux calculations. The final paragraph of the results and discussion section finally acknowledges the uncertainties, but these issues should have been addressed earlier.

In summary, with only two catchments (i.e. no replication, and with catchments areas that differ by an order of magnitude) it is unclear whether differences in DOC concentra-

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tions leaving the fen are attributable to drainage and re-wetting, or to other factors. All of that said, I am prepared to believe that the large differences in DOC concentrations between the sites are indeed a result of management factors, but I would like to see much stronger presentation of the evidence, and the methods used to obtain it, in order to be convinced. On this basis, I do not think that the paper is currently acceptable for publication.

Some secondary general comments - I am not particularly keen on the mixing up of results and discussion. This results in some subjective statements during the reporting of results - e.g. 'interestingly' on the first line of this section, 'we assume that' later on, and 'in our opinion' on page 7088, where the authors propose a hydrological explanation for observed differences in DOC between sites, without having first described their hydrological results. Similarly, results from previous publications are interspersed with and sometimes described before the authors' own results, which makes it difficult to work out what is new here. There is a lack of statistical analysis, resulting in a largely description and discursive analysis. The discussion of seasonal variations was rather confused (or confusing) and I was not sure it added much to the paper. Finally, units are mixed up unnecessarily - i.e. g C/m²/yr and kg C/ha/yr, mm/cm/dm/m.

Specific comments:

Abstract: I think the statement about DOC 'harming' water quality and diminishing carbon storage is questionable - the 'harm' is only really true for water treatment, and the connection between DOC and carbon storage is not straightforward, e.g. if most DOC is from recently fixed carbon, it is not really a loss of soil carbon - see e.g. Evans et al., Geophysical Research Letters 2007

Page 7080: Line 8: 'Conducted' mis-spelt.

Line 22: I think Parish et al is a secondary reference for this figure

Page 7081: Line 9: Driscoll is mis-spelt.

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Line 15: '17% of the total carbon exchange' does not make much sense to me - net C exchange can be positive or negative, and is the balance of two large gross fluxes (photosynthesis and respiration) plus some smaller ones (DOC loss being usually the most important).

Line 18: This is not the correct final reference for the IPCC Wetland Supplement - also elsewhere. I suggest using IPCC (2014) as the reference in the text.

Page 7082: Line 10: The reference to acidification due to declining water table requires some explanation, i.e. that is the result of sulphur oxidation.

Line 19-24: The two sentences here about DOC response to re-wetting appear to contradict each other. This whole paragraph seems a little confused, throwing a lot of references together without a clear structure. The seasonality issue should be more clearly differentiated from the discussion of drainage and re-wetting effects

Page 7083: Line 23: I think these are hypotheses, not assumptions? Hypothesis 2 seems so well-established that I wonder if it is worth including here? Also, why nothing about fluxes here?

Page 7084: Line 14: Use m or cm rather than dm for depth.

How far apart were the study sites? It would be good to know this without having to process the latitude/longitude data from table 1.

Line 21: 'Mainly peatland specific plants' and 'a little swamp forest' is completely inadequate - what species are present, what is the management? Similarly, what is the management of the agricultural site - livestock? arable?

Page 7085: Line 12: What method was used to estimate DOC - total C minus inorganic C, or non-purgeable organic carbon? This is important - the NPOC method is problematic for high-pH because some of the organic carbon tends to precipitate out when the sample is acidified. On this subject, it seems to me that inorganic C has been largely ignored in the paper, but in a fen it may be a large part (the majority?) of the

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carbon export, even if it does not all necessarily derive from the peat.

Line 13: What depth were soil waters collected from within the wells (see also later comment about the results)

Line 15: Was a baro-diver used to correct for atmospheric pressure? Line 21: Use of 'interval' here is unclear. Line 23: Where is the rating curve? A figure, or regression equation with accompanying statistical information should be provided. Line 27: What was R used for? I can't see any statistics in the paper.

Line 7086: The information on sources of weather data should be in the site description or methods. Line 3: Delete 'interestingly' - better just to report the results here. Line 9: Hard to distinguish negatives from dashes – suggest using 'to' instead of '-'. Line 14: 'Unordinary' should be 'atypical' or similar.

Page 7087: Line 22: I think higher DOC concentrations in porewaters could also be explained by low mobility of water within the peat, particularly if samples were collected from deeper within the peat profile.

Page 7088: Line 10: It seems highly questionable to use literature data from a fen in Canada as a natural reference condition for the re-wetted site in Germany. It is reasonable to compare the data, but I would be more cautious about the statement that DOC concentrations at the RW site are only marginally elevated. You would need data from a natural site in Germany with similar site characteristics and water balance to support this conclusion.

Page 7089: Line 1: Unless you have some data on either phenolic concentrations or enzyme activities, the reference to the enzymic latch is no more than speculation – I suggest removing this, and restricting the discussion to what the measured data actually do or don't show.

Line 3: I think possibly the ditches at the study site of Kalbitz and Geyer intersect the mineral soil? I believe their sites were shallow relic peats, which could explain

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reduced DOC concentrations in drained sites due to greater mineral DOC retention.

Line 24 (and elsewhere): Use full site names or acronyms, but don't mix them up, especially not in the same sentence.

Page 7091: Line 3: The hypothesis that 'concentrations of DOC underlie seasonality' does not make much sense.

Line 22, 23: 'Billet' should be 'Billett'. Also, 'has shown' should be 'showed'.

Page 7092: Line 1: I don't really understand how a paper from 1998 could have questioned the peat C accumulation rates published in 2004-2007? Also, there are a number of more recent full carbon budgets for peatlands that could be referenced here, several of which included DOC and some of which (e.g. Dinsmore et al 2011) highlight the importance of aquatic C losses.

Line 9: 'Balancing' should be 'balances'.

Figures 1 and 2: These are very uninformative – what are all the lines supposed to show (ditches? natural streams? dams in figure 2?). I would have liked a far more detailed map showing (for example) the boundaries of the peat area, field boundaries, location of pumps, elevation, geographic location. In figure 2 the lake is obviously just a hand-drawn oval, and I don't understand how the water is moving – does water drain from the lake into the fen (in which case surely this is part of the catchment area?) or is the lake hydrologically isolated from the fen (in which case there is perhaps no point in showing it on the figure?) Arrows showing flow lines would also be helpful.

Figures 6 and 7 do not seem to be referenced or discussed anywhere in the manuscript, and I cannot really see any relationship between the climate anomalies and the DOC concentrations. If no relationship can be demonstrated and the figures are not important enough to be discussed in the text, I suggest that they are removed. At the least, I think that these two figures could be merged.

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