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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.

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The authors present a 300 day lab incubation study on the effects of one drying and rewetting cycle on the C dynamics of 3 soil columns taken from a fen. The columns, comprising two vegetated columns and 1 defoliated column, were subjected to 2 different hydrological treatments (constant at minus 10cm, or c. one month of water table draw down to minus 55cm) The authors combine an impressive number of approaches, such as turnover and flux calculations, changes in isotopic composition of CO2 and CH4, isotopic budgets, changes in isotopic fractionation and thermodynamic

calculations to elucidate the effects of drying and rewetting on C fluxes and their isotopic fractionation as well as the mechanisms and pathways involved in belowground CH4 production and oxidation. At the end of the experiment, 13C pulse labelling was applied to identify the zone of main root activity in the columns. The high spatial and temporal resolution of the measurements as well as the different approaches used, fully compensate for the absence of replicates in this study.

The study is well thought out and improves our knowledge on the small-scale spatial and temporal processes involved in methane production. The study also challenges the predominance of the acetoclastic pathway in the upper peat profile.

The abstract and introduction are clear and well written, the rest of the paper, particularly the materials and methods section could be improved. I must admit however, that my knowledge in this field is limited, and some of my remarks may be due to unfamiliarity with the techniques or jargon. Still, adapting the text style/ wording to a less knowledgeable reader, might broaden the accessibility of the paper to a wider public, as the special issue aims to do.

General remarks.

Methods: after reading the methods section I was left with quite a number of questions regarding the experimental set-up and the measurements. A schematic drawing of one columns with inserted TDR probes, silicon tubes, irrigation device (?), piezometers, 2 (?) gas collars and rhizons at different depths would be most welcome. In addition I suggest arranging the text into 2 subheadings: 1) experimental set-up (explaining the requested figure)& incubation conditions and treatments and 2) measurements, with a bit more information on the sampling procedures involved (see questions below).

What was the vegetation prior to the defoliation treatment? Was it similar to the other columns?

Just curious. What was the cover of the Sphagnum? Was there any within your

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methane measurement-collars? Generally, presence of Sphagnum increases liklyhood of methane oxidation before efflux.

Can you describe the peat a bit more? From your carbon content data it looks as if there was quite some ash content/ mineral influence.

How was the irrigation water supplied: from above or from below, with a dripping device, or something else?

Did you check the concentrations of the elements applied with the irrigation water? Was there no accumulation? Can the electron acceptors (NO3, SO4) applied with the irrigation water have influenced methanogenenis ?

Was the irrigation done by checking the water table depth in the piezometers? Or by weighing?

Please move the information on the relevance of the drying and rewetting treatment from page 1336, paragraph 4.2 to your methods. This avoids leaving the reader wondering about this for a couple of pages.

Please give the frequency and depth of all measurements. How often was soil moisture sampled through the rhizons? Was sampling still possible during the dry period in the DW treatments? What was measured in the soil solution? And how? What was the pH of the soil solution? Also 4.8? Did it change as a result of drought? What I always like is when the reason for the main measurements is explained; this is a question of personal taste I guess.

About the silicon tubes. I assume they were installed permanently? I was wondering about the following. You indicate that the gases in the silicon tubes were in equilibrium with the soil/gas solution surrounding the tubes. The gas diffuses from the surroundings into the tube. Is there any chance of a kind of fractionation to occur with lighter isotopes diffusing faster? I can imagine that heavier DIC is likely to be in the form of HCO3- whereas lighter C shifts to the CO2 form and diffuses. Did you check this

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maybe with measurements on the water itself? Associated with the above: As you did not measure CO2 directly in its dissolved phase, but rather indirectly, I would not put DIC so prominently in the title. I suggest focussing your title on the main research aim: the mechanisms and pathways of CH4 mobilisation/ turnover/ cycle after a drying and rewetting cycle. Most other things seemed to me tools to answer this question.

How exactly (and when: before/ after experiment) were the C, N contents and the porosity determined? Where they taken as a small core in each column and then seperated into layers? And/or were the taken around each TDR? How did you manage to get 100 cm3 samples without disturbing the water-soaked soil? Did you freeze the columns/ cores before cutting?

Can you give an indication of the accuracy of the TDR probes? (in my experience, very wet and very dry are a bit difficult).

How was this pulse-measurement done exactly? Were the columns measured simultanously? Or after each other? Was the gas mixture applied before or after the chamber was placed on the vegetation? The text now suggests before. If this is correct, how did you prevent loss to the atmosphere?

Being a bit unfamiliar with the procedure, I was wondering about the calculation of the anaerobic CO2 flux page 1327 (lines 7 & onward). Could you perhaps elaborate a bit more on why you would want to calculate it, why you call it an anaerobic flux? Is this calculated over the whole experimental period, with the fractionation ratios taken from each experimental phase and layer? How does the mass balance cope with changes in pools? Such as acetate or uptake by vegetation? Or can we assume that this is negligible over the whole period? As it is now I find it rather speculative, both as a calculation and as a major result worth mentioning in the abstract.

Results I was wondering whether you could consider omitting one or two figures, as the paper is rather figure heavy at the moment. Perhaps figures 1 and 3 could be discussed in the text? In addition, could you reduce the overlap in information between

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the results and the discussion?

I would suggest using the following order when discussing results. 1) Major differences between columns. 2) Then, chronologically, the changes within each treatment as it goes through the 4 phases. Where applicable from deep layers to surface (or other way around). At present the order of descriptions varies between measurements: I find that a similar order of description facilitates quick reading of a text.

I suggest adding the following information in the figures/figure legends. Flux direction to atmosphere/soil (Fig 1), Measurement frequency (Figs 2, 3 and 5; alternative option, convert to line graph, with one graph per depth to facilitate comparing treatments), Phase (almost all figures), In figs 4, 6 and 8 I would also indicate the phases in the figure instead of (only) the measurement dates. Why did you specifically select those days? Perhaps you can elaborate this in the methods? Why do the days between the figures not match: Figure 4 has different days than figs 6 and 8.

Figure 1: out of curiosity: were those independent measurements from 2 different collars? Can tell something about the variability? Figure 4: Out of curiosity: the high methane concentrations measured at -5 cm in DW-V above the water table: is this the only point where you measured methane production in the unsaturated zone? This is also the layer where your C content is quite high (table 2). Is the porosity there also smaller?

Could you perhaps arrange figures 2 and 5 a bit closer to each other so that you can compare more easily? Why is the resolution of figure 5 smaller than that of 2?

I suggest indicating the fractionation factor range of the different methanogenesis pathways in figure 7.

Discussion At present the discussion is very much chopped up into different paragraphs with in-depth discussion regarding very specific topics: some information is used more than once. To my mind this obscures the overall synthesis and main find-

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ings. Perhaps you could focus it more along the research aims (the effects of drying and rewetting on C fluxes and their isotopic fractionation as well as the mechanisms and pathways involved in belowground CH4 production and oxidation) as stated in your introduction.

I suggest checking the text carefully regarding overlap with the results.

Perhaps you could discuss/mention the role of pH as a reason for observation differences with bog-studies.

How would/could the ability of shifting to Fe-reduction by methanogens affect your fractionation results (since you mentioned it in your introduction, but did not really come back to in your discussion?

Could you add a cross reference to the paper by Schrier et al (this special issue, also in Biogeosciences discussions), when discussing methane production/fluxes measured in the field?

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