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Biogeosciences Discussions

Interactive comment on "The greenhouse gas exchange responses of methane and nitrous oxide to forest change in Europe" *by* P. Gundersen et al.

P. Gundersen et al.

pgu@life.ku.dk

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Response to reviews

First off all we want to thank the referees for the valuable and supportive comments to our paper. We have improved the manuscript along the lines agreed upon by the reviewers, except for one issue that can be debated. Below we address each of the points raised what we have improved, and if not, why. We have shortened the text somewhat and skipped one figure.

Anonymous Referee #1 (Received and published: 2 July 2012) Review of the manuscript "The greenhouse gas exchange responses of methane and nitrous oxide to forest change in Europe", submitted to Biogeosciences Discussions This paper

compiles a comprehensive set of data on GHG flux measurements in different European forests emphasizing the change in GHG budgets upon simulated anthropogenic impacts and climatic changes. Although the strength and direction of responses in some cases were unexpected, the overall findings generally support our conceptual understanding of dominant regulators on GHG exchange in forest ecosystems, and it is interesting to learn that the concepts apply to a variety in ambient climatic and soil conditions as well as forest types. Thus, publication is recommended. Generally, the paper reads well with a good organization in the data presentation, discussion and outlines.

1. Concerning the experimental description and in particular the GHG measurement techniques, only little details are included, although references to primary articles are given. Nevertheless, I do think it would be helpful with a short description of uncertainties in the GHG flux observations. Clearly, the authors are aware the chamber based GHG flux observations have been / are subject to critical considerations. In Table 2 the number of concentrations measurements are indicated and flux calc. method is indicated, however no further comments on this information is given in the text. Does this imply that some dataset are more reliable than others? Please, specify in text.

Authors reply: The reviewer has a good point and we have added a new paragraph in section 2.2 Gas exchange measurements. We thought the placement here is more appropriate as the paper is more focused towards synthesizing data rather than discussing measurement uncertainty per se. Based on the information we have gathered of the GHG flux methodology in Table 2 it is difficult to assess the level of uncertainty of GHG fluxes, both qualitative and quantitative. One source of uncertainty stems from the actual chamber measurement and as we have written we assume that this uncertainty is comparable between chamber methods because of the relative similar protocols used. The uncertainty from the chamber measurement is most likely much smaller than the uncertainty related to spatiotemporal variability within the respective sites. Thus area sampled (given in Table 2) give some indication how representative

the fluxes may be. Another aspect we have included is that although chamber measurements may bias the absolute flux they are well suited to test treatment effects. We hope that the reviewer find our disposition towards this relevant topic adequate in the manuscript, where the following was added “Greenhouse gas fluxes measured with static chambers are susceptible to multiple sources of error from chamber design, chamber handling, sampling protocol and subsequent flux calculation (e.g. Rochette & Eriksen-Hamel, 2008). Improper use of fans to mix chamber headspace and linear flux calculation leads to underestimation of the absolute flux out of the soil (Christiansen et al. 2011). This underestimation of the GHG flux by static chambers is critical in terms of establishing budgets, however, less so in relation to testing treatment effects as is the case in this study (Rochette & Eriksen-Hamel, 2008). The agreement on a common minimum protocol was meant to reduce the uncertainties and improve the comparability of the GHG fluxes measured across sites. Hence we expect that uncertainties related to the site specific measurement protocol was minimal compared to the spatiotemporal variability encountered at the individual sites. “

2. In addition to the GHG methodology, protocols for pH observations should be included (e.g. water or CaCl2), as different protocols per se will give different results, which might have influence on the current data-analysis.

Authors reply: We have moved soil pH to Table 1 as suggested by the reviewer below and indicated in a footnote that pH was measured in CaCl2, although adjusted from measurements in water for two sites.

3. For the synthesis of data and discussion of GHG responses it might be helpful to convert the N2O and CH4 responses into the common scale of Global Warming Potentials, expressed as CO2 equivalents. This information will facilitate the intersite comparison both in terms of strength and direction. The information could be included in Table 3.

Authors reply: We have chosen not to do this conversion, since the objective of our

paper is not to make GHG budgets, but to focus on the reasons for the response of fluxes to change. Going to CO2 equivalents would be more relevant if we had also CO2 exchange data. The way we have focused the paper, CO2 equivalents are only useful in the last section 4.4, but not needed. In case the editor find it useful we can add an appendix with the fluxes and responses expressed as CO2 equivalents, including also the % contribution by each gas. We would then have to make some minor additions in section 4.4 to accommodate this additional information.

4. The authors refer to internal drivers as being important for the GHG flux control, and in particular addresses N availability with focus on NO3-. However, no data on NO3- concentrations in the different sites are presented, only the indirect evidence in form of leaching observation from previous studies. In order to sustain the discussion on N2O increase (P6145) and CH4 ox decrease (P6147), data on NO3- / NH4 should be included.

Authors reply: The reviewer is right about the pivotal role of NO3- in relation to N2O fluxes and N availability in general on CH4. However, several of the detailed studies that we synthesize in this manuscript have already addressed the subject of NO3- and N2O dynamics. Instead of treating the dynamic behavior of N2O or CH4 fluxes in this study we rather consider the state of the system and how changes to this state alters N2O or CH4 fluxes and we thus use indicators or proxies of NO3- concentrations or N availability such as C/N ratio and NO3-leaching that integrates the effects over much longer time periods and larger spatial scales than do the dynamics of NO3- concentrations. We believe that our approach is useful in pointing to indicators that are more usually known than mineral N concentrations and their dynamics. Using NO3- /NH4 concentrations would require stringent similar methodological approaches between the underlying case studies which unfortunately are not available for all the sites.

5. In addition to this, a number of specific comments need to be addressed by the authors. Title: Consider to rephrase. Suggestion “The response of methane and nitrous

oxide greenhouse gas exchange to forest change in Europe"

Authors reply: Title changed to: The response of methane and nitrous oxide fluxes to forest change in Europe

6. Introduction P6133, L15: I suggest leaving out Fig. 1. Most information in this figure is already in the text. Authors reply: Suggestion taken

7. Methods P6134, L22: Be consistent when listing the site names, and not only use the abbreviations. Authors reply: We checked consistency throughout the paper. We used the traditional way: the first time a site was mentioned the entire site name was spelled out followed by the two letter abbreviation in brackets. The abbreviation was then used consistently throughout the manuscript.

8. P6135, L2: The pH values should be in Table 1 (or as is in Table 2), and not repeated in the text. Along this, there is no need to repeat information from Table 1 e.g. on soil type. Authors reply: We have removed this info from the text and added soil pH to Table 1 so that it comes with the soil type information.

9. P6137, L10: Explain what is meant by stabilized ash. Authors reply: We have given more information on the ash used: 'hardened and crushed wood ash with a pH(H2O) of 13'

10. P6137, L15: As you do not consider results from the high ash treatment, there's no need to describe this in the text (e.g. line 10). Authors reply: We have rephrased and excluded the high ash treatment

11. Results P6139, L10: Here, and in a couple of other places (e.g. L27), you refer to the direction of the correlation with reference to Table 4. But the direction is not apparent from R2; correlation coefficients (r) need to be included. Authors reply: Correlation coefficients have been added to Table 4.

12. P6140, L10: Consider to remake Fig. 4. It's confusing that data in Fig. 4 is on a positive scale when the majority of the fluxes are still negative. Perhaps this could

be illustrated by using a sort of "stacked bar" indicating the control flux and treated flux with different shading.

Authors reply: We have tried different approaches, but found that this was the best way to focus on 'the response to change'. A positive number (e.g. the response factor: treatment – control) indicate that the treatment result in more GHG to the atmosphere. For CH4 a lower uptake rate due to treatment, thus end up as a positive number (even though the experienced GHG people know these fluxes are usually negative. The main reason to keep it this way is that if you compare the N2O and the CH4 figures you will immediately see that almost all the manipulation treatments had the same direction of response (mostly more GHG in the atmosphere). We have added a note to the CH4 figure caption to help clarify: "A positive treatment response indicates that more CH4 is left in the atmosphere (i.e. a lower CH4 uptake occurred in the treatments compared to the controls)."

13. P6140, L20: According to Fig. 4 the wood ash addition was significant; correct in text. Authors reply: Done

14. Discussion P6141, L23 - P P6142, L15: I suggest leaving out Fig. 5. It does not add information to the text; the length of the text-section could also be reduced.

Authors reply: We disagree on leaving out Fig. 5 (now Fig. 4) – the C/N figure. We would like to show the relationship with the best predictor and discuss the interaction with the other regulating soil characteristics. A special reason is also that the C/N-figure displayed in the paper by Pilegaard et al. (2006) has the wrong data, although the right data are presented in a table. We thus compare here the rather strong relationship found by these authors with the results from a more diverse selection of sites and treatments. The relationship still holds, although the exceptions need to be explained by other regulating factors. The text was shortened 4 lines as suggested.

15. P6143, L13: It should be emphasized that low pH depresses overall denitrification, and thus not necessarily leads to increased N2O emission. (This is in fact mentioned

later, P6145, L14-17).

Authors reply: We have mentioned that the N2O/N2 ratio increases with decreasing pH. This is in line with the review comment above. Our analysis indicated that N2O emissions increased (treatment response and thus increasing the net climate forcing) but we could not assess the overall pH effect on total N gas emissions (NO, N2O and N2) originating from denitrification based on the available data.

16. P6144, L16: Add a reference to the statement about relationship between soil moisture and pH. Authors reply: We added a reference to support this statement.

17. Table 1: MAT for Gardsjon is missing Table 2: Do not refer to unpublished material (Moldan et al) Authors reply: MAT was added and reference was made to a previous campaign using the same chambers and methods, Klemedtsson et al. 1997.

Anonymous Referee #2 Received and published: 11 July 2012 Review of the manuscript by Gundersen et al. submitted to Biogeosciences Discussions. The paper presents results of a very well structured research from a comprehensive range of forest sites into the impact on GHG fluxes of climatic (temperature and precipitations), soil characteristics (type, pH, moisture), N additions (atmospheric and soil) and forest management (harvest and wood ash addition). The results, discussion and conclusions are well structured to capture interactions between these variables and GHG fluxes and therefore I recommend it for publication.

18. - In methodology section the authors indicate that flux measurements were made weekly to monthly intervals and (P6137, L25) and for a period of one or more years (P6138, L4). However all flux data are expressed per hour basis. It would be helpful to express the result on per year basis so comparisons with other studies and in some cases the relationship with other environmental variables such C/N ration and NO3-leaching in Fig 6 (expressed as per year) would more appropriate.

Authors reply: We agree that a per year basis is more appropriate in this case. Thus

figures tables and numbers in the text have been changed

19. This would also be appropriate if the data to be expressed as Global warming Potential (as suggested by Referee 1 and I do agree with) for discussions of GHG responses.

Authors reply: See above reply to reviewer #1.

20. The measurement period for each site could be added to Table 2. Authors reply: This is a good point and we added the period.

21. - I was confused at times with the site and code listing and agree with Referee 1 that the authors need to be consistent through the manuscript and also suggest delete Fig 2.

Authors reply: We checked consistency throughout the paper. We used the traditional way: the first time a site was mentioned the entire site name was spelled out followed by the two letter abbreviation in brackets. The abbreviation was then used consistently throughout the manuscript.

22. In Table 2 it would be better to put site code in a separate column similar to Table 1. Authors reply: Done. This has also been done in Table 3.

23. - In the Abstract (first line) suggest to indicate that what you mean by air pollution here is N-deposition. Authors reply: We have added the following "...air pollution in the form of nitrogen (N) deposition,..."

24. Also the authors mention that "The impact of these changes on forest GHG balances is currently difficult to predict" but no explanation was given. Need a sentence to explain why e.g. due to methodologies or lack of comprehensive and reliable data for modelling?

Authors reply: We have changed the sentence to the following: "The concurrent impact on the forest greenhouse gas (GHG) balance is currently difficult to predict due to a

lack of knowledge on controlling factors of GHG fluxes and response to changes in these factors."

25. - In the Discussion 4.1 (P6141, L3) change Fig 4 in (Table3, Fig 4) to Fig 3 as you are refereeing to N2O. Authors reply: Done

26. - There is a lot of discussion on the wood ash and its impacts effect on GHG fluxes but no mention of the measured pH. It is important to mention this so that readers that are not familiar with wood ash know that the pH could be as high as 12 or more. Suggest putting this in Table 3.

Authors reply: As mention above in response to reviewer #1 we have specified the high pH of the ash. Also we mention now added (to page 6143, line 21 in Discussions version) that the ash addition leads to 'increasing soil pH by 0.3 in the top 5 cm layer'

27. - P6140, L 11, you mention that "The mean CH4 flux for the treatment sites was -4. . .". Can you indicate what you mean by "treatment sites", I could not see how you came to the value of -4 ug CH4-C m-2 h-1 in Fig 4 ?.

Authors reply: The reviewer is right that this number seems to come out of the blue. It is not possible to extract this number from the figure. The number of -4 ug CH4-C m-2 h-1 is simply the arithmic mean of CH4 fluxes across all the treatments that we included in the study. We have clarified how we came by this number in the text with the following sentence: "The mean CH4 flux calculated for all the treatment sites (excluding control sites) was -4$\pm$6 μg CH4-C m-2 h-1,. . ." However recalculate to per year.

28. - Suggest delete Fig.1 as all the information are shown in Fig 7 and mentioned in the text. Authors reply: Suggestion taken.

Interactive comment on Biogeosciences Discuss., 9, 6129, 2012.