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Biogeosciences Discussions

Interactive comment on "Belowground in situ redox dynamics and methanogenesis recovery in a degraded fen during dry-wet cycles and flooding" *by* C. Estop-Aragonés et al.

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Referee 2 The authors present a comprehensive report on the changes in concentrations of redox-sensitive compounds (NO3-, Fe 2+, SO42+) and methane and DIC (CO2, HCO3-) concentrations following in situ natural and enforced drying and flooding in a minerotrophic peat in Germany. In situ studies with high spatial and temporal measurement densities are rare and invaluable in the upscaling of lab processes to field conditions. Nevertheless, in it's current form the paper gives too much detail (i.e. 14 figures) to communicate it's main conclusions: 1) that methanogenesis occurred above the water table position in the presence of low concentrations of SO4 - giving

field evidence of the importance of decoupling of redox processes in protected microsites, 2) that only a small part of the electron flows could be explained by changes in soluble electron acceptors pointing towards an important role for the solid phase electron acceptors and/or to DOC as alternative electron acceptor at relatively low pHs, 3) that electron acceptor availability interacted with substrate quality to explain the recovery of methanogenesis after drought.

General comments

The paper would improve by:

A careful selection/recreation of 5 clear figures (max) that support the main findings, with maybe a number of supporting graphs in an appendix.

Application of statistics – even if only some (partial?) correlations. Now ALL conclusions are based on visual comparison of graphical patterns and differences of patterns between plots.

Response: Satisfying this demand requires a rewriting of the manuscript with a complete change of its current structure. The visualization of the patterns provides raw information to the reader to appreciate the relation between peat properties and dynamics of processes considering the differences in WT between and within treatments. We plan to reduce the number of figures and move figures 2, 3, and 12 from the main text to the appendix. We consider it necessary to maintain the figures 4 and 5 in the main text as oxygen is a critical variable for the response of other variables and DIC is used for the mass balances.

A clearer indication of the new contribution of this paper relative to the other two-three (?) preceding papers (Knorr et al. 2008, 2009, Estop-Aragones et al. 2012). Now the reader is left wondering whether the contribution of this paper is mainly in presenting all data together, particularly since 5 out of 14 papers are reprints.

Response: We plan to emphasize that the study characterizes the delay of methano-

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genesis after different drought intensities under in situ conditions as well as investigates the effects of flooding in fen peat. The only overlap with previously shown data is with Estop Aragones et al. (2012), where the oxygen dynamics was analyzed in detail. Knorr et al.'s earlier papers were based on data from controlled mesocosm experiments and in one case on field data from the same site but a time period before the current study was conducted. In that paper the points mentioned above could not be adequately addressed and the spatial and temporal resolution of the concentration data was much lower.

A stronger emphasis on (dis)similarities between rates/patterns in this study and others (lab/field) and what this tells us about methanogenesis in (fen)peat soils.

Response: We agree that comparison of the results with other studies may appear hidden due to the data load and manuscript length. Nevertheless, we refer to related findings from other studies to compare or establish a link with our findings. We address the effect of drying intensity on methane and cite studies related to this topic (Kettunen et al, 1999; Dowrick et al. 2006). We also compare our sulfate concentrations and establish a relation with findings from sulfate additions in mesocosms (Watson and Nedwel, 1998: Wieder, et al, 1990). We mentioned the potential role of humic substances as a source for our unexplained O2 consumption/CO2 production and link in a quantitative manner with previous findings in the lab (Aeschbacher et al., 2010; Roden et al, 2010; Heitmann et al., 2007). We also compare the flooding effects in our fen site with those from bogs (Hines et al., 2001; Duddleston et al., 2002; Shannon and White, 1996).

One of the main conclusions of the MS is that the size of the electron acceptor pool generated during a given drying did not affect methanogenesis recovery after rewetting. This is contrary to general assumptions and contrary to your own hypotheses as well. For me it is unclear how you arrived to this conclusion. Reading the results (332-345) gives the impression that the conclusion is based on comparison between plots (plots with most electron acceptor generation were not the ones with the most methanogen-

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esis delay). But is this the right way to test the hypothesis? I always thought that this statement implicitly assumed that other conditions (for example substrate availability) were comparable. So wouldn't it be better to look at within-plot-within year comparisons: i.e. comparisions under conditions where the soil physical structure and general composition/ position within the fen are the same. In the latter case we of course assume that the rate-supply of root assimilates by plants are roughly comparable.

Response: One of the main conclusions is that peat properties TRIS and OM content control the sulfate pool generated during drying and the recovery of methanogenesis after rewetting, respectively. These "comparisons under conditions where the soil physical structure and general composition/ position within the fen are the same" referred by the referee are not feasible since these properties strongly differed from plot/profile to plot/profile (in addition to the WT differences). These properties were related in our study. If there is a cause-effect relation for this correlation is speculative but one might argue that increasing OM content leads to a higher organic bound sulphur in peat (which is the highest fraction of sulphur in peat) and that might be related with the higher TRIS fraction .

Specific comments: General Please check use of abbreviations throughout paper: I had the impression some abbreviations were not written once in full

Response: We corrected the abbreviation AFP (air filled porosity).

Introduction You describe what research has been done so far but not so much what we do not know and how your study provides/will provide this information

Response: We include the sentence "little information exists about the effects of drying intensity on postdrought methanogenesis".

77-78: ..it is thus important...to what end is this important? I assume to assess potential impact of weather extremes on methane production, an import greenhouse gas..

Response: Added

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84 – replace ‘was’ with ‘is’ Response: Added 85 – please rephrase – be more specific than “it has not been reported for a wide range in..” Response: It was substituted by “and the redox zonation in peat soils undergoing dry-wet cycles and flooding has not been investigated in detail” 92 – add one sentence explaining why we need this knowledge Response: We added “to explain the electron acceptor sources associated to CO2 production” 95 – why a degraded fen – is this representative? Response: We mention degraded because the site appears to have undergone peat extraction. As well in western and central Europe nearly all of the fens have undergone degradation. In that sense degraded fens are indeed representative although of course different states of degradation may occur.

95 – background conditions – can you be more specific? Response: Replaced by “natural conditions”

Methods You used enforced drought and flooding. How do these treatments compare to what is naturally observed in these systems over a longer time span: a drought that happens once every ten years or so? I.e. how representative are these manipulations of the fluctuations that can be found for these sites ?

Response: The frequency of occurrence of these extreme events is difficult to quantify. Based on our observations on the site drying occurs seasonally each year but the event is either shorter or with a shallower WT decline. We plan to mention the maximum differences on annual precipitation over the last years in the site to reflect how representative is the drying manipulation. Regarding flooding, this is an exaggerated event for this particular site considering the amount of water discharged and the slight slope surface which would prevent/reduce water accumulation. We add the sentence “The flooding was useful to observe in situ processes in peat soils under continued anaerobic conditions but it must be noted that such a prolonged flooding event is unlikely to occur in this particular site under natural conditions considering the slight site slope and the water discharged”.

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120 - You give a general description of the vegetation of the site: but what about the vegetation in your plots? Was the plant species composition the same between your measurement area's? reason: plant species may have widely different rooting patterns and root exudates

Response: We detail differences in vegetation in the site and mention that samplers were installed in the narrow hollows between plant cushions. We rephrased as follows: "Vegetation is dominated by vascular plants. In its north-western part vegetation comprises mainly Carexrostrata and patches of Sphagnum fallax, and Nardusstricta, Agrostis sp., Molinia caerulea and Eriophorum vaginatum dominate towards the south-east. Samplers and sensors were installed in the narrow hollows covered by decaying litter from vascular plants."

120 Mollinia –>Molinia (one el): Note: vegetation suggests to me an acidified situation – what was the pH?

Response: This was replaced and pH value included (4.8).

129-130 Where were the measurement points actually situated relative to the plot edges? Close or far from the gravel ditches? How close were the plots relative to the tree-line? I.e. were some plots shaded or not?

Response: The samplers were installed at about 2 meters far or more from the gravel ditch and at about 1 meter or more relative to the plot edges. In figure 1 the distance of the plots relative to the tree-line is displayed. Shading effects by trees on the plots depended on the time during the day; plots on the west were the first ones to receive sunlight. We consider this information too specific to be included in the text for the temporal scale we investigated.

129 (and elswhere): perhaps also give dates for the beginning and end of the measurement periods to help people with the DOY notation.

Response: We included these dates

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135: The discharge water – how did it relate to the pore water composition in the fen, particularly in respect to concentration of alternative electron acceptors? Perhaps also indicate the dates of the flooding

Response: We add the in the final part of the sentence “and implied an input of electron acceptors in peat”. The dates of flooding were included.

150: please include measurement frequency for all measurements

Response: Sampling frequency is shown by arrows in figures. We added in the text that samplings were “usually weekly (2008) and biweekly (2009)”.

187: please indicate here your assumptions regarding lateral flow of water and concomitant lateral supply of solutes - You indicate the fen is a (moderately) sloping fen (methods), yet lateral water flow was not an issue (Warren et al. 2001 - discussion)– can you please explain this a bit more? A very important assumption that you make with your electronflow budgets is that your main transport is from above to below, assuming no lateral changes .

Response: We include this important assumption in the methods section as follows: “It must be also noted that this approach assumes that there is no advective transport but restricts the mass balance to vertical diffusive transport.” Probably we underestimated overall rates. Despite the absolute values may not be correct, we expect the relative numbers should be right considering that the transport processes were identical for all solutes. We also consider that in our highly decomposed peat close to the surface this may not be as critical as in other uncompated materials with higher hydraulic conductivity. In the discussion we rewrote “Sulfate might be rapidly flushed out from peat soils upon rewetting events but it is not likely to have been important in our peat deposit since sulfate release was relatively well balanced by SR (Fig. 13), it is fed by confined streams and the WT raise was not persistent and neither lead, under natural conditions, to surface flow. These conditions have been shown to influence the relevance of sulphate export (Warren et al. 2001).”

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211-215: Perhaps split into two sentences

Response: It was splitted.

Results:

You describe a lot of concentration/ rate ranges in your results. Perhaps you can consider adding a table with these ranges and focussing on the main message in the text? Such a table also makes it easier to compare the sizes of the different processes taking place.

Response: We show in figure 4 temporal phases related to different hydrological conditions and use these phases to report time-averaged rates in the figures 13 and 14.We designed it this way so that the reader can trace the conditions for each plot and for each period of time. Since we decide to maintain figures 13 and 14, adding a table would be redundant.

Figure 12: In my opinion this is too complex: try to keep the meassage per figure restricted to maybe 2(3) things.

Response: This figure will be moved to the appendix.

Personally I would prefer more scatterplots with maybe envelope curves (quantile regression) and less “compare pattern of plot so and so between figure x and figures y and z”. In my opinion this facilitates extracting the main patterns/drivers more than the current representation .

Response: As mentioned previously this implies a strong change in the structure of the current version and adding more figures would make the manuscript even more complex to read.

Discussion:

Please shorten

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I suggest to avoid results here as much as possible.

Response: We revised and eliminated sentences to avoid results and combined parts of sentences. As an example, we eliminated in section 4.1 the following part: “Based on the SO42- accumulation during drying and on the greater Fe2+ accumulation upon rewetting, electron acceptor release increased with more severe drought. The resulting electron acceptor pool ranged from 170 $\pm$ 31 μmol SO42- L-1 in dry periods lasting about 60 days with a WT decline to around -40 cm (2009 C plots) to 479 $\pm$ 308 μmol SO42- L-1 in drying with similar duration but deeper WT decline to -80 cm (2008 D plots) (Fig. 7 and S1). Shorter events of about 35 days with intermediate WT decline to about -60 cm led to intermediate sulfate release of 279 $\pm$ 153 μmol SO42- L-1 (2008 C plots).” I would prefer more a focus on the broader implications of the results and less a comparison of what has been measured before on this same site – the balance seems a bit off now.

How do changes in plant productivity factor in your results? I.e. more plant productivity means more autotrophic respiration (source of DIC?) but also more root assimilates into the soil –> food for the saprotrophs. This likely contributed to the differences between the years as well.

Response: The degree of influence of vegetation on the observed dynamics we cannot evaluate with the current data set as we did not monitor plant biomass or root biomass (and their changes) nor differentiated between autotrophic and heterotrophic respiration.

441, 512 – perhaps also express in %, instead of only in mmol equivalents?

Response: This has been included and expressed in % (relative to total O2 consumption).

492-495 – where is the stats to “prove” this?

Response: We plan to include in the appendix a figure supporting the relation of TRIS-

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Sulfate and of Organic Matter-Postdrought CH4 with differing drying intensity (S4).

548 : Add' The' to sentence

Response: It has been added.

717: was your site really Carex dominated? Or was carex one of the species present (as the methods sugest?)

Response: The expression "Carex dominated" has been eliminated and the sentence rephrased as follows "The provision of oxygen in soil via roots of Carex spp. has been demonstrated (Mainiero and Kazda, 2005) and could account for additional recycling of electron acceptors in our site."

Conclusion:

Please shorten.

Response: We will shorten the conclusion and address more specifically the main findings.

Interactive comment on Biogeosciences Discuss., 9, 11655, 2012.