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Biogeosciences Discussions

Interactive comment on "Nitrous oxide emission reduction in temperate biochar-amended soils" *by* R. Felber et al.

R. Felber et al.

raphael.felber@art.admin.ch

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We would like to thank reviewer 1 for his critical comments and the numerous orthographical corrections. We are aware that the paper needs a thorough restructuration following the roadmap indicated by the reviewer. Essentially we were too ambitious with the interpretation of the data based in light of the results that we presented.

In the following we will address the comments in detail and clarify our intention and propose the improvements.

The aim of our laboratory experiment was to test whether incorporation of two types of biochars to two types of temperate agricultural soils show changes in N2O and CO2 fluxes. The experiments were repeated after the storage of the samples for three

months to mimic changes over a time span comparable to a vegetation period of agricultural plant species. We regard our experiments as an “entrance” test for the “applicability” of biochar in temperate agricultural systems. As biochar is not a well defined substance any measured effect will be specific to the type of biochar in combination with the type of the soil. These aspects will be clarified in the introduction during revision. We are aware that the presented data does not allow to investigate the significance of the several proposed mechanisms.

Furthermore the presented data should help to filled the lack of N2O and CO2 fluxes of biochar amended temperate soils compared to experiments in tropical soils.

The introduction part will be shorten addressing the above described aim of the experiments and the hypothesis will be removed from the introductory part.

The experiment was designed as a combination of three treatments (untreated samples, addition of glucose, addition of glucose and potassium nitrate) with three factors (two soils, two biochars and two points in time). Each individual combination was repeated three times in order to get information on the natural variability of homogenised samples, but not on the variability of the fields where the soils were collected.

As reviewer 1 points out, the main interest is whether addition of biochar has a significant effect on the level of the CO2 and N2O fluxes for a given treatment. We will restructure the results considering the suggested flow chart of the referee. Furthermore, we will add significance levels in Tab. 2 behind the reduction levels comparing whether biochar amended samples significantly reduced fluxes with respect to the sample without biochar (paired t-tests). This will give a more detailed picture than the ANOVA tables.

In the discussion part we will mainly focus on the effect of C and N availability, which we actually manipulated in our experiments. The discussion part will be shortened along with removal of speculative statements and section 4.2 will be deleted

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Please see responses to specific comments below.

Comment: *Title needs to be more descriptive of the experiment performed and reported on. E.g. 'Green waste and coffee ground biochar effects on N2O fluxes from repacked, temperate soils treated with glucose or glucose plus nitrate*.

Response: Thanks for the suggestion of this more descriptive title. But we think it is rather to long and there is no need to say what kind of biochars we used, since biochars even from the same feedstock differ widely (e.g. the pyrolysis temperature is of importance too). The term repacked may mislead too. It is often used if soil is transferred in layers e.g. into a lysimeter. To emphasize that we made laboratory experiments, we will add to our title (...) under controlled conditions.

Comment: *P153 L10 Why is it necessary to first test effects of biochar application on N2O in the laboratory first ?*

Response: It is certainly advantageous to test the reduction potential of specific biochar in the laboratory before applying in the field in order to sort out biochars which may have no effect at all. Laboratory incubations also allow to control factors which are known to influence the soils N2O production and thus to get a better handle on uncertainties.

Comment: *P153 L26 onwards needs to be clearer. Suggest 'The main objective of this study was to assess if N2O and CO2 fluxes from temperate, nutrient rich, agricultural soils were affected following biochar application at rates similar to those applied to tropical soils. A further objective was to assess if any biochar effects persisted following the ageing of biochar in the soil. A factorial experiment utilising 2 biochars x 2 soils x 3 amendments was performed in the laboratory with gas fluxes assessed over 3 month periods. Amendments included no additional substrate addition, glucose addition and nitrate+glucose addition'.*

Response: This part will be over-worked during revision as suggested above. The

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term over three month may mislead that the samples were measured several times during three months and will therefore be changed into (...) in the laboratory with gas fluxes assessed at the beginning and end of a three month storage period.

Comment: *: P157 L1 change ppm to SI units uL/L*

Response: We prefer to stay with ppm resp. ppb as this unit is commonly used in BG papers for measured gas concentrations, but we leave this decision to the editor and the publication office.

Comment: *: P158 How can you possibly continue and perform statistical analyses (requiring a normal distribution of the data) on data that you know is not normally distributed!*

Response: Typically chamber flux measurements values of several replicates are not normally distributed and a scaling procedure must be applied to get distribution that safely can be regarded as normally distributed. We reached this goal with the Yeo-Johnson power transformation. Due to a programming error the transformation was wrongly applied for the CO2 flux resulting in a violation of the normal distribution. Correcting for this also the CO2 values become normally distributed. The recalculation of the Anova statistic altered the values for CO2, but the significance levels did not change (except for the interaction year:biochar). The exact values of the ANOVA analysis given in the Appendix will be updated during revision.

Comment: *: P159 L26 I recall no mention of mineralization rate measurements in the materials and methods.*

Response: We admit that the term NO3- mineralization rate was used in the wrong way to say that there was an accumulation of NO3-. We will change that to NO3- accumulation and NH4+ accumulation, respectively.

Comment: *: P159 L29 How can you say the N availability increased in the absence of plants? Plants were not a treatment.*

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Response: We will remove this sentence as indeed plants were not a treatment.

Comment: *I am concerned that relatively large N2O reductions are being expressed, of the order of 62-98%, when as the authors state these numbers are based on relative reductions compared with the control BUT the reductions only appear large because the control fluxes were so low that any reduction becomes a large percentage. Really the treatments that should have been used to determine the effect of biochar on reducing N2O emissions should have included a treatment where N2O was readily able to be produced in a control situation i.e. a soil with adequate N substrate.*

Response: We are aware that the report of N2O reductions as relative percentages can be misleading for small fluxes. We therefore also reported the absolute fluxes. There might have been confusion caused by the concomitant discussion of both, absolute and relative emission reduction. We will make sure that this will be described more clear during revision. Regarding your concern that a control with a non limited N substrate situation should have been introduced as a treatment is difficult to realize. After measuring the first untreated samples we first thought that they are N limited and added potassium nitrate. The fluxes did not increase, hence we added in a second step glucose and the emissions went up (this data is not shown in the paper). After this experiments we decided to first add glucose to overcome C-limitation and then add glucose+nitrate. Because the three treatments ought to represent field situations of variable C and N availability there is no reason to focus on just one treatment. However, we see the point here and will clarify that issue during revision.

Comment: *P161 L1-6 Very confusing. Does it make sense?*

Response: This paragraph will be revised as follow: The applied amount of biochar do correspond to a typical amount that might be applied in field conditions. Compared to other studies it is a t the lower end. The second part (L7-9) are removed.

Comment: *P161 L24-26 Speculation is exactly what this is and it cannot be rationalised. IF you had measured inorganic-N overtime during the incubation then you*

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would be in a position to speculate on the results. But unfortunately you do not have this data and I do not believe you can speculate in this way.

Response: We will remove this sentence.

Comment: *P162 L25 How can you make this statement 'With ageing the effect of adsorbing nitrate at the biochar's surface sems to diminish: : :' Did you measure nitrate adsorbtion on your biochar materials before and after they had been in the soil for the period? If so how was this done. If not it is not correct to state what you have.*

Response: We do accept the statement of the reviewer. We will remove this sentence as we have not directly measured the absorption of nitrate at the biochar's surface.

Comment: *Did your biochars contribute C to the CO2 flux? I.e. actually contribute C. Some recent studies have clearly shown biochar C can be partially available after addition to soil (e.g. Smith et atl. SBB). I am uncomfortable with the speculation around this without good data.*

Response: With our measurements we cannot distinguish different sources of the respiration. CO2 fluxes from untreated samples were not significantly increased after biochar application. Therefore it is not likely, that biochars C contributed to the CO2 flux in our experiment. The CO2 fluxes from biochar amended samples were smaller than from the control. This is evidence that there was no important contribution of C as an energy source for micro-organisms or a priming due to biochar application.

Comment: *Section 4.3 is too long and speculative given the relatively small laboratory incubation – without plants, using repacked soil, at limited moisture contents etc.*

Response: We think that comparing the laboratory result to the field is of most importance. The duration of the experiment is comparable to crop production times. The treatments (untreated, nitrate, and nitrate+glucose treatments) can be related to specific events in the field and are therefore of interest and the corresponding application rates were chosen to mimic field situations. The soil moisture content was adjusted to

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values typically found at the Oensingen site. As shown in section 4.3. rates are similar as we can infer from our multi annual N2O record from the Oensingen site. To shorten Sec. 4.3 and get rid of some speculations we will omit from P164 L17 until L26.

Interactive comment on Biogeosciences Discuss., 9, 151, 2012.