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Discussions

Interactive comment on "High soil solution carbon und nitrogen concentrations in a drained Atlantic bog are reduced to natural levels by 10 yr of rewetting" *by* S. Frank et al.

Anonymous Referee #2

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The study by Frank et al describes differences in porewater carbon and nitrogen species in near-natural, drained (agricultural) and re-wetted (ex peat extraction) sites in Northern Germany, with a primary focus on the effects of drainage status on DOC and DON loss. I found the paper to be interesting, and to contain results with significant implications. In particular there has been much debate regarding the implications of peatland drainage and re-wetted for DOC loss (e.g. for IPCC 'waterborne carbon' accounting) but rather little robust field data to actually demonstrate whether or not these effects occur. For this reason I believe that the paper merits publication, but I have a number of concerns and recommendations for improvement, as follows:

1. Information about the sites is inadequate, and this makes it difficult to interpret

whether or not they are truly comparable. A site map would be helpful, as well as better information on the location of individual sampling points, their vegetation cover, land-management and water table management. For example the drainage of 'RW' is described in Table 1 as 'polder', but this is not explained until halfway through the results, information on management practices at the 'IG' and 'EG' sites is only briefly summarised in this table, and the presence of a floating peat layer at the RW site is again only mentioned in the results section. What is meant by 'organic sediment' in Table 2? My greatest concern here (and in fact my main concern for the whole study) regards the comparability of the two grassland sites with the re-wetted former extraction site. There is an implicit assumption that differences between the sites are mainly related to drainage status, but it is clear that the grassland sites have received large nutrient inputs (slurry, perhaps fertiliser) that have clearly influenced %N in the surface peat horizons, and could be equally or more important for DOC quantity and quality, as well as for DON, NO3 and NH4 leaching. In contrast, the extraction site would have received no extra nutrient inputs, and the removal of surface vegetation may even have impoverished it. This is fundamentally to the major conclusion of the paper described in the title, namely that dissolved C and N levels have 'reduced' after 10 years of re-wetting - but can the authors demonstrate that they were really similar to those in the IG or EG sites in the past? Data from a drained extraction site or a re-wetted grassland site would be helpful here. On a more positive note, the comparison between IG, EG and NN sites seems robust and the differences are very clear – perhaps the major (and still important) finding of the study is actually that DOC and DON losses have increased from drained grasslands versus natural peatlands? For me, the RW data suggest that this may be reversible, but more work would be needed to demonstrate this fully.

2. References to the 'enzymic latch' as an explanation for DOC behaviour are not backed up by any evidence from the study, or adequately explained. The evidence from Freeman et al (2001) referred to decomposition processes leading to CO2 emissions, whereas the net effect of drying on phenol oxidase activity and subsequently DOC concentrations is less straightforward – see e.g. Toberman et al., Soil Biol Biochem 2008.

The Worrall et al. (2007) study cited in page 15823 did not include any enzyme measurements, so cannot be considered as definitive evidence for this mechanism. Unless the authors can provide supporting evidence, strong statements such as this one, or the line on P15822 which suggests that drying has activated phenol oxidase and 'led to elevated DOC concentrations' are essentially speculation and should be avoided – indeed it may be better just to refer to the effects of waterlogging on decomposition and DOC production in a more general sense.

3. The possible influence of organic matter inputs to the grassland sites (especially IG) may need greater consideration. Is it possible that higher DOC outputs are partly just a consequence of larger inputs, e.g. from slurry? The observation that DOC concentrations at IG are highest in the surface horizons, and decrease with depth, would appear potentially consistent with this. The interpretation of DOC quality (e.g. on P15830) attributes differences to decomposition processes, but does not consider whether it could also be partly due to the direct throughput of qualitatively different organic matter applied at the surface.

4. The manuscript contains a large amount of data, information and discussion. The key messages might be clearer if less material were included.

5. The manuscript would benefit from a modest amount of English language editing, to avoid occasional lack of clarity.

Minor comments:

P15810: The 'peeper technique' seems like a nice approach, but is not adequately explained on first usage (in the abstract). Perhaps omit this until the main text?

P15812, line 11: Also photodegradability

P15814, line 11: Could also refer to IPCC wetland supplement and references therein

P15814, line 18: 'Assume' should be 'hypothesise'.

P15816: Need to explain where peepers were installed, and why some procedures were followed – e.g. why chambers were filled with de-ionised (not 'dionised') water, and why different peepers were deployed with different vertical resolutions

P15817: Why was a wavelength of 280 nm used? 254 nm seems to be more widely used as an indicator of aromaticity.

P15820, line 14: Could higher pH (not 'PH') values also be explained by fertiliser or lime additions to the grassland sites?

P15820, line 17: I think 'nutrients' should be 'solutes', since the high EC is also partly explained by SO4, and presumably base cations.

P15821, line 24: Do the authors have any information on evaporation rates from the sites? If this were higher from the IG or EG sites this could help to explain differences in concentrations vs the NN and RW sites. It would also be very helpful to know something about this so that some inferences can be drawn regarding the implications of these results for overall DOC fluxes from the sites – if evaporation rates are not changed by management, the increase in DOC flux (and hence the contribution of this flux to overall carbon loss) from the grassland sites would be very large . P15822 line 5: The DOC concentrations are quite high relative to some other studies, e.g. those in blanket mires.

P15822 line 11 and elsewhere: Results can support a hypothesis, but I don't think they can confirm it.

P15826 line 4: Sentence about SO4 suppressing methanogenesis is not clear, and needs to be referenced (e.g. Gauci et al, PNAS)

P15827 line 14-21: This information is partly repeated on P15828 line 24-26.

P15828: This is an example of where too much discussion of secondary aspects of the study detracts from the primary results. Also, I do not think that the statement about negligible risk of NO3 leaching is quite correct – if a large amount of NH4 or labile

DON is leached into river systems, this may mineralise and nitrify to give high NO3 concentrations downstream.

P15831 line 21: I think this is the first reference to DOC export, i.e. fluxes. As noted above, it would be helpful if this could be given greater consideration. More generally, there is a bit too much repetition of results here – greater consideration of the wider implications of these results would be more helpful.

Interactive comment on Biogeosciences Discuss., 10, 15809, 2013.