

## ***Interactive comment on “N<sub>2</sub>O emission from organic barley cultivation as affected by green manure management” by S. Nadeem et al.***

**Anonymous Referee #2**

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### General comments

The manuscript reports the results of an experiment that explored the effects of different green manure management strategies on emissions of N<sub>2</sub>O. The manuscript is generally well-written and the experiment is well-planned and the methodology for measuring N<sub>2</sub>O in line with current methodologies. However, I have some concerns over the measurements and the presentation of the results.

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

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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.

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?

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.

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

My main concern with the paper is that a relatively low frequency of measurements of N<sub>2</sub>O 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 N<sub>2</sub>O-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.

The paper discusses the effects of soil water, NO<sub>3</sub> and NH<sub>4</sub> on N<sub>2</sub>O 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.

Figures 1 to 3 do not show error bars for the measurements of soil mineral N and N<sub>2</sub>O. This limits the interpretation of temporal changes in treatment effects.

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The text in some cases mentions “nutrient”, where the right word would be “nitrogen”.

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 fertilisers.

I recommend the paper for major revision.

Specific comments

Page 2308, line 23 Change “green house” to “greenhouse”.

Page 2310, line 7 Change “applied” to “returned”.

Page 2312, lines 10-12 This sentence is not needed.

Page 2317, line 12 Change “NH4-“ to “NH4+”.

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

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.

Page 2320, line 16 Which “agronomic field operations”?

Page 2320, line 22 Change “years” to “seasons”.

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 N<sub>2</sub>O, 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.

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

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Page 2321, lines 21-28 This is not clear to me. Perhaps this can be illustrated by a graph.

Page 2322, line 1 What do you mean by “discussed”?

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.

Page 2322, line 17 How do you know the N<sub>2</sub>O emissions were higher in G-3M? Was any statistical analyses performed?

Page 2323, lines 8 to 11 I don't think the data allows this conclusion.

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?

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

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 N<sub>2</sub>O emissions following a green manure?

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

Page 2325, line 8 Change “fermenting” to “digesting”.

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