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Biogeosciences Discussions

Interactive comment on "Fluxes and ¹³C isotopic composition of dissolved carbon and pathways of methanogenesis in a fen soil exposed to experimental drought" *by* K.-H. Knorr et al.

K.-H. Knorr et al.

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We thank the anonymous referee for the helpful and well thought comments on our manuscript. We carefully went through the manuscript and tried to clarify the mentioned shortcomings to make the manuscript accessible for a broader audience. Below are our responses to the specific comments made:

Abstract: "I think you need a little more than the first sentence to provide the rationale and justification for the study." We included one more sentence to provide more justification for the study

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Introduction: "One thing lacking is a discussion of the role plants play in peatland C dynamics" We did not want to focus too much on a comparison of the treatments with and without plants, as there are no replicates and therefore we can not statistically prove that the effects are attributable to presence or absence of vegetation. This point was also raised by the editor. We included some more points about vegetation effects, but keeping pointing out the possible uncertainty within this comparison.

"I can understand why the authors chose this system, but some readers might wonder why they did not create bog mesocosms." The site was characterized as an acidic minerotrophic fen, having a pH of mostly 3.5-4.5 (see also Paul et al., 2006). We think that it is thus a comparable site to many other fens commonly found in northern temperate regions (Aerts et al., 1999, Chimner and Cooper, 2003, Smemo and Yavitt, 2006, Dettling et al., 2007). The reviewer is right that bog mesocosms would also be desirable for a comparable study. Maybe we could address this in another study.

Methods: "What was it about your treatments that caused Carex to increase in dominance over time?" We think that presumable the permanently wet conditions were favourable for the sedges in comparison to other graminoids present. A short sub clause was added.

"The wetting and drying treatments are somewhat difficult to follow at times. On page 1323 line 25 you state that after 40 days the water table was raised from 30 to 10 cm. How exactly did you achieve this with 30 or 40 mm of water? Why did you use different amounts depending on the treatment?" We hope that we could successfully clarify the time schedule of the drying and rewetting treatment. We also tried to better point out that the treatment was always based on the water table position. Thus, we had to apply different amounts of irrigation water on the different treatments, depending on the corresponding water table response. One could of course also use similar amounts of irrigation water in the treatments. Due to the absence of vegetation in the DW-D treatments this would, however, lead to much wetter conditions in this treatment, compared to DW-V.

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"The irrigation water used approximated field water chemistry, but that is in an open system. You added it to a closed system. Do you think there was any effect of electron acceptor or end-product build-up in the mesocosms? I doubt this was a major problem, but did you measure any of this? You also used H2SO4 to alter pH? How many μmols of sulfate did you add for pH?" The first point is indeed very important and was missing. Thus we included the information that the contribution of the irrigation water to electron acceptor budgets was generally much less than 1%. To answer the second point, we clarified that the sulphate stemming from the sulphuric acid was already included into the concentration data given above.

"How many 20cm diameter collars did you put into each mesocosm?" We added that there was only one 20 cm collar in each mesocosm. This is also now depicted in Figure 1.

"I know that you already have too many figures, but I would have liked to see a diagram of what a mesocosm looked like or a photo. Not sure it is possible given the length of the manuscript." Together with the revised manuscript we may provide a schematic sketch of the mesocosms, as this point was raised by both reviewers (Figure 1). To keep the number of figures, we combined Figure 2 and 5. We have to clarify, though, if it is still possible to include one more figure.

"I am not exactly sure what data you got from the silicone tubes that you could not get from the Rhizon samplers. Did you extract porewater with the silicone tubes and strip the gas? I guess I am confused about how the silicone tubes functioned to sample the gas." We understand that the description of this method may be confusing for some readers, as this technique is - unfortunately - not very often applied. To keep the manuscript short, we could not add much more information to this point, but the reader is kindly asked to consult the reference given here (Kammann et al., 2001). The basic principle underlying this technique is to make use of the high permeability of silicon for gases, but not for liquid water. Therefore, the use of silicon tubes may be seen as an in-situ headspace technique. The dissolved or gaseous phase surrounding the silicon

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tube equilibrates with the inner atmosphere within the tube. Thus, sampling of the inner gas phase allows recalculating the dissolved gas concentrations using Henry's law and values of pH for DIC speciation. The major advantage of this method is that the gas phase can be injected directly into the gas chromatograph as obtained from the tube and no dilution or transfer is necessary. Furthermore, this method may be applied in saturated and unsaturated soil. Further explanation in this point is also provided in the Author Comment to the Review of Juul Limpens.

"Overall, the results section is very long, but much of this is due to the scope of the data and cannot be helped. However, I think this could be clarified in some areas and the grammar improved. Clarification and removing discussion points from the results could help reduce length." The reviewer was right and we tried to improve grammar and clarity of this section. All points raised by both reviewers were incorporated into the text. Thus, large parts of the results section are rearranged and rewritten now. Furthermore, discussion points were removed and included in the discussion section.

"At times, lack of consistency in descriptive terminology can be confusing. For example, the authors describe isotope values as higher or lower, increased or decreased, or more negative. I would try to be consistent with the way you describe the isotopes number as this can get confusing even for non-isotope biogeochemists." There was indeed some confusion with terminology in this part and also in the discussion. We hope that we successfully addressed this issue now.

"In Figure 1, why is actual methane emission shown as negative flux? Wouldn't negative flux mean consumption when taking chamber measurements?" According to the point of view this may be confusing, we have to admit. However, to be consistent with an earlier manuscript (Knorr et al., 2008: Experimental drought alters rates of soil respiration and methanogenesis but not carbon exchange in soil of a temperate fen. Soil Biology and Biochemistry 40, 1781-1791) we decided to assign a negative value to fluxes which mean a loss of carbon from the mesocosm. Thus emission from this point of view is a negative flux, whereas photosynthesis would be a positive flux.

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"On page 1329 line 13, you say that 'this was a value typically observed 5 cm above the water table when the water table was below'. Below what? Is that a typo or a misuse of grammar. I am not sure." This was indeed misleading and we clarified this point. Actually, we want to say that these 2 % volumetric gas content were typically observed at the uppermost soil moisture sensor (at 10 cm) when the water table level was 5 cm below that sensor (i.e. at 15 cm).

"The first sentence of section 3.6 does not make sense. Does that mean rates calculated at the point where the water table is maintained on average?" The sentence was rewritten. We wanted to say that during the wet phases the highest turnover rates occurred at those depths where the mean water table was located.

"In figure 4, what is your actual definition of negative net turnover rates? This is not mentioned." We added this information, as it is important to know that negative turnover could be either consumption but also degassing of dissolved CO2 and CH4 with the peat becoming unsaturated during dry phases.

"Figure 5 is either a bad image or has missing data. The pdf I received has large white patches that do not exist in the legend." This point also needed clarification and the information was included in the figure caption. A white patch just that there is no data available for CH4 isotopic composition, as the concentration was too low to be measured at the Isotope-ratio mass spectrometer. This was for example the case during drought, when CH4 concentrations strongly declined, i.e. on days 100-150 in the upper soil layers. Smaller white spots during phases of high water level arise from a lower data density of the isotope measurements. This is also the reason why the interpolation is not that smooth as for concentration data.

"Figures 4, 6, and 8 should also denote which wetting/drying phase each of these days represents. Is there a reason that these three graphs have different measurement days on the top? Perhaps getting rid of days and noting the phase is a better option." As the exact days of measurement are not as important as the actual phase of the experiment,

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the reviewer is right that stating the phase only would improve clarity of these figures. This was changed accordingly. We also forgot to mention in the figure caption that the dotted lines denote the actual water table at the time of measurement.

"Figure 7 is a very nice figure, but it might improve the clarity if you cold denote which metabolic pathway the different fractionation factors are associated." As this is a very important point, we added this information to the figure.

"The last sentence of the Results section should really go in the Discussion." This sentence was moved to the discussion.

Discussion "Although the Discussion section has a lot of excellent information, it is the weakest part of the paper. This is mostly because grammatical clarity starts to break down and it is divided into a series of small sections. The section does not flow very well as a result. I would even consider getting rid of sections and writing one cohesive discussion section that tells a story, or at least rethink how it is organized or how you name your sections." The reviewer is right and there was unfortunately still much to improve in the discussion section. Large parts of this section were thus completely rewritten or rearranged to improve clarity. The number of sub-headings was reduced and parts were combined to get more cohesion in this section. Finally, we asked two native speakers to cross-read the article and included their suggestions. We think that the manuscript did indeed benefit a lot from reworking the discussion.

"I dislike the way the Discussion begins by naming the key findings of the paper. You do that in the conclusions section, which is where it belongs. I would start off by stating the most significant overall finding of the study, and then build from there." This may probably have been too much conclusion in the beginning of the discussion, this is correct. Thus we deleted some of the conclusive remarks here and moved them to the conclusion.

"The focus of the Discussion is centered around the drying and rewetting effects, with little focus on the defoliation treatment. This seems to be an important point that is

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downplayed." As stated above, we weakened this point due to a lack of replicates. We discussed this point also with the associated editor. We may include some interpretation on this point but it will, unfortunately, remain somehow speculative although for sure interesting and plausible.

"Support for the idea that there is not isotope fractionation during breakdown of organic matter is interesting. Has that been shown to be the case with controlled studies looking at oxidative and hydrolyzing enzymes?" The reviewer is right that this would be an interesting subject to study in more detail. However, in the studies available to date support to this idea is so far only given by the fact that the isotope mass balance of the isotopic composition of the organic matter and the CO2 produced is closed and no enrichment of a specific isotope in the CO2 produced was observed. We cannot say whether this holds true also for different intermediates. According to present studies about variations in isotopic composition of different cell compounds of plants this seems rather unlikely. Nevertheless, the isotope fractionation during methanogenesis is exceptionally high, in the range of 20-90 per mil, and thus little affected by slight differences in the substrates, mostly being in the range of a few per mil. In our data such a fractionation is not visible, as we would rather expect CO2 to be lighter in isotopic composition compared to the organic matter.

"In the last line of page 1339, do you mean less methane emitted, or the methane was less depleted?" This was indeed misleading. It was changed into 'emitted methane less depleted in 13C'.

"On page 1342, lines 8-14 make no sense. What is the 'latter' that is being referred to?" This was probably a left-over from an earlier version. We wanted to say that in contrast to other studies where mostly acetoclastic methanogens dominated in the upper peat layers, we found that in our peat most likely hydrogenotrophs were the dominant methanogenic bacteria also in the upper peat. The sentences were changed accordingly.

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"This section is decent, but I do think it also needs to include some discussion of the implications that you laid out in the introduction. What will climate change do to peatland C cycling and methane dynamics based on the findings of this study?" The reviewer is right. The conclusions were thus rewritten to a large extent.

We want to thank the reviewer for a lot of very helpful and constructive comments on the manuscript. We hope that we could successfully address all the shortcomings. The manuscript did really benefit a lot from the reviews.

Klaus-Holger Knorr on behalf of all authors.

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