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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.

Anonymous Referee #3

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General Comments

Knorr et al. present results from a peat mesocosm experiment where the authors used controlled conditions to simulate the influence wetting-drying and vegetation cover on C flux and methane dynamics. The authors utilize a limited study design from a statistical standpoint (n=1 for each treatment mesocosm), however the rigorous methodological approaches and sampling design make up for the lack of replication, and in my mind, makes this a seminal study of peatland biogeochemistry that transcends the questions related to wetting and drying. Using multiple approaches to separate microbial path-

ways of methane production and source-sink relationships (i.e. mass balance/turnover, isotopes, and thermodynamics calculations) is the primary strength of the manuscript and the authors should be applauded for a well-thought out and executed study. That being said, this is a very dense manuscript with a lot of information that can be hard to follow for all but the seasoned wetland biogeochemist or microbiologist. Not to say that the manuscript is difficult to follow, but it is difficult to keep track of all of the different approaches, results, and terminology, and would benefit greatly from some thoughtful organization of the methods, results, and discussion sections. Moreover, the conclusions section simply reiterates many of the key findings (something already done at the beginning of the Discussion section). I think this section needs to discuss more of the implications of the findings and link the conclusions back to the main points of the introduction: i.e. the impact of drought and re-wetting on peatland C cycling and storage. The manuscript has a few too many figures and tables, but those figures are information dense and none are gratuitous. Thus, I would keep all of them if page limitations allow. It might be possible to merge a few figures, but that might reduce clarity. Overall, this is a very interesting and well-written paper. Some re-organization and grammar/typo reviewing in order to appeal to a broader and more general audience is the primary need at this point. The grammar is quite good overall but seems to become a little more problematic in the discussion, which needs the most re-organizing and proofreading.

Specific comments

Abstract

The abstract is very good overall, but I think you need a little more than the first sentence to provide the rationale and justification for the study.

Introduction

- The indroduction is quite good overall. A very nice literature review. One thing lacking is a discussion of the role plants play in peatland C dynamics. This effect of plants is

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one of the primary treatments, yet little is discussed about why you chose a defoliation treatment.

- Of the major peatland types that are significant C sinks and methane sources, why did the authors choose a poor fen? Particularly a fen with a fairly high mineral content. This is not mentioned in the intro or the methods, as far as I could tell. I can understand why the authors chose this system, but some readers might wonder why they did not create bog mesocosms.

Methods

- What was it about your treatments that caused Carex to increase in dominance over time?

- 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?

- 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 umols of sulfate did you add for pH?

- How many 20cm diameter collars did you put into each mesocosm? I assume not many in a mesocosm that was 60 cm diameter.

- 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.

- 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

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the gas? I guess I am confused about how the silicone tubes functioned to sample the gas.

- The authors did a very good job at laying out all of the calculations they made, the equations they used and the underlying assumptions.

Results

- 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.

- 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.

- In Figure 1, why is actual methane emission shown as negative flux? Wouldn’t negative flux mean consumption when taking chamber measurements?

- 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.

- 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?

- In figure 4, what is your actual definition of negative net turnover rates? This is not mentioned.

- 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.

- Figures 4, 6, and 8 should also denote which wetting/drying phase each of these days

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Interactive Comment

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.

- 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.

- The last sentence of the Results section should really go in 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.

- 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.

- 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 downplayed.

- 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?

- In the last line of page 1339, do you mean less methane emitted, or the methane was less depleted?

- One of the most interesting findings in the study is the lack of agreement between different methods to estimate pathway contributions to methane production and the in-

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ability to reconcile observations with thermodynamic predictions. This is not a criticism or a weakness. It is interesting and tells us that we need to either improve our in situ measurements of both process rates and concentrations at varying spatial and temporal scales, or alter our assumptions. The authors have appropriately demonstrated an important frontier in biogeochemistry.

- On page 1342, lines 8-14 make no sense. What is the “latter” that is being referred to?

Conclusions

- 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?

Interactive comment on Biogeosciences Discuss., 5, 1319, 2008.