Biogeosciences Discuss., 9, C2096–C2099, 2012
www.biogeosciences-discuss.net/9/C2096/2012/

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.

Anonymous Referee #4

Received and published: 25 June 2012

General:

The study presents trace gas fluxes from peat soils and shrublands located in regions with different climatic conditions in Europe. At each site, the effect of a specific manipulation measure, such as drainage, drought, N deposition etc., was tested. Beside the effect of manipulation, this approach with study sites along "climatic gradients" also gives the chance of evaluating the impact of climate change on trace gas budgets of natural ecosystems. Generally, the paper address questions within the scope of BG and helps us to improve our fragmentary insight into the complex interactions of drivers for the release/consumption of trace gases in soils. The introduction is well written and

gives a very good overview of the state of knowledge and it clearly identifies the aims of the study.

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.

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.

Further remarks:

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.

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.

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.

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

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

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

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 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?

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.

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!

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

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.

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