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Biogeosciences Discussions

Interactive comment on "Synthesizing greenhouse gas fluxes across nine European peatlands and shrublands – responses to climatic and environmental changes" *by* M. S. Carter et al.

M. S. Carter et al.

mthy@risoe.dtu.dk

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Dear Editor,

We hereby submit the revised manuscript entitled "Synthesizing greenhouse gas fluxes across nine European peatlands and shrublands – responses to climatic and environmental changes". We have carefully considered all comments made by the two referees and Dr. Heinemeyer, responded to each comment (please see below) and made the necessary adjustments in the manuscript. In addition, we have reduced the extent of some parts of the Introduction section. Finally, in the former manuscript the term 'feedback to global warming' was sometimes used in a broad sense. This has been

changed, so 'feedback' only appears when referring to a process that affects itself.

Sincerely, Mette S. Carter

RESPONSES TO COMMENTS FROM REFEREE #3

GENERAL COMMENTS

What is the point of reporting CO2 efflux data from vegetated sites?

Response: In line with the referee's comment below, CO2 efflux measured at vegetated sites can be used to assess the maximum threshold for soil respiratory CO2 emission. This is how we use the ecosystem respiration data from the northernmost site in Sweden (SE-Sto). It supports that soil CO2 emission had climatic optimum at the temperate sites (Fig. 7). Please also see our response to the specific comment below.

I don't see why the presented sites have been selected (apart from the fact that they have been examined within NitroEurope).

Response: The first sentence of the M&M section states that: "The nine experimental peatland and shrubland sites included in the synthesis were all part of the NitroEurope project." We do not try to insinuate that the sites were selected based on any other criteria, except that all sites were open landscapes. On the other hand, we agree that the word "selected" may sound like there was a pool of many sites among which we selected these 9, which was clearly not the case. The nine sites were the ones available, and the study brings the data from these together. Therefore we have change the following sentence "The ecosystems were selected to represent open landscapes in these regions. . ." to "The ecosystems represented open landscapes in these regions. . ."

I do not understand why heathlands and one maquis site have been selected.

Response: Covered by the answer above. These sites were the sites included in the

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NitroEurope project and were not “chosen” among many possible sites.

Treating the non-peatlands separately may yield in two strong and not one incoherent article. Considering the already present extensive introduction and discussion chapters, I recommend using the presented manuscript as a basis for two review-type contributions.

Response: It is not clear why a split into two papers would be so much stronger and the referee is not clear on this point. We strongly feel that cutting the paper into two, may of course make the papers more targeted, but also very slim. In our opinion, the strength of the present paper is that it illustrates both similarities and differences in GHG budgets and fluxes among shrubland systems across climatic gradients, including those with peat/non-peat characteristics.

Please don’t say “peatlands”, but “peat bogs” as fens were not examined.

Response: This question has been discussed intensively among the co-authors. We finally decided to use the term ‘peatland’ in the title, abstract, conclusion etc. as this is not a “hardcore” wetland paper. After the sites are introduced in the M&M section we also use the more specific terms ‘ombrotrophic bog’ or simply ‘bog’.

SPECIFIC COMMENTS

P. 3696, L.10: Isn’t the potential C loss from drainage of N fertilization the bigger problem? Not all ecosystems you are presenting store a lot of C (heathlands). Maybe it would be better to also refer to the global significance of C loss from peatlands and add few sentences on heathlands.

Response: In the beginning of the Introduction section we would like to focus on the situation in undisturbed ecosystems. The effect of human intervention for the C loss from peatlands is touched upon later in this section (drainage and N deposition, page 3698-3699). Our intention was briefly to list some examples of ecosystem services provided by natural ecosystems. We are aware that heathlands do not sequester sig-

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nificant amounts of C. Therefore we have added the word 'may'. The sentence now reads: "These ecosystem types make up approximately 7 % of the European land area... and may contribute valued ecosystem services, such as biodiversity, habitat provision, recreation, water purification and carbon (C) sequestration (Wessel et al., 2004; Kimmel and Mander, 2010)."

P.3696, L.25-28: The sentence "Methane. . .Le Mer and Roger, 2001" is not necessary and can be deleted.

Response: This sentence has been modified.

Introduction: I am aware of the difficulties in calculating a complete C budget (including GPP) and sometimes, it is the best thing just to report CO2 efflux. However, in that case a paragraph should be devoted to this problem arguing why it is sensible to limit CO2 data to efflux and what the benefits of this procedure are.

Response: We agree and have changed the wording of page 3698 – line 1-2 from: "Decomposition of soil organic matter and plant root respiration together make up soil respiratory CO2 emission, which in peatlands typically correlates positively with both temperature and water table depth (Smith et al., 2003; Danevčič et al., 2010)." The sentence now reads: "Soil respiratory CO2 emission originates from decomposition of soil organic matter and from plant root respiration. Together with gross ecosystem photosynthesis, soil respiratory CO2 emission is the dominant flux of carbon between terrestrial ecosystems and the atmosphere (Sclesinger and Andrews, 2000), and changes in soil respiration in response to climate treatments is an important indicator for ecosystems' feedback to climate change. Soil CO2 effluxes from peatlands typically correlate positively with both temperature and water table depth (Smith et al., 2003; Danevčič et al., 2010)."

P. 3702, L. 1 to 7: If CH4 and N2O have been measured at only one collar, it is impossible to gain any idea of spatial variability. This would be a grave shortcoming and considerably decreases the significance of the presented results. On the other hand,

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looking at Table 3, I get the impression that GHG fluxes have been measured from several collars at each site. Please clarify this.

Response: The referee refers to the following sentence: “Overall, well before simultaneous measurement of CH4 and N2O fluxes started, a permanent soil collar was installed in each plot on which a chamber was placed during measurements.” Unfortunately, it did not clearly appear that the treatment plots were replicated. Therefore we have changed “in each plot” to “in each replicated treatment plot ($n \geq 3$)”.

Also, the measurement frequency is on the low end of what can be considered acceptable, especially considering the simplistic linear upscaling procedure.

Response: This is true and is true for the majority of studies in this area where continuous measurements are used very rarely. Therefore our results match similar results from other studies and other ecosystems, and comparisons are therefore possible. This said, it is clear that more elaborate upscaling procedures could be useful, e.g. establishing response functions related to the main drivers and estimate fluxes based on these. However, we do not think our dataset is extensive enough to establish such functions, and have therefore chosen to use the simple extrapolation. Since we use the same procedure for all sites, this provides a reasonable foundation for comparison, and we provide the fluxes from the sites as the range across the sites rather than as a definite and “true” number. In addition, the majority of results are concerned with treatment effects, and again, since we use the same procedure for the controls and the treatments, we are convinced that the relative effect of the treatments would not change significantly even with a more elaborate calculation procedure. We have added a sentence in the M&M to highlight the limitations associated with the simple upscaling approach (page 3703, line 1-2 changed to say): “Annual cumulative fluxes were obtained by linear interpolation between measurement days. This is a common, but crude, upscaling procedure since measurements carried out at one point in time, and over a relatively short time span, will represent the whole period between two neighboring measurements, despite potentially large variation in the controlling climatic factors

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during this period. A more precise method would be to model the fluxes based on more frequent measurements of key drivers such as temperature and soil moisture (e.g. Selsted et al., 2012). In the present study, we did not have sufficient measurements to build such a model for each site. At the Swedish peatlands SE-Fäj. . .”

What does “well before” (L. 5) mean? At least how many days or weeks?

Response: This sentence now reads: “Overall, at least five weeks before simultaneous measurement of CH4 and N2O fluxes started...”

P. 3704, L. 19-20: How was C stock estimated? From the supplementary material, I get the impression that is has been measured, not estimated.

Response: Carbon stock in the 0-10 cm soil layer was calculated based on soil bulk density and total C content. We have changed ‘estimated’ to ‘calculated’ in this sentence, so it now reads: “Furthermore, bulk density, total C and N contents, and microbial biomass C were measured as described by Beier et al. (2009), and C stock in the 0-10 cm soil layer was calculated.”

Is it sufficient to report the C stock in the first 20 cm in peatlands?

Response: As it appears above, we calculated the C stock in the upper 10 cm soil layer at all sites. These numbers were only presented in relation to the CH4 fluxes at the shrubland sites. We decided to limit the soil horizon to 0-10 cm as this is the depth where soil C is located at shrubland sites. Furthermore, we wanted to do the calculations in the same way for the two ecosystem types.

P. 3707, L. 14: Why don’t you say “. . .at the site with the highest. . .”

Response: Done

Chapter 3.4: You are right, CO2 fluxes are probably even lower. But this means that the real flux is not known. How is it possible to analyse that kind of data? In my opinion, the only use for that kind of data (from vegetated sites) is assessing minimum fluxes

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that can be used to support other arguments.

Response: Unfortunately, soil respiratory CO2 emission data was not available for the two Swedish sites. Instead we decided to include their data on ecosystem respiratory CO2 emission. We clearly mention this deviation every time the data is presented in the Results section (Chapter 3.4, Table 3, Fig. 7). As stated under general comments, the ecosystem respiration data from the northernmost site, SE-Sto, is used to support that soil CO2 emission had climatic optimum at the temperate sites. Thus, in line with the referee this data is used to assess the maximum threshold for soil CO2 emission. We have not made any corrections to account for this comment.

P. 3710, L. 5-12: The fact that, despite not having considered other major controls of CH4 release, you achieved such a nice temperature dependency is astonishing. Still, one wonders what the value of this finding is considering your limited dataset with a focus on ombrotrophic bogs. Fitting your data points to other, published data, with known plant species or water table, would help in coping with this caveat.

Response: It may be astonishing, but nevertheless is what the data shows. We only provide the results as they are. As for the value of the findings, we do not really understand the reservation. We bring together data from a range of ecosystems and a range of treatments, and in our view the value lies in this "gathering" of data showing similarities and differences across sites and treatments. We do not know what other data the referee is referring to? We have used the data we had available. It may be that other factors also contribute to the controls of the fluxes – most likely so, and we do not try to claim that the factors we have looked at are exclusive, but some of them seems to exert part of the control on the fluxes - others do not. We have not made any corrections to account for this comment.

And: You did not do a space for time substitution. Too many other environmental factors differ.

Response: This is correct – we have rewritten this part and now refer to 'natural gradi-

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ent analysis' instead of 'space for time substitution'.

P. 3711, L 12-27: Maybe decreased CH4 oxidation as a consequence of presence of NH4 plays a role here?

Response: The fact is that the vast increase in CH4 efflux occurred when N was applied in oxidized form (NO3-), not in reduced form (NH4+). Thus, the referee's suggestion does not really fit in. We have not made any corrections to account for this comment.

P. 3711, L.26-29: This shows that the non-consideration of "specific properties" makes new insights very difficult. Why weren't these properties examined?

Response: Same comment as given for page 3710, line 5-12

P. 3712, L.16-17: Maybe looking at % water filled pore space might help. Generally, the high measurement interval hurts N2O upscaling most.

Response: The reviewer is right. However, we don't have data for soil porosity at the nine sites and therefore we are not able to calculate water filled pore space (WFPS). For mineral soils, it is possible to calculate WFPS using soil bulk density and literature values for particle bulk density. However, the soils in this study consist partly or fully of organic matter, which makes such a calculation impossible. In the manuscript we avoid to put focus on small differences across sites in the volumetric soil moisture and mainly use the soil moisture values to discuss treatment responses at the site level.

Table 2: Did you account for the different soil temperature probe depth at the sites?

Response: We don't really understand what the referee means? We report the temperature data, which are available. And yes, these data were obtained using soil temperature probes that were installed at different depths as it is described in the M&M section.

For GHG analyses, %water filled pore space (WFPS) is a better parameter compared to %vol. Why wasn't WFPS used?

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Response: Same comment as given for page 3712, line 16-17

All Figures: Nice Figures! Figures 2 and 3: The SE of what is shown by the error bars?

Response: In figure 2, 3, 4, 6 and 7, error bars indicate SE of annual flux rates based on measurements in replicated plots. In some cases, SE is relatively small and therefore the error bar is hidden behind the site symbol. We don't understand why the referee only raises this question in connection with Figure 2 and 3? We have not made any corrections to account for this comment.

RESPONSES TO COMMENTS FROM REFEREE #4

GENERAL COMMENTS

In the results section, the authors too often switch between the results from the peatlands and the shrublands, or they combine the results of both. This back and forth, and the many abbreviations for the study sites make it very difficult to read and apprehend the manuscript. To make it easier for the reader, I suggest dividing each chapter in two parts, one for the peatlands and one for the shrublands.

Response: We are sorry that the referee finds the Result section difficult to read. Our idea was first to present results from the natural gradient analysis, including control plots only, and subsequently to present results from the experimental manipulations where they fit in. Unfortunately, this approach causes a swapping forth and back between the two ecosystem types. In itself, the natural gradient analysis makes it difficult to divide the result chapters into separate parts for shrubland and peatland as it includes both ecosystem types. Furthermore, we prioritize to present all related results in continuation (for instance effects of N availability on N2O fluxes), even though this means that results from shrublands and peatlands are presented together. To comply with the referee we have made the following changes to the Result section: To guide the reader in the beginning (CH4 chapter), we have clearly differentiated between results from the natural gradient analyses and the experimental manipulations: "For the peat-

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land sites, the natural gradient analysis showed a clear relationship between annual CH4 emission and mean annual air temperature (Fig. 2). ... Concerning experimental manipulations, the effect of increased N input was tested at three peatland sites..." Furthermore, we have joined results for each ecosystem type where possible. Thus, in the CH4 chapter, results on N addition are moved up before results on drainage, which produce a longer continuous text on peatlands. Finally, we have made sure that the ecosystem type is always mentioned before the site abbreviation, for instance 'the peatland UK-Whi'.

The material and methods section leaves many questions unanswered. I am missing a uniform experimental setup and have the impression that the manuscript is a patch up of single measurements without any consultations between the project partners. I find it extremely difficult to compare trace gas fluxes from different sites, which were obviously measured with static chambers of different sizes, with varying gas sampling techniques (syringes, evacuated vials, double needle approach), sampling frequencies (twice a month – monthly), sampling periods (5 months – annual measurements), sampling years, different analytical detectors (i.e. for N2O: ECD, photoacoustic) and a varying number of replicates. In other projects with a similar multi-site approach, a uniform sampling device is used or different sampling devices were at least compared at one site.

Response: To play the devil's advocate, it does actually not seem like many questions were left unanswered: the reviewer seems to have got a quite good grip on the complexity. The referee states that in other such multi-site projects a uniform sampling protocol is used. This would of course have been optimal and we would have preferred this, but the sites included in this project and this study all existed before the project was started and therefore had established ongoing methodologies and procedures. This complexity clearly provides a big challenge for the comparison, but it does not necessarily rule out a synthesis and comparison. We feel generally comfortable with any comparison "within site" where all procedures were the same across treatments

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and the relative change by treatment is unaffected by procedure. The “between site” comparisons can be slightly more difficult, especially if the goal is to provide robust and accurate numbers. To clarify why the experimental setup differed between sites, we have added the following sentence in chapter 2.1: ”The experimental sites existed before the NitroEurope project started and therefore already had ongoing methodologies and procedures.”

Specific response for ’Static chambers of different sizes’: Within the NitroEurope project, a chamber intercomparison campaign was carried out with focus on CH4 effluxes (described in part by Christiansen et al., 2011, Plant and Soil, 343: 171-185). One of the conclusions of the campaign was that larger chambers (area, height, volume) gave more precise estimates of the actual CH4 efflux, partly because larger chambers have less edge effect than smaller chambers (pers. comm. Mari Pihlatie). More specifically, the results showed that chambers with height $\leq$ 22 cm, area $\leq$ 1000 cm2 and volume $\leq$ 20 L tended to underestimate the CH4 efflux rates. The static chambers used for CH4 and N2O measurements in the present synthesis were constructed before the project started, and the chamber dimensions where more or less set by the vegetation density and height at the individual sites. The chambers used at the peatland sites generally complied with the recommended minimum sizes proposed by Pihlatie et al. (i.e. height 15-44 cm, area 780-1600 cm2, volume 20-59 L). Thus, we are rather confident that CH4 efflux rates are comparable across the peatland sites. However, at the shrubland sites the chamber dimensions were more variable (e.g. volume 0.8-179 L). Here net CH4 uptake primarily took place, thus as such the findings by Pihlatie et al. are not relevant. In the GRACEnet Project, a comprehensive protocol for chamber-based trace gas fluxes measurements was made (Parkin and Venterea, 2010, in Sampling protocols, Follett (ed.), p. 3-1 to 3-39. Available at www.ars.usda.gov/research/GRACEnet). Here it is stated that: “Chambers should be large enough to cover at least 182 cm2 of the soil surface, and have a target height of 15 cm (height can be decreased to increase sensitivity or increased to accommodate plants)”. Chambers used at the shrublands UK-Clo and ES-Gar were below this area

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recommendation (154 and 127 cm2, respectively). Thus, we cannot exclude that differences in static chamber dimensions may have biased the comparison of flux rates (CH4 or N2O) across the shrubland sites. The first time results are compared across sites in the Discussion section, we have made the following addition: “Although our natural gradient analyses could be biased by differences in measurement years and methods, the results are in line with Christensen et al. (2003) who compared ...”

Specific response for ’Sampling devices’: We think this is a partly “artificial” argument as we are not aware of any studies showing that vials versus syringes make any big difference. Specific response for ’Sampling frequency’: This has mainly consequences for the upscaling process and could lead to differences among sites because of the different frequency. Only three sites had CO2 emission measurements more frequently than monthly, and for those we have tried to estimate the difference in extrapolated annual fluxes based on monthly and twice-monthly measurements. The annual CO2 efflux was only marginally affected whether a monthly or twice-monthly measurement frequency was used (deviation 0-6 % for 3 treatments at 3 sites). At the Dutch shrubland Oldebroek, twice-monthly data for N2O and CH4 were available. The deviation between annual N2O fluxes based on monthly and twice-monthly data was about 15 mg N m-2 yr-1, but the flux for each treatment kept the same “direction” (emission or uptake) independently of measurement frequency. The annual CH4 uptake in the control and drought treatments increased by ca. 30 mg C m-2 when changing to a monthly measurement frequency, primarily because one CH4 emission event was left out of the monthly dataset. However, these changes would not affect the main conclusions of the paper. We prefer to use all available data to estimate the “true” annual flux rates as good as possible.

Specific response for ’Sampling periods’: It is described how it was done. It is not uncommon in science to do assumptions, which can then be assessed and criticized. We think the assumption we applied is reasonable. Please also see our response to the specific comment below.

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Specific response for 'Sampling years': The biggest challenge occurs for the across site comparisons. We have added the following in chapter 4.1: "Although our natural gradient analyses could be biased by differences in measurement years and methods, the results are in line with Christensen et al. (2003) who compared ..."

Specific response for 'Analytical detectors': There may be small differences among detectors, but to our mind and experience this is not adding any major uncertainty to the overall results. In the M&M section, we already refer to two studies, which showed that gas flux rates measured using a photoacoustic gas analyzer were statistically identical to flux rates based gas sampling and analysis by gas chromatography.

Specific response for 'Number of replicates': This mainly adds to the uncertainty of the numbers which is indicated on all graphs. Furthermore, the statistical analysis that we used is suitable for datasets where the number of replicates vary between sites. This is already explained in the M&M section: "The GHG flux datasets were unbalanced as the number of replicates was unequal between sites, however the MIXED procedure is used in the same way whether data are balanced or unbalanced (Littell et al., 2002). More specifically, equal weight is given to each site regardless of the number of replicates when determining treatment effects."

SPECIFIC COMMENTS

P. 3702, l. 2: Since the temporal variability of trace gas fluxes may be extremely high, the interpolation of flux rates between monthly measurements does not allow a quantitative determination of trace gas fluxes. I would exclude the "uncertain" study sites with only monthly measurement frequency.

Response: We have commented this for Referee #3. Increasing the frequency of sampling clearly increases the certainty of the upscaled numbers, and more frequent measurements would definitely be preferable. However, manual measurements will by definition have this "pointy" character and will be associated with a significant uncertainty, which exists also for the sites with a more frequent measurement interval.

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Monthly or twice-monthly measurements are quite common in the literature, and our measurements are therefore quite comparable – despite the uncertainty. We prefer to live with the uncertainty and explain clearly how the numbers were derived, allowing any reader to judge the certainty and use the data with the caution they find necessary.

P. 3702, l. 10: For the Spanish study site, the enclosure period was only 15 minutes. I don't think that 15 minutes are enough for the reliable measurement of low CH4 fluxes.

Response: Chamber enclosure duration should be adjusted according to the height of the chamber and to knowledge on flux rate levels for the ecosystem type. We have calculated the Time:Height ratio for the chambers used at all nine sites. The ratio for the Spanish shrubland site was 2.4 min cm-1. This is not very different from 2.8 min cm-1, which is the ratio for the Danish shrubland Brandbjerg during summer season (height 54 cm, enclosure duration 150 min). We have not made any corrections to account for this comment.

P. 3703, l. 1-8: The measuring period at the Swedish sites was 7 and 4 months. For the remaining cold months, the authors used ratios (contribution of winter period to total annual flux) from CO2 measurements to estimate the winter fluxes. The same ratios were used for the CH4 and N2O fluxes. It is well known that the Q10 values for the N2O release from soils may be several times higher than the Q10 value for CO2 emissions (see Smith, 1997; Global Change Biology 3, 327-338). I therefore have some doubts about the application of the same ratios for N2O. If the data is too arguable, I recommend excluding it from calculations.

Response: First of all, we would like to emphasize that N2O fluxes were not measured at the Swedish site SE-Sto, thus the issue about upscaling of N2O fluxes from 7 months to annual values is only relevant for the Swedish site SE-Fäj. We have looked at the paper that the referee refers to (Smith, 1997). Here it is stated that: "In natural terrestrial ecosystems the rates of the two processes of nitrification and denitrification are usually governed by the availability of mineral N (NH4+ and NO3-), librated by organic matter

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decomposition, in the course of N mineralization." In Table 1 of Smith (1997), some Q10 values for N2O emissions from soils are listed, but none of them are for natural ecosystems. The most undisturbed ecosystems in the table are unfertilized grassland and short grass prairie. Here the Q10 value ranges 2.8-3.1 and 1.5-5, respectively. The only Q10 value for soil respiratory CO2 emission, which is mentioned in the paper, is 2.5 for deciduous forest. Thus, based on Smith (1997) we are not convinced that the Q10 values for N2O and soil CO2 fluxes are so much different. In addition, we have done a literature search using Web of Science and the following keywords:

Title = (peatlan* OR wetlan* OR bog OR fen OR mire) AND (N2O OR nitrous oxide OR CH4 OR methane OR CO2 OR carbon dioxide OR soil resp* OR GHG OR trace gas), combined with Topic = Q10

This search only resulted in a single paper (Kim and Verma, 1992. Soil surface CO2 flux in a Minnesota peatland. Biogeochemistry, 18: 37-51). Here the reported Q10 value for soil respiratory CO2 emission in a peatland was 3.7. Other search attempts confirmed that no papers so far have be dedicated to compare Q10 of N2O and CO2 fluxes, irrespectively of ecosystem type. Our conclusion is that studies focusing on this topic is currently lacking. We have explained how our calculations were done, and we believe the assumptions are resonable. We have extended the description of the assumptions in chapter 2.3: "These ratios were also used to estimate annual CH4 and N2O fluxes, assuming that CO2 fluxes represented the general level of microbial activity; more specifically, that Q10 values for CO2, CH4 and N2O fluxes were not substantially different. Reported Q10 values for CH4 and N2O fluxes in natural ecosystems are sparse, however, Smith (1997) compiled Q10 values for N2O emissions ranging 2.8-3.1 and 1.5-5 in unfertilized grassland and short grass prairie, respectively. This is within the range of Q10 values often reported in literature for soil respiratory CO2 emissions (e.g. Kim and Verma, 1992)."

P. 3704, l. 14-15: For the extraction of NH4+ and NO3- three different extracting agents were used. These agents extract different amounts of NH4+ and thus are unsuitable

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for a pooled calculation of regressions or correlations with trace gas fluxes.

Response: This is correct. The main difference in extraction efficiency occurs for NH4+. Data for soil NH4+ is presented in Table A1 in the Supplement. We have added the following footnote to Table A1: "The extraction agent varied with site, thus soil NH4+ data should only be used to compare treatments at the site level"

P. 3704, l. 17-18: The same problem. Different extraction agents used for the determination of soil pH.

Response: It is correct that suspending soil in salt solutions rather than in water result in significantly lower pH values. During the data analysis we had this issue in mind. In the manuscript, soil pH data is only used when discussing the CH4 results from the Scottish bog UK-Whi. The pH data are presented in Table A1 and Table A2 in the Supplement. Here it is clearly stated that other solvents than water was used for the pH measurements at three specific sites: "Soil pH was measured in 1 M KCl at EE-Män and in 0.01 M CaCl2 at DK-Mol and DK-Bra." After this we have now added: "These extraction agents result in lower pH values compared to soil suspended in water."

P. 3708, l. 4: Now the reader receives information that there were also vegetated plots. Is that correct? I understood that the vegetation was removed from soil collars. This information is missing in the M & M section!

Response: It is correct that CO2 fluxes at the Swedish sites were assessed on vegetated plots (also see comments to Referee #3). This information has now been added to the M&M section.

Reading lines 2-5, I must assume that dark chambers were used; insert this information in the M & M section!

Response: Done

P. 3711, l. 11-25: Here the authors discuss why the addition of nitrate alone stimulated CH4 emission at one site, whereas CH4 fluxes did not respond on the combination

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of nitrate + ammonium. The difference between the two N applications was that nitrate was added as sodium salt at the English study site, and as ammonium nitrate at the Swedish sites. Is anything known about the effect of Na+ on the formation or consumption of CH4 in soils?

Response: This is a very good question. For comparison, if NO3- is added together with K+, potassium could stimulate CH4 methanogenesis, but a similar mechanism is not known for Na+ (pers. comm. Paul Bodelier). But the sodium ion could be important in another way. In fact it is probably the NaNO3 salt, and not only the NO3- ion, which causes soil pH to rise at the Scottish peatland site (described in more details by Evans et al., 2008). In chapter 4.1 of the Discussion, we therefore now refer to the added salts instead of the N-ions only.

P. 3712, l. 16-19: The authors state that volumetric soil moisture is not suitable for the prediction of gas diffusivity in soils with different porosity. I agree, and I would like to encourage the authors to calculate the water-filled pore space (WFPS) using the soil density data that the authors already have. The WFPS can easily be calculated using estimated values for particle density.

Response: Unfortunately, this approach is not possible for the peat soils. Please see our comment to Referee #3 concerning WFPS.

P. 3713, l. 17-21: Here, the authors report low fluxes and the resulting long enclosure periods (up to 60 min) at the site ES-Gar. This is in contradiction to page 3702 l. 9-11 where an enclosure period of only 15 minutes is reported for that site. Clarify!

Response: It is correct that 15 min enclosure period was used at ES-Gar. The 60 min refers to the other site (NL-Old), which is discussion in this section. We have clarified this point and the sentence now reads: “This implied longer enclosure periods, i.e. 15 or 60 minutes compared to 1-5 minutes when measured directly in the field using an infrared gas analyzer.”

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Table 3: I recommend inserting the statistical comparison of the treatments within every site with different letters.

Response: Thanks for the suggestion. However, we have not tested treatment effects at each site individually, but only across all relevant sites.

Table 3: Now the reader is informed that the fluxes were not measured in the same experimental year. This information in missing in the M & M section, even though it is essential. We know that annual N2O emission from one site may vary by factor 4 or more (at least for agriculturally used soils). Inter-annual variability as a reason for differences between trace gas emissions from the study sites should be discussed briefly.

Response: In chapter 2.3 of the M&M section we have now added: “The year of measurement deviated between sites, but was generally within the period 2006-2009.” In chapter 4.1 of the Discussion section we have added: “Although our natural gradient analyses could be biased by differences in measurement years and methods, the results are in line with Christensen et al. (2003) who compared ...”

RESPONSES TO COMMENTS FROM DR. HEINEMEYER

I still feel that your statement on page 3713 “Longer enclosure periods may lead to non-linear development of headspace CO2 concentrations over time, and subsequently to underestimation of flux rates if calculations are based on linear regression” needs a reference.

Response: We now refer to Heinemeyer and McNamara (2011)

Still, I feel that you did not clarify the potential effect of collar insertion on measured fluxes in the discussion. Specifically, the final section “For this reason, generalized conclusions based on the mean responsiveness presented here should be drawn with caution. Especially,we lack information on...” might require this addition; it is maybe not only due to a lack of information on NPP but rather the cutting of NPP supply to

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the rhizosphere? I only thought your discussion should consider this potential effect. Currently, I cannot see where you discussed this - I think that for any synthesis such information is an important piece of information and should be discussed alongside observed differences or trends etc.

Response: We acknowledge your findings in Heinemeyer et al. (2011), which show that insertion of collars 10 cm or more into the soil may reduce soil CO2 effluxes due to cutting of fine roots, and furthermore that this effect may be long-lasting (>21 months). However, once more we would like to emphasize that this is a synthesis paper and not a methodological paper. Collar insertion were used at all sites, thus measurements should be broadly comparable across sites and directly comparable within site. We have not made any corrections to account for this comment.

Interactive comment on Biogeosciences Discuss., 9, 3693, 2012.