Author reply to the comments of referee #1
(Interactive comment on Biogeosciences Discuss., 10, 15809, 2013)

We thank referee #1 for the valuable comments on the manuscript.

1. I must say I found the results somewhat predictable, what are the substantial new concepts /results? I'm not so sure this is substantial contribution to scientific progress, to me it seems like this has already been shown before. If this really is a new substantial contribution to scientific progress, the authors need to clarify how in the updated manuscript.

   AC: Most studies dealing with carbon or nitrogen dynamics in peatlands focus either on C or N, but information on both C and N are required to improve the understanding of their closely interlinked cycles. This was already mentioned in the manuscript (page 5, lines 108-112). We are not aware of any study on both C and N for near-natural and rewetted sites in comparison to (intensive) grasslands which is the major land use of peatlands in Germany and neighbouring countries. Furthermore, most research on the impact of drainage and rewetting was conducted in Northern Europe, North America and UK, while in Germany studies on C and N cycling in German peatlands were mainly conducted in fens. As fens are influenced by groundwater and generally more nutrient-rich and less acidic than bogs, these two systems cannot be directly compared. Furthermore, studies on DOC were frequently carried out either in peatlands in the boreal zone or in blanket bogs, which are shallow and sloping and therefore hydrologically and climatically not comparable to raised bogs in the temperate zone. Thus, there is a lack of information on this ecosystem type. This was added to the text (page 5, lines 112-115). Indeed, we found much higher DOC concentrations than, for example, reported for blanket bogs, which clearly shows that previous results cannot be simply transferred to other peatland types or climatic zones.

   Additionally, for the reporting of off-site CO_2 emissions from peatland caused by drainage as well as rewetting more robust field data is required. Especially information on intensively used grasslands and cropland on peatlands are lacking, and the presently available data is not sufficient to calculate emission factors for this type of land use (IPCC, 2013: Annex 2 A3). Our results clearly show that bogs which are intensively used as grasslands bear a huge risk of DOC export due to the increase of DOC concentrations (nearly natural vs. intensive grassland) by over 300%. This is by far higher than previously reported (IPCC, 2013: maximum 118%). The reference to and the comparison with the IPCC guidelines was added to the text (page 5, lines 115-117 and page 12, lines 359-362).

2. One concern I have is the lack of a baseline. They are comparing treatment effects, which is usually done by comparing before and after treatment. But they don't have a "before", they use one site as their baseline based on the assumption that all sites were the same before treatment, but do they have some evidence that the sites were the same before drainage?

   AC: Since drainage of pristine peatlands in Germany has been forbidden for decades, it is impossible to compare the C and N dynamics before and after drainage directly. Furthermore, comparison before and after treatment frequently suffer from non-comparable hydro-meteorological conditions. Therefore, we used the commonly applied "space for time"-substitution, which was also used by Beetz et al. (2013) who measured greenhouse gas emissions at these sites and, indeed, by most studies focussing on DOC dynamics and fluxes in peatlands (e.g. Wallage et al., 2006, Glatzel et al., 2002). Additionally, the study sites IG, EG and RW are close to each other (less than 200 m). NN as reference is approx. 1 km away (see Fig. 1 in the updated manuscript), but Ahrendt (2012) and Schneekloth (1970) describe the part of the "Ahlen-Falkenberger Moor" peat bog complex where our study sites are located as a large homogenous area.
   Additionally, soil properties of the deeper soil horizons at the drained sites are comparable with the reference site.

3. I also wonder about the effect of the plant community. The near natural site is dominated by Sphagnum species while the IG and EG sites are grasslands. How does this change in plant community affect the water balance, the plant exudates, etc. isn't there a risk of confounding when interpreting the results? Now the results are mainly discussed from an abiotic perspective, but what about the interactions with the biota?
   AC: Different plant communities can indeed affect the water balance by different evapotranspiration rates. However, plants well-known for their high evapotranspiration rates (e.g. *Phragmites australis*) are neither present on the drained nor on the wet sites. We did not measure the evapotranspiration directly, and published values on evapotranspiration from typical peatland communities are very rare for the temperate climate zone. We did, however, measure the discharge from the intensive grassland and the re-wetted site. The annual discharge did not differ significantly. As we could not measure discharge from the other two sites, and as the methods and results would probably overburden the manuscript (reviewer #2 already asked us to show less results), we decided to exclude these numbers. Drainage management clearly seems to be the most import driver for water table dynamics. The intensive grassland (IG) with deeper drainage ditches and drainage pipes shows a steeper drawdown of groundwater table during summer than the other sites. Additionally, the drawdown of the groundwater table at IG and EG during winter months is nearly as deep as during summer months and can thus not be

attributed to the vegetation. This information was added to the text (page 10, lines 288-291).

Plant exudates can of course influence microbial processes especially in soils lacking carbon. This is not the case in peatlands. The shift in the amount and quality of litter input from Sphagnum moss to graminoids at the drained grassland sites may have a promoting effect on the degradation of the peat. We only found one paper studying the effects of plant exudates on carbon cycling in peatlands (Basiliko et al., 2012). The authors found that only 4% of total microbial respiration in the control plot was additionally released due to priming effects. They stated that the impact on net C release is likely to be rather small. This was added to the manuscript (page 13, lines 387-390), but any quantification of the impact of the plants would by highly speculative.

4. Specific comments
   Title: "und" should be "and"

   AC: We corrected the mistake and changed the title as suggested.

5. I personally would prefer a reorganization of the manuscript so results and discussion were separated; I believe it would be easier to follow that way.

   AC: We decided to keep the structure as, for example, the discussion of the DOC:DON ratio builds on the previous discussion of the DOC concentrations. As we tackle many different aspects a separation between results and discussion would have made the logic of the presentation fuzzy and repetitive.

6. I found the language mostly good, however, I found some sentences that I had to read over and over again to try and understand what they meant. I believe that the language improved later on the manuscript, I had more problems with this in the beginning. I suggest that the authors do a proper read-through or seek the help of a native English speaker to improve on the language and to clarify what they mean. The use of commas could also be improved, which will facilitate the reading.

   AC: We did a proper read-through as suggested and shortened complicated sentences.

7. I'm not a fan of using acronym's for sites, it makes sense to the people who work at the sites and are used to them, but as a reader you constantly need to go back to the definitions and remind yourself what they stood for. I suggest spelling them out. In the last section (3.6) you help the reader by saying "drained (IG, EG) and wet sited (RW, NN)", I missed that help in the earlier sections. It would make it easier to read.

   AC: We decided to keep the acronyms. From our point of view, the acronyms are quite intuitive (and there are only four of them). To help

the reader unfamiliar with the study site, we added “drained sites” and “wet sites” at several occasions.

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Author reply to the comments of referee #2
(Interactive comment on Biogeosciences Discuss., 10, 15809, 2013)

We are very grateful for the valuable and detailed comments provided by referee #2. They helped us to considerably improve the manuscript.

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.

   AC: We have added a site map and marked the location of the individual sampling points (Fig. 1). The vegetation cover was added to Table 1 and the vegetation data was extended. Further management information was added to manuscript chapter 2.1 to provide a comprehensive background of the water and agricultural management in the study area (page 6, lines 149-174).

   What is meant by 'organic sediment' in Table 2?

   AC: Organic sediments are limnic deposits formed mainly from organic material. We substituted organic sediment with limnic organic deposit.

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?

   AC: The extensive grassland site did not receive any fertilizer or slurry for at least the last 20 years, and still the soil properties of IG and EG are nearly identical. Additionally, comparable DOM properties at IG and EG suggest that fertilizer/slurry application do not have a visible impact on DOM cycling and therefore DOM quality in a depth > 10 cm. This was added to the text (page 14, lines 400-406). Previous studies suggest that drainage and aeration commonly cause a preferential loss of carbon from the peat, and therefore a lowered C:N ratio even without fertilizer addition (e. g. Wells and Williams, 1996). Thus, differences between IG, EG and RW are in our point of view mainly driven by past and present differences in water table position.

Furthermore, the peat properties of IG, EG and RW below the uppermost degraded peat layer at IG and EG as well as the floating peat below the newly established peat layer at RW are nearly identical. This was added to the text (page 9, lines 266-268). Taken this into consideration IG, EG and RW in our point of view were comparable. The main differences between the grassland sites and the re-wetted site is therefore the missing uppermost degraded peat layer, which if present after rewetting may have an initial impact on DOC, NH4 mobilisation (Zak & Gelbrecht, 2007). Therefore, the issue of the topsoil removal is discussed now in the manuscript, and we are aware that our conclusions are valid for re-wetted extraction sites, and that care has to be taken when drawing more general conclusions on re-wetting (page 14, lines 415-420). However, the wetter grassland site (EG) could be interpreted as a partially re-wetted site, and the lower concentrations at this site compared to the intensive grassland seem to allow the interpretation that re-wetting grassland without removing the topsoil would reduce DOC concentrations at least in our study area. This information was added to the manuscript (page 14, lines 401-406).

3. Data from a drained extraction site or a re-wetted grassland site would be helpful here.

   AC: We agree, but the peat extraction at the bog complex is fully terminated, and thus we could not find a drained extraction site. As re-wetting was only conducted for former peat extraction sites or for degraded, but not agriculturally used peatland areas, we could not sample a completely re-wetted grassland. As mentioned above, the closure of the drainage ditches at EG compared to IG indicates the effect of partial re-wetting grassland without establishing groundwater tables at the surface and without removing the topsoil.

4. 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.

AC: As suggested by the reviewer, we added this aspect "near natural vs. drained" during the discussion (page 12, lines 363-369) and in the conclusions (page 21, lines 625-628).

5. 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.
AC: We agree and discarded the sections mentioning the enzymatic latch mechanism after rewetting on DOC production as well as the activation of phenol oxidase due to drainage and discussed both in a more general way as proposed.

6. 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.

AC: DOC concentrations are also highest in the upper peat layer at the extensive grassland (EG) which did not receive any slurry for at least 20 years. Furthermore, highest DOC concentrations in the upper most peat layer were also found by McKnight et al. (1985) in a pristine bog without any management. Thus elevated concentrations in the uppermost peat layer give no evidence that these are caused by slurry application. This is supported by the sampling after slurry application in April where neither DOC nor DON was increased (page 13, lines 394-396; page 17, lines 515-517, Figure 6). After manure application DOC was found to increase in soils with low soil organic carbon (SOC) content. This was caused by increased SOC contents as main source of

DOM due to long time manure application (Liu et al., 2013). The application of slurry at IG adds an additional pool of 216 g m^{-2} a^{-1}. This is rather small compared to the amount carbon stored in the upper 65 cm of the peat (53 kg C m^{-2}). This was added to the manuscript (page13, lines 396-400).

DOC to DON ratios of soil solution at IG (with slurry application) and EG (without slurry application > 20 years) were identical. In addition, the DOC to DON ratios in soil solution at IG and EG fits fairly well with C to N ratio of the degraded peat and is thus a consequence of bulk SOM decomposition. Thus, the application of slurry to soil surface at IG shows no direct impact on DOM cycling and therefore DOC to DON ratios in soil solution. This is included and discussed in the updated manuscript (page: 20, line: 601-605). Differences in DOC to DON ratio between wet (RW and NN) and drained (IG, EG) sites are therefore mainly driven by different decomposition processes as well as different peat C to N ratios as main source of DOM. This was already mentioned in the manuscript.

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

   AC: One key aspect of the current study is to gain simultaneous information on a broad range of dissolved solutes and to find common patterns in their relation to peatland drainage and rewetting within one bog complex. Thus we decided to keep all material included in the study.

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

   AC: We edited the manuscript as suggested, shortened overly long sentence, and hopefully improved the clarity of some complex paragraphs.

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?

AC: The sentence was removed from the abstract.

P15812, line 11: Also photodegradability

AC: At this point, we referred to the cycling of DOC within the soil profile, while photodegradability is more important after DOC release to adjacent streams.

Thus, we decided not to discuss this process at the mentioned line, but include this information to the discussion (page 20, lines 613-615).

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

AC: The citation was added as proposed.

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

AC: We changed the expression as suggested.

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

AC: We added further information on peeper preparation and installation to chapter 2.2 (page 7, lines 181-182, 187, 192-198).

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

AC: A broad range of different wavelengths is used for investigations on DOC/DOM quality and quantity as shown by Grayson and Holden (2012). In case of aromaticity two wavelengths (254 nm, 280 nm) are commonly used in literature. Specific UV absorption (SUVA) at both wavelengths was shown to have a good correlation to aromaticity derived by ^{13}C-NMR (Chin et al., 1994, Weishaar et al., 2003) (page 8, lines 221-223). Our results were discussed in relation to previous German studies using SUVA values around 280 to investigate DOC quality (aromaticity) in relation to intensive land use (Kalbitz et al., 1999), re-wetting (Hoell et al., 2009) and soil type (Don and Schulze, 2008). To gain a better comparability of our results with the mentioned studies, we decided to use SUVA280. In addition SUVA254 was also measured at our samples and the results (rather relative differences than absolute values) were comparable between the four study sites as well as within the soil profile at each study site. Thus the interpretation of the results and therefore conclusions using SUVA280 or SUVA254 are identical.

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

AC: The addition of lime during peatland improvement for agricultural use as well as fertilizer application in the past could additionally explain the increase in pH at this site. This was added to the text (page 11, lines 309-310).

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

AC: We changed the expression as suggested.

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.

AC: Unfortunately, we did not measure the evapotranspiration directly, and published values on evapotranspiration from typical peatland communities are very rare for the temperate climate zone. We did, however, measure the discharge from the intensive grassland and the re-wetted site. The annual discharge did not differ significantly, and thus we assume that a dilution of the concentrations due to reduced evapotranspiration rates at the re-wetted sites is negligible. As we could not measure discharge from the other two sites, and as the methods and results would probably overburden the manuscript, we decided not to include these numbers.

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.

AC: We have no data to compare the discharge at the extensive grassland and the near natural site. Due to the comparable discharge at the intensive grassland and the re-wetted site, we assume that the difference in the concentrations will be directly converted into differences in fluxes. This was added to the text (page 13, lines 363-369).

P15822 line 5: The DOC concentrations are quite high relative to some other studies, e.g. those in blanket mires.

AC: This is correct, and the discussion of the DOC concentrations was extended to compare them to blanket bog peatlands in the UK.

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

AC: We changed the expression as suggested.

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

AC: We clarified the sentence and added the mentioned reference (page 16, lines 476-481). The sentence was not only related to the impact of SO4 on methanogenesis. The main aspect was to mention that methanogenesis is only present in environments where concentrations of other electron acceptors (e.g. NO3, SO4) with higher standard redox potential are lacking or have low concentrations.

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

AC: The repetitive part was removed from the text.

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.

AC: We agree and removed the part of the discussion.

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.

AC: We agree and edited (and shortened) the conclusions as proposed, to give wider implication of results and to reduce repetition (page 21, lines 618-629).

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