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Comparison of soil greenhouse gas fluxes from extensive and intensive grazing in a temperate maritime climate

by Skiba et al, Dec 2012

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We wish to thank the two reviewers for their constructive comments. Most of the comments have been considered in the revised document. The revised manuscript is certainly a better product and we hope the reviewers will be satisfied.

Reply to Anonymous Referee #1

1) General comments: To estimate the GHG fluxes for different areas, for different countries is essential in calculation of GHG budgets on country or larges scales. The default values for fluxes in GHG budget models are simplified and rough, causing one order error in predicted values. It is important to get as many information by field measurements, as possible, both for validation of models and to get more precise default values in models. Though methodology in the manuscript based on routine measurements, and there are several papers in the literature dealing with GHG balance at similar surface, I strongly believe the paper has much useful information. I recommend publishing in BG after a revision.

Author reply: Many thanks for your supportive comments

2) Specific comments: Authors measured the soil respiration but it does not need for GHG budget calculation, anyway it is an interesting information, it would be nice to see the NEE, soil exhalation and the difference of these too, i.e., the uptake in one diagram.

Author reply: Yes indeed, soil respiration data provide interesting information on the overall CO_2 budget. The relationship of NEE with CO_2 respiration is shown in Figure 5, using monthly total fluxes.

3) Page 10059, row 5: it is a question whether N2O emission after application of mineral fertilizer is anthropogenic or not.

Author reply: We have changed this phrase to '*including emissions from seminatural/natural ecosystems'*

4) Page 10061, rows 21-22: it has not sense to indicate the detection limits (0.2 and 1.3 ppb) when background concentrations are 320 and 1900 ppb. Would be more informative to calculate the detection limit for fluxes, after a statistical analysis, rows

Author reply: We have changed this to : The minimum detectable flux was 3 μg N_2O m^{-2} h^{-1} and 6 μg CH_4 m^{-2} h^{-1}.

5) 22-26: I miss an equation for calculation fluxes including conversion factors.

Author reply: The static chamber is the most widely used method to obtain soil N_2O and CH_4 fluxes. We really don't think it is necessary to include this equation, which requires more space and by now is common knowledge. However we failed to include that standard atmospheric pressure and temperature were used in the conversion of concentrations from ppm values to ug/m3. This information has now been included. Thank you for pointing this out to us.

6) Page 10065, row 5-6: significant relation is mentioned in contrast to the sentence on page 10064, rows 25-28.

AND

Page 10068, rows 15-18: if water table affects nitrous oxide and methane emissions in the same manner, and large CH4 and small N2O emission occur when water table is at the top 7 cm layer it is a contradiction. What does it mean: both gases were absorbed; does it mean they remained absorbed? Authors should have been treat the problem more precisely, i.e. role of soil wetness in anaerobic methane production, point out that there is an optimum soil wetness for N2O emission (not for production), production of N2O is fast also close to saturation but remained in soil solution with no emission making possible the denitrification up to N2 et c. Anyway Fig. 3 says nothing; there is no significant relationship between fluxes and water table. It would be easier to understand, if we take into account the critical role of soil temperature in soil production. Monthly mean values range the whole year including warm and cold periods. A two variable analysis (in one diagram) would be better as the dependence of individual fluxes on soil temperature and on soil moisture (at the typical layers of both anaerobic decomposition and denitrification).

Author reply to both points: apologies for these inconsistencies arising from calculating regression equations using all 78 data points (p 10064) but only using monthly average data (p 10065). This has all been tidied up now. Figure 3 now shows CH_4 fluxes in relation to water table, and the relationship with temperature is explained in the text. We have cut the N_2O data from Figure 3 and also limited our description of these data to two sentences. The text now reads:

'At AMo CH_4 fluxes ranged from -38 to +604 μg CH_4 m^{-2} h^{-1} between January 2007 and December 2010 and the average flux was +37μg CH_4 m^{-2} h^{-1} (Figure 2a). Methane fluxes correlated significantly with water table height ($p < 0.05$, Figure 3) and soil temperature at a depth of 0.4 m ($p < 0.05$), but not with soil temperatures at 0.1 or 0.2 m depth, air temperature or cumulative precipitation that fell on the day and 6 days before flux measurements were made. A net CH_4 uptake dominated when the water table was below 0.2 m; then the average flux was -8 μg CH_4 m^{-2} h^{-1} with a range from -38 to +18 μg CH_4 m^{-2} h^{-1}. When the water table was closer to the surface (> 0.2 m depth) CH_4 emissions prevailed; the average flux was 51 μg CH_4 m^{-2} h^{-1} with a range of – 6 to +604 μg CH_4 m^{-2} h^{-1}. Methane fluxes > +100 μg CH_4 m^{-2} h^{-1} were only measured when the soil temperature at 0.4 m depth was at and above 10^oC, expect on one occasion when at 5 ^oC a flux of +161 μg CH_4 m^{-2} h^{-1} was measured.'

The equivalent N_2O section was shortend to the following sentence
'At AMo, the average N_2O flux was -0.22 μg N_2O-N m^{-2} h^{-1} (range -29 to +36 μg N_2O-N m^{-2} h^{-1}) (Figure 2a). Correlations between N_2O flux and water table height, air and soil temperature, heat flux or precipitation were not statistically significant.'

Reply to Anonymous Referee #1

General comments:

1)

The paper presents the measurements of GHG fluxes of two NitroEurope supersites in Scotland run by CEH. Both sites are grazed grassland systems with a large difference in grazing intensity and fertilizer input. The presentation of site specific papers is important as these data are further used in up scaling and validation and verification of different models.
The paper presents a synthesis GHG fluxes on the field scale of the four year 2007 – 2010 corresponding to the duration of NitroEurope with the focus to compare the annual GHG budgets of two sites. Annual budgets are expressed in CO2 equivalents per m2 and year of the experimental fields. The GHG fluxes are dominated by the CO2 exchange. The cumulated annual NEE represents the net C exchange of the fields. A negative number means that the ecosystems gain carbon during the specified year. Whereas the Auchencorth Moss (AMo) data seems consistent and are perfectly within the expected range of low input systems, the Easter Bush (EBu) data are inconsistent. Due to the sparse information on the management it is difficult to chase reasons for the inconsistency. More detailed information is needed on the variability of the stocking density and the management over the last 10 years. As the EBu field was part of previous major program such information must be available.

Authors reply: We have included more detailed information of the management. Table 2 now contains annual LSU and N fertiliser application for each of the 4 years and the below paragraph has been added to section 2.1:

' The fields have not been ploughed in the last 20 years, and were only cut on 3 occasions, once in 2002, 2003 and 2004. The fields are grazed all year round, and animals always have access to the entire field. Grazing occurs all year round, but livestock is occasionally taken of the field for periods of 1 day to several weeks. Annual livestock numbers were calculated from farmers and our own records. Between 2002 and 2006 the average stocking density was 0.7 LSU (Livestock units) ha^{-1}, and consisted of beef cattle (30%) and ewes + lambs (70%). Lambs are only present between April and September. To maintain the high stocking densities the grassland received predominately granules of mineral N fertiliser, either as ammonium nitrate or a NPK compound fertiliser between March and August. The average annual N fertilisation rate between 2002 and 2010 was 245 kg N ha^{-1-} y^{-1}. Since 2002 cattle slurry was only applied in September 2004 (69 kg N ha^{-1-} y^{-1}) and March 2005 (158 kg N ha^{-1-} y^{-1}).'

2)

The cumulative NEE at EBu are much higher in the years 2009 and 2010 compared to the two previous years. As the authors stated themselves this can hardly be explained.
The management do not show any major changes over the four years. Only the stocking density was reduced by approx. 20% over the last two years. The dry weather

conditions in spring 2009 rather would point to a reduced productivity and a reduced NEE in contradiction with the presented results.
The EBu field is largely dominated by a Lolium perenne. Such a system has a potential high yield (over 10 t DM per ha and year) with optimal management and good meteorological conditions. The high NEE of the last two years approximately corresponds to a yield of 12t DM per hectare. That is on the upper limit of perfectly managed Lolium perenne systems under a mown management according to the English guide for fertilization (Fertiliser Recommendations for Agricultural and Horticultural crops7th edition ISBN 0 11 243068 9). The management in EBu is suboptimal as the animal intake only uses up to a third of the potential productivity. Consequently "old grass" is produced over the years that will negatively influence the productivity and will not be eaten any more by the animals. In such situation a usual procedure of farmers is to cut the field to maintain the productivity. I see possibilities to check whether the sudden increase of the NEE values from 2008 to 2009 is plausible. These are:
Compare diurnal variations of NEE measurements between 2008 and 2009 on days with similar temperature, radiation and soil humidity. An amplitude shift could give a hint of a potential scaling error. Compare temperature dependence of nocturnal EC fluxes with soil respiration chamber values, are these consistent over the four years? Compare cumulated NEE with vegetation parameters such as LAI, canopy height, potential yield based on enclosure cages.

Authors reply: Thank you for your helpful suggestions. We carefully investigated the data again and changed the last paragraph in section 4.2 to:

'The large differences in EBu NEE flux between 2007/2008 and 2009/2010 (Table 2) implies a larger CO_2 sink in 2009/2010 during the annual growing seasons (April – September) (Figure 6). There is no reason to believe that a systematic bias was introduced between the years 2008 and 2009 as the eddy-covariance measurement system was not modified nor moved and the reanalysis routine remained the same. However, environmental and management conditions were slightly different in 2009 and 2010 compared to 2007 and 2008.Total annual rainfall was 12% lower in 2009 and 2010 compared to 2007 and 2008, but rainfall distribution and number of days with precipitation < 5 mm during the growing period (April to September) did not change. Air temperature patterns did not differ significantly. but, there was a rise in total solar and net radiation (both by 11%),PAR (16%) and a 17 fold increase in soil heat flux. Most importantly sheep stocking densities declined over the years and fell to 25% and 40% of their 2007 levels for the April to September periods, which correspond approximately to the annual growing season in 2009 and 2010, respectively. It is likley that the changes in NEE observed in 2009 and 2010 were caused by the partial removal of a source of CO_2 (the sheep) rather than a significant change in uptake or the introduction of a methodological bias.'

The comparison of sheep numbers and NEE is shown in the below figure. However, as the main remit of the paper is compare Auchencorth and Easter Bush fluxes, we would prefer to safe these data for a more detailed Carbon paper (2002 – 2012) planned to be written soon.

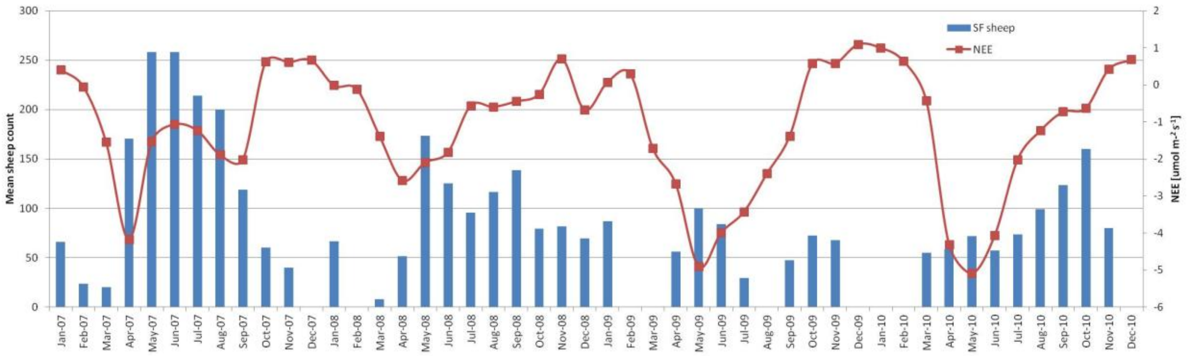

Figure 1: Net ecosystem exchange and sheep numbers at Easter Bush from January 2007 to December 2010.

3)
It is important to check the reliability of the NEE data also in relation to the N2O fluxes. E.g. if the cumulated N2O flux is expressed as fraction of the yield, the years 2009 and 2010 do have a very low emission factor and the two years before a very high emission factor. This could be a pure random effect, as the chamber measurements are extremely variable and also have a high variability among the chambers. It is also possible that in the last two years fertilizer induced peaks are underrepresented, while in the first two years they are over represented by the chosen gap filling algorithm.

Authors reply:
The main reason for smaller N2O emission factors in 2009 and 2010 is the lack of rainfall during the fertilisation period. We have added the following information to section 3.2:

'Smallest emission factors were calculated when the cumulative rainfall was < 55 mm, and where all fertilisations in 2010 and the March and May 2009 and May 2008 fertilisations (Figure 4).'
In addition we have changed the symbol type in Fig 4, to identify different years. The figure legend reads:
'Different symbol types represent emission factors in 2007 (circle), 2008 (triangle), 2009 (dimond), 2010 (square)'.

4) I cannot recommend the paper for acceptance in BG as the data analysis and the interpretation have major deficits.

Author reply: Thank you for all your useful comments, which we have taken on board. The result hopefully is a much better paper, which we hope is acceptable to you.

A selection of specific details:
5)
Abstact: The authors are using this sign convention inconsistently. In the abstract the average GHG budget is positive, while in Table 2 a negative value is given.

Author reply: We have added the negative signs to the GHG budget values in the abstract.

6)
page 10059 lines 17-20. At least a brief summary of the management history of the

EBu field must be given. Have been there cuts and/or plowing over the last 20 years?

Author reply: as far as we know the field has not been ploughed in the last 20 years and And only cut 3 times: in 2002, 2003, and the last cut was in 2004. This information has been added to section 2.1.

7)
page 10059 lines 24-26. The description of the stocking density is unclear. The average density is given as LSUha-1, then the range of the animal numbers are given as an absolute number. The range must be indicated as LSU per hectare. It would be important to have more information on the temporal variation of the stocking density.

Author reply:
We have clarified the stocking densities, the text in section 2.1 now reads:
Auchencorth*:'The moss is extensively grazed by sheep at a very low stocking density of 0.2 LSU ha^{-1}, which equates to circa 1 ewe ha^{-1}, accompanied by 1-2 lamb during April – September.'*
Easter Bush: '*From 2007 to 2010 the fields were stocked with sheep at an average annual stocking density was 0.77 LSU ha^{-1}. Animal numbers ranged from 0 to 170 ewes (0 to 17 LSU ha^{-1}) and 0 to 260 lambs (0 to 10 LSU ha^{-1})'.*
Table 1: The LSU in Table 1 have been included for individual years, 2007, 2008, 2009, 2010.
8)
Could the animals freely move over the whole field or was it a kind of a rotational grazing established?
Author reply: we added this information '*Livestock always has unrestricted access to the entire field'* to section 2.1

9)
page 10061 lines 18 – 24. Most important is to give the precision and not the detection limit for the GC measurements. The fluxes have been evaluated from the linear increase in the headspace. For N2O as criterium r2= 0.96 was taken. Our own analysis based on a reevaluation of the chamber data with the HMR algorithm (Pederson et al, 2010, European Journal of Soil Science, December 2010, 61, 888–902) show on average an underestimation of roughly 30% if the r2 = 0.96 criterion is used. The same holds for the CO2 chamber measurements. Such an analysis is very crucial as a 10% underestimation of the respiration flux is equivalent to the annual NEE derived from EC measurements for the years 2007 and 2008.

Author reply: We have changed this to:
'The minimum detectable flux was 3 g N_2O m^{-2} h^{-1} and 6 g CH_4 m^{-2} h^{-1}.

10)
page 10064 lines 1-4. An overview should be included showing data coverage and precise criteria how malfunction of the LICOR device were filtered out. This is important as animals in the footprint can cause rapid fluctuations of the concentrations that are real and eventually filtered out.

Author reply: All data were filtered and the following information was included in section 2.3

'The data were filtered. CO_2 molar fractions not in the range 350 ppm to 500 ppm and ½ hourly data (1) with standard deviations of mean vertical wind velocities > 1 m s^{-1}, (b) with total sample numbers < 30000 and (c) not meeting the criteria of the stationarity test described in Foken and Wichura (1996) and, Foken et al.(2004) were excluded from the analysis. The annual data coverage, expressed as a percentage of total annual ½ hourly data before filtering ranged from 76% - 85% and after filtering from 57% - 77%.

	Raw data [%]	Filtered data [%]
2007	82.4	53.6
2008	78.2	56.6
2009	85.2	75.4
2010	81.6	77.2
2011	75.9	62.4

Table 1: annual eddy-covariance data coverage (expressed as a percentage of the total annual half-hourly data) at Easter Bush before and after filtering. This table will not be included in the manuscript.

11)
page 10064 lines 6-10. There is a hole in the precipitation series from EBu shown in Figure 1a. The reasons seems to be snow disrupting the measurements. Most mean diurnal temperature are clearly above zero degree in this period, so snow seems for EBu seems the wrong reason for the malfunction.

Author reply: We looked at our rainfall data again and also those recorded by other local met stations, and revised text in section 3as follows:

Neither site normally receives more than 50 mm of snow, except in 2010, when snow disrupted precipitation measurements from 1st January to the beginning of March 2010 followed by no precipitation until beginning of April.

12)
lines 14-15. Average N2O fluxes of N2O at EBu are 507 times larger than at AMo. This is a strange ratio, as AMo fluxes are +- zero and the ratio is undefined.

Author reply: We have changed the text in section 3 to :
'Average fluxes of CO_2 respiration and NEE over the 4 years were 5 and 3 times larger at EBu than at AMo, respectively and CH_4 fluxes were 5 times smaller at EBu than at AMo. Nitrous oxide fluxes were around zero at AMo and 2 orders of magnitude larger at EBu.'

13)
page 10066 lines 12-14 The scaling of Figure 4 must be wrong. Figure 4 shows a mean emission around 20% of the applied fertilizer or roughly 40 kg N ha-1 year-1 in contradiction to the values given on page 10069 line 13.

Author reply: Thanks for spotting this mistake, we had spotted it too and corrected the y axis in Fig 4 accordingly.

14)
page 10070 lines 8 -10 the authors stated that annual cumulative precipitation and average annual temperature explained 85% of the interannual variation in respiration rates. I doubt whether it is meaningful to indicate an explained variability for four data point and two variables. There are not many remaining degree of freedom in this regression analysis.

Author reply: Agreed, this sentence was removed.