Biogeosciences Discuss., 9, C1446–C1454, 2012
www.biogeosciences-discuss.net/9/C1446/2012/

Biogeosciences Discussions

Interactive comment on "N_2O emission from organic barley cultivation as affected by green manure management" *by* S. Nadeem et al.

S. Nadeem et al.

shahid.nadeem@umb.no

Received and published: 24 May 2012

Anonymous Referee #2

The manuscript reports the results of an experiment that explored the effects of different green manure management strategies on emissions of N2O. The manuscript is generally well-written and the experiment is well-planned and the methodology for measuring N2O in line with current methodologies. However, I have some concerns over the measurements and the presentation of the results.

A.R We thank the reviewer for his/her positive appraisal of the experiment and try to improve the way the results are presented by responding to the criticism point by point:

R2-1: The sampling of soil for mineral N is not totally clear to me. On page 2314 (line

21) it is stated that 8 soil cores were sampled per treatment. However, it does not say how many cores were taken per plot (or even if the block structure we used for this sampling). Was this pooling done before analysis of the samples? Does this mean that only one composite sample were available for each treatment and thus no replication across blocks in the experiment? If this is the case, it would not be possible (or very difficult) to analyse statistical differences between treatments in soil mineral N. Is this the reason for omitting error bars in Figures 1 and 2.

A.R. 8 soil cores per treatment were sampled from two plots (4 cores per plot in close vicinity of microplots used for flux measurements) and then pooled to one composite sample per treatment which was analysed for 2M KCl extractable NH4+ and NO3-. Thus, the presented mineral N values are the results of one composite sample per treatment. We pooled soil samples because we considered it as more important to follow the temporal dynamics than exploring statistical differences in mineral N between treatments. We choose temporal over spatial resolution because we expected clearer contrasts in the mineral N dynamics in the various treatments. We are aware that there is a trade-off between temporal and spatial resolution in our study, but logistic reasons (i.e. lack of funding) prevented us from studying both. Consequently, there are no error bars for mineral N in figures 1 and 2.

R2-2: The soil air probes appear to have been installed as two replicate sets per treatment. Does this mean that the block structure of the experiment was not used? Which plots were then equipped with the SAPs? How was this design used for statistical analyses?

A.R.We agree that information about where the soil air composition was measured is missing. We initiated these measurements late in 2009 by installing one set of soil air probes (3 depth) in each of the two plots used for the treatments G-3M, G-0M(B) and C-(B). These treatments were selected because we were interested to see whether GM ley and mulching would result in different patterns of N2O accumulation throughout winter (as compared to the cereal reference) and whether this could be used as an

indicator for high winter/early spring N2O emissions. Given the complex regulation of N2O in soil air throughout winter and its unclear relation to N2O emission flux in spring, we consider these data as a qualitative rather than quantitative indicator and did not perform any statistics on these data.

R2-3: The section on statistical analyses is extremely weak, since it does not describe how the layout of the experiment and of the measurements was taken into account in the analyses.

A.R. We agree with the reviewer that the experimental layout of our study did not exploit the randomized complete block design given by the main experiment on GM effects on barley yield. In manually operated trace gas studies, only a limited amount of microplots can be observed in a reasonable timeframe. In our case, we used 24 microplots for gas exchange measurements. Instead of having one microplot per plot, we opted for a somewhat assymetric design with two replicate microplots placed closely next to each other in two of the four available plots per treatment. Thus, only 2 of the 4 available blocks were covered (see MM section). This was done to compare small-scale intra-plot variability within one treatment with inter-plot variability across the experimental field. Surprisingly, we found no temporally consistent patterns of variability on the two different spatial scales. We tested the block effect on cumulative N2O emissions in the 2 remaining blocks by 2-way ANOVA and found no significant effect. Therefore, the block factor was excluded and the four replicate plots are treated as independent replicates. This information is now given in the Materials and Methods as well as in the Results section.

R2-4: The concept of “biorest” is not properly defined. I suggest calling this “biogas residue”.

A.R Corrected in the revised manuscript

R2-5: My main concern with the paper is that a relatively low frequency of measurements of N2O was used. This is a particular problem for the measurements in

spring 2010, where field operations prevented measurements during several weeks after ploughing the green manure. The measurements may therefore have missed important N2O emissions following ploughing. This could invalidate the calculations of annual emissions and conclusions concerning their ranking. The discussion already mentions this, but still goes on to estimate seasonal emissions and also compares these to measured yields. The caveats concerning the missing measurement periods are not sufficiently discussed for these estimations.

A.R. Under the climate conditions prevailing in Norway and in the snow-rich winter 2009/10, conventionally operated whole-year-round chamber measurements were not impossible. The lack of winter measurements and their possible effect on annual emissions are dealt with at the beginning of the discussion. Likewise, we address the lack of emission data during the critical phase after ploughing. We agree with the reviewer that this was unfortunate, since there seems to be considerable uncertainty about the short term effects of tillage and/or GM incorporation on N2O emissions. We have rephrased the entire section in the discussion on plough effects and now also point at the caveats arising from missing data.

R2-6: The paper discusses the effects of soil water, NO3 and NH4 on N2O emissions based on the temporal developments shown in Figures 1 and 2. This is very difficult to follow, and may just be based on circumstance. I strongly suggest a statistical analysis, where the emissions are related in a multiple regression analysis with soil mineral N, soil water content and temperature.

A.R. We discussed the temporal dynamics of N2O emissions in relation to ancillary variables (soil temperature, daily rainfall, WFPS, soil NH4+ and NO3-) in some detail because we expected a shift in environmental controls given the contrasting cultivation regimes in 2009 and 2010. In addition there was considerable inter-annual variation in weather (warm dry in 2009; cool wet in 2010). A discussion like this necessarily remains “circumstantial” as N2O field fluxes depend on the interplay of variables governing different microbial processes (nitrification and denitrification) as well as diffusion

conditions etc., commonly resulting in insignificant coefficients in multiple regression analyses. Prompted by the reviewer's suggestion, we performed stepwise multiple regression analysis and found only a week relation between N2O emission flux and soil temperature and WFPS in 2009 (R adj=10%). The reasons for this low coefficient of determination are given in the discussion (e.g. low emission fluxes at high WFPS towards the end of the year). In contrast, multiple regression analysis for 2010 revealed a significant relation between N2O emission flux and soil mineral N content (especially NH4+) and soil temperature (R adj= 25.7%). We added the regression coefficients to the result section and discuss them in detail in the Discussion section.

R2-7: Figures 1 to 3 do not show error bars for the measurements of soil mineral N and N2O. This limits the interpretation of temporal changes in treatment effects

A.R We now indicate dates of significant difference on N2O flux among treatments by asterisks in the figures (see also response to reviewer 1-4).

R2-8: The text in some cases mentions "nutrient", where the right word would be "nitrogen".

A.R Changed in the revised manuscript

R2-9: I am missing a table on the amount of N recycled in organic residues. This could be combined with data on the amounts of organic and mineral N applied in biogas residues and fertilizers.

A.R. This issue was also raised by reviewer 1 and we have now included an additional table 6 where we list the organic and mineral N values in mulched above ground biomass, biogas residue and mineral fertilizer (see also response to R1-5).

R2-10: I recommend the paper for major revision.

A.R. We appreciate your important and valuable critique and try to improve the quality of the paper along the lines of your comments.

R2-11: Page 2308, line 23 Change "green house" to "greenhouse".

A.R. Changed

R2-12: Page 2310, line 7 Change "applied" to "returned".

A.R. Changed

R2-13: Page 2312, lines 10-12 This sentence is not needed. A.R. The sentence is deleted in the revised manuscript

R2-14 Page 2317, line 12 Change "NH4-" to "NH4+".

A.R. Changed

R2-15: Page 2318, line 14 The low sampling frequency does not allow any conclusions on short-lived peaks.

A.R. We agree that the low frequency doesn't allow us to conclude short lived peaks after ploughing. We change the sentence from ËİafterploughingËİ to ËİduringthegrowingseasonËİ (see also response to R1-1).

R2-16: Page 2319, lines 24-27 It should be mentioned that the relative emissions are based on total-N applied. Some of this total-N is in organic form that would not be available for microbial turnover (or emissions). This could likely be the reason for higher emissions from mineral fertilizer.

A.R. We are aware of the fact that not all N in GM or biogas residue is available for microbial turnover (or emissions) directly upon incorporation. However, the added organic N may be considered as a cohort, which releases available N throughout the following years. We now give the time horizon of one growing season after incorporation as a basis for our emission factor thereby neglecting N2O emissions arising from N –mineralization from this cohort during later years. It was outside the scope of our study to assess the amount of biologically available N deriving from GM at every time point (see also response 1-6).

R2-17: Page 2320, line 16 Which “agronomic field operations”?

A.R. Field operations were marking of plots, ploughing, application of biogas residue and mineral fertilizer, harrowing and sowing. Due to high snow packs in 2009/10, the soil was quite wet in spring. In addition, abundant rainfalls occurred after ploughing. Staff responsible for the field operations waited therefore for sowing until the field dried up. This prevented us from installing the equipment in the field earlier.

R2-18: Page 2320, line 22 Change “years” to “seasons”.

A.R. Changed in the revised manuscript

R2-19: Page 2321, 8 to 15 I think a better statistical analysis could improve the interpretation here. It may not be counterintuitive to have a negative relationship between mineral N and N2O if other factors are overruling the effects. Also I am not convinced that N turnover governs emissions, when no substantial analysis of the relations has been performed.

A.R. We now report multiple regression analysis in the Results section and discuss relationships between N2O emission and ancillary variables using Pearson’s correlations coefficients in the discussion section. The reviewer is right that we did not perform a “substantial analysis” of N-turnover processes, which was outside the scope of our flux study. However, given the fact that mineral N concentrations were not correlated with N2O emission in 2009 whereas soil temperature and WFPS were, we suggest that N2O emissions were primarily fueled by temperature and moisture dependent mineralization processes in the sward rhizosphere, since no extraneous nitrogen was available in the system in 2009. We hope that the reviewer can follow our reasoning at that point.

R2-20: Page 2321, line 18 Change “can be a sink” to “act as a sink”. Delete “aggressive”.

A.R. Changed in revised manuscript

R2-21: Page 2321, lines 21-28 This is not clear to me. Perhaps this can be illustrated

by a graph.

A.R. We deleted the entire paragraph about the WFPS limits critical for N2O emission known from the literature for as since our data do not reproduce these relationships; WFPS > 90% was only found on one date which does not allow to conclude this critical limit relationship.

R2-22: Page 2322, line 1 What do you mean by “discussed”?

A.R. Here “discussed” means ËİreportedËİ in the literature. None of these studies are conclusive so we used the word “discussed”, as it is merely impossible to prove direct production of N2O during N fixation in field studies.

R2-23: Page 2322, lines 7 to 8 Where does this value of the N surplus come from. I cannot find it from Table 3. I also does not find “surplus” to be the right word.

A.R. We used the wrong table number in the text. It should have been table 4 instead of table 3; The “surplus” of cumulative N2O attributable to mulching was calculated as the difference between the treatments G-0M/G-0M(B) and G-3M which is given in table 4. We removed the term “surplus” and rephrased the sentence (see also response to R1-3).

R2-24: Page 2322, line 17 How do you know the N2O emissions were higher in G-3M? Was any statistical analyses performed?

A.R. We tested differences for single dates by ANOVA and now indicate dates with significantly different emission rates by asterisks in figures 1 and 2. For the spring measurement, it was shown that G-3M had significantly higher emissions on April, 4th. See the revised figure 2.

R2-25: Page 2323, lines 8 to 11 I don’t think the data allows this conclusion.

A.R. We have rephrased the entire paragraph and now discuss the likely effect of fresh organic matter incorporation on N2O emissions during the period of missing data.

R2-26: Page 2323, lines 16 to 18 How do you know that GM effects only became evident after 7 weeks after ploughing, since measurements were missing before that time? Also were there any significance tests of this?

A.R. We have rephrased the sentence and now indicate dates of significantly different flux emission by asterisk in figure 2.

R2-27: Page 2324, lines 10 to 12 This conclusion is very speculative, and I see no substantial evidence in the results.

A.R. We have rephrased the sentence

R2-28: Page 2324, line 19 The paper by Thomsen et al. (1993) is on nitrate leaching from animal manure and mineral fertilizer. How can this be used to argue for N2O emissions following a green manure?

A.R. The reviewer is right; we omitted the reference from the text

R2-29: Page 2325, line 7 It is not clear what is meant by a “small cooling effect”.

A.R. Small cooling effect means that soil can act as sink for CH4 thus reducing the global warming. We replaced the term “cooling effect” in the revised manuscript.

R2-30: Page 2325, line 8 Change “fermenting” to “digesting”.

A.R Corrected

Interactive comment on Biogeosciences Discuss., 9, 2307, 2012.