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Interactive comment on “The relevance of particulate organic carbon (POC) for carbon composition in the pore water of drained and rewetted fens of the “Donauried” (South-Germany)” by S. Fiedler et al.

S. Fiedler et al.

Received and published: 1 September 2008

We would like to thank the referees for carefully reviewing the manuscript and their very helpful remarks.

The authors of the present manuscript concur with submitting the manuscript in a revised form, and wholeheartedly believe that the referees comments have helped considerably in improving the manuscript. A native English speaker additionally proofread the text.

We greatly look forward to hearing from you and hope that the submitted revised ver-

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sion of the manuscript will find your acceptance.

BGD

Anonymous Referee #1

R1: Page 4, line 7. This sentence is not true as porewater carbon has been used in carbon budgets of peatlands. ***Sentence has been changed into: The dynamics of total carbon in the pore space of hydromorphic soils and its contribution to the ecosystem carbon balance, however, remains uncertain. ***

Page 4, line 8 – Most POC moving from peatlands is sourced due to erosion of exposed peat and may not be coming from leaching, the authors seem unaware of the literature on peat erosion. ***Thank you for this comment. We have included a new reference.***

Page 4, line 18 – sentence does not make sense in English. ***Sentence has been re-written – Samples for the determination of microbial colonisation of POM particles were taken twice in autumn at the moderately drained fen (MDF) at 20, 40, and 60 cm depth.***

Page 6, line 7 – do the authors really mean “introduced”; ***The sentence has been rewritten.***

Page 6, line 18 – sentence does not make sense in English. ***The sentence has been rewritten.***

Page 7, line 1 – sentence is very poor style. ***Sentence has been rewritten.***

Page 7, line 7 – there are unexplained acronyms in this sentence – please detail. ***FID has been changed into Flame Ionization Detector, and ECD into Electron Capture Detector***

Page 7, line 8 – ppm is shorthand and should be removed and mg/l used if that is what is meant. ***Thank you for this comment: ppm has been converted into $\mu\text{g l}^{-1}$ ***

Results section – remove acronyms in bold and make sure headings are con-

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sistently formatted. ***Corrected.***

Page 9, line 9 – is this effect significant? There are too many statements made without any idea whether the observations are significant or just believed to exist by the authors. ***No significance was observed and the sentence has been deleted. All trends have been deleted as well!***

Page 9, lines 12-13 – is there a new paragraph here? ***Yes***

Page 9, line 25 – insert the word “individual” before POC. ***Corrected***

Page 10, line 1 – sentences should be merged. ***Corrected***

Page 10, line 6 – this sentence does not make sense in English. ***The sentence has been rewritten.***

Page 10, line10 – is GWL defined as an acronym elsewhere? ***Yes, page 4 line 96***

Page 11, lines 13-15 – brackets are poorly positioned and make the sentences difficult to understand. ***Brackets has been changed.***

Page 11, line 20 – sentence does not make sense in English. Further, what does “tendency” mean here? ***The sentence has been deleted.***

Discussion section – inconsistent use of headings please format consistently. ***Corrected***

Page 11, line 23 – what does “most” mean here, again this is an example of lack of rigour – what proportion are we talking about? Was it a significant proportion? ***Sentence has been rewritten.***

Page 12, lines 12-15 – terrible style and also not true. In scientific writing what is a “take home message”? What are conclusions doing in the discus-

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sion? Equally, none of these are actually conclusions from this study ; they are implications or proposals from this study. ***Rewritten.***

Page 13, line 18 ; sentences needs re-writing. ***Deleted***

Page 14, second paragraph ; the authors make a lot of there not being a difference between POC and DOC but one they fail to discuss is that DOC can be truly colloidal while POC is suspended the cut off point between the two is then not arbitrary. ***Thank you, this has been included.***

Page 15, line 25 ; inconsistent table reference. ***Has been made consistent.***

Page 16, line 17 ; the reason that most of the values in the literature the authors cite are lower than those reported in this study is that most of these studies do not correct for in-stream losses and so will be artificially low, if the comparison is made with studies that correct for these losses the differences are not great and reported ranges overlap. ***You are absolutely right that in-stream losses must be accounted for; however now we mainly refer to soil solutions which also show lower values (maybe also due to different measurement system open bottles). Therefore we still have to be careful and the comparison to aquatic systems is not useful since it highly depends on were the samples were derived from ; therefore we deleted it.***

Page 17, line 24 ; how can you state assumptions in the conclusions, the authors are assuming POC and DOC are the same but that is not a finding of the study.

It is the finding of this study that POC and DOC have similar concentration which makes them similar important. Biochemically there is no reason that they should differ significantly and the definition we took $0.45\mu\text{m}$ does also not differentiate between truly colloidal and suspended ; real suspension begins at maybe 1 to $2\mu\text{m}$ (nobody really knows) and highly depends on physicochemical properties of the substance and therefore a definition separating between colloidal and suspended is not possible.

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Referee 2 (Glatzel)

I recommend changing the title to Pore water carbon fractions in drained and rewetted fens in S Germany. The reasons are: The relevance of POC is only touched in your contribution. What you are mainly reporting are fractions, which is good enough. You also give an outlook on the relevance, but not more than that. The fact that you are talking about a fen in S Germany is sufficient for the title. The reader will learn about the Donaumoos in the text. ***Please note: The paper mainly deals with POC in pore water of fens in the Donauried! The title has been changed to: Particulate organic carbon (POC) in relation to other pore water carbon fractions in drained and rewetted fen in Southern Germany***

The introduction is swift and gets to the point quickly, maybe too quickly. I suggest adding a sentence or two on why the size limit of DOC particles has been set to 0.45 micro m. This is an issue you come back to in the discussion. ***We inserted the following paragraph: There is no unified definition separating DOM and POM. For the hydrosphere and pedosphere, DOM is commonly defined as organic matter in water samples smaller than 0.45 μm (Thurman, 1985). However, POM is frequently defined as organic matter larger than 0.7 μm (Hope et al., 1997; Dawson et al., 2004). Consequently, given the two frequent definitions, there is a gap in particles between $> 0.45 \mu\text{m}$ and $< 0.7 \mu\text{m}$. Therefore for this study the definition of Zsolnay (2003) was applied separating DOC as particles $< 0.45 \mu\text{m}$ and POC as particles $> 0.45 \mu\text{m}$.***

I have a couple of suggestions on how to improve the results section: First, I would delete the sentence on POC concentrations and distance to water table (p. 2057, l. 15-16). I see no evidence for such a trend. Why is there no graph on the temporal course of the concentrations of C the control (deeply drained fen site)? ***We are not

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agree with the referee. There are clear trends between groundwater fluctuation zone and POC concentration. Graph on the temporal course has not been inserted.***

The materials and methods section is fine. Please add a sentence on the locations on delta13C sampling. ***The information has been inserted now.***

Chapter 3.2.2. needs to be improved: The expressions a rough calculation and in tendency are, especially in the absence of numbers, inappropriate. It would be interesting to learn where you found bacteria and where archaea. Suddenly, filamentous fungi are mentioned. I have the feeling that all that you know is that the surface POM is colonized by different microbes. So if this is so, say so. ***The paragraph has been rewritten***

Discussion: I suggest starting the discussion with POC occurs; and consider deleting the take home message part, which is quite colloquial. I am surprised that you expect POC depletion relative to DOC. While it is certainly true that POC particles are sedimented, could it not be that DOC is preferentially respired? So please strengthen your argument with appropriate references. ***Paragraph has been rewritten.***

In case the upper size limit for POC is abandoned, POC will depend on water temperature and flow velocity. Is this really such a good idea? Please discuss or delete this suggestion. I also think that your suggestion that POC may function as an important shuttle for C goes too far. You do not have any data supporting this. So please find a more careful expression. ***If POC without an upper size limit varies with temperature and flow velocity, why should this be different for temperature and POC with a discrete upper limit? I do not understand the comment. For flow velocity, I do not see so much why this should be relevant in the soil pore water of our site with a low flow speed. This is more complicated in open water. Just mention the caveats. Can you quote Hendriks et al for DOC losses and assume that POC may be the same magnitude?***

Why must CH₄ be considered for the understanding of C turnover? The amount of CH₄ is small. Probably you intend to prove the existence about anoxic conditions. Is that true? If yes, please be more precise. ***No, this was not our intention. No we

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do not intend to prove the existence of anoxic conditions. The emissions are large and amount are never an argument to stated that it is unimportant for turnover only for budgets maybe. We have inserted the following details about CH₄-emissions- The amount of CH₄ release to the atmosphere was extremely high at the wet site with more than 70 g CH₄-C m⁻² a⁻¹, (Freibauer et al. in prep.) although the small CH₄ pool was no indicator for that. This confirms the hypothesis of fast escape, which is triggered as well by the aerenchymous leaves of the dominating Typha and Carex species as chimneys for CH₄.***

Conclusion: In the conclusion, your statement on labile POC strikes me. How do you know? Why should POC be labile? ***Why should POC not be labile? The question is how labile, which we do not know. Therefore, we have changed it to active carbon pool involved in current carbon turnover (i.e. not in stable pools that have turnover rates of hundreds or thousands of years).***

Also, you say that it was not possible to derive a mechanistic model. Was that a goal of your research? So I think it would be best to delete that sentence. ***The sentence has been deleted .*** Specific comments: P 2053, L 24: Typha, not Thypha P 2058, L 20: Delete Relatively P 2058, L 24: Compared to, not than in P 2063, L 25: Has hardly been, not was hardly, exhibits not exhibited ***Has been corrected in the revised version of the manuscript!***

Figure 1: Caption: Dates of measurements: do you mean number of measurements?
We have exchanged "dates" for "number". *** Figure: Can you insert the graphs into a grid with depth as y- axis and site as x- axis? That would make the figure easier to read. ***Grids have been inserted on the x-axis.

Interactive comment on Biogeosciences Discuss., 5, 2049, 2008.

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