

Interactive comment on “Biochar’s effect on soil nitrous oxide emissions from a maize field with lime-adjusted pH treatment” by R. Hüppi et al.

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Author response to anonymous review

We thank the anonymous reviewer for this review and the general agreement with our interpretation of the data. We are happy to include the helpful comments into our manuscript.

Comment: *The title is a bit confusing. It seems that biochar was applied in a limed soil. However, lime and biochar were added as different treatments. The title should be changed to make this clear.*

Response: We will change the title also in the sense of A. Gattingers comment to:

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“Effect of biochar and liming on soil nitrous oxide emissions from a maize field.”

Comment: *Abstract. It is in general ok, but it could be improved. For instance, it is stated that “laboratory incubations have shown significantly reduced N₂O emissions from soil when mixed with biochar”. This is true in average, but there are many laboratory studies that also found an increase in N₂O emissions after biochar treatment, and this seems to be linked to the specific mechanism leading to N₂O formation.*

Response: We will include this suggestion accordingly and mention adverse effects, especially in nitrification lab situations.

Comment: *Please include in the abstract what kind of N fertilizer was applied, since this could have important implications for the N₂O formation pathway. Include also average soil pH before and after liming.*

Response: We will add this information to the abstract.

Comment: *The last statement of the abstract is a bit simplistic. This is a field study, there are many factors interacting and out of control, so it is difficult to distinguish which is the role of pH. It could be important for N₂O produced by denitrification pathways, but at the same time it could have promoted nitrification emissions. . . so I would not simplify the conclusions in this way. The discussion about pH in the subsequent sections reflects better the conclusions of the study, for instance, in the conclusions “there is no evidence that the reduction with biochar, relative to control is solely induced by a higher soil pH”.*

Response: We very much agree on this comment and change the abstract to be inline with the conclusions.

Comment on Materials methods: *I am not convinced about removing data. Even if this won't probably change the final conclusions, low CO₂ flux does not necessarily*

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mean that the lids were not functioning properly.

Response: We accept low and even slightly negative CO₂ fluxes, to capture measurement uncertainty around zero fluxes. But there is no mechanistic explanation for substantial net CO₂ uptake from bare soil. Soil always emits CO₂ depending on temperature and substrate availability. As explained in a comment to the first reviewer also CO₂ uptake larger than 0.5 $\mu\text{mol}/\text{m}^2/\text{s}$ are a very robust indicator for technical problems not related to soil-atmosphere exchange. With this procedure we do not have to manually remove data points, what would have been difficult to document and justify.

Comment on Results: *It is frequently mentioned in the manuscript that the differences between treatments are not significant. I would de-emphasize this point. P values are not the “absolute truth”, and especially in N₂O studies (even more in the field) it is very infrequent to find “significant differences” according to traditional P values. Considering that you have calculated emission factors and also maize yields, it shouldn’t be much work to include also yield-scaled emissions.*

Response: We will include yield-scaled emissions (also suggested by Andreas Gättinger). We are happy about your perception of statistical significance in our dataset. Indeed it is very rare to reach $p < 0.05$ significance with this setting, especially with only 3 replicates. But, following the scientific rules we have to admit that we can’t see a significant statistical effect.

Comment on Discussion: *When you compare with other field studies where an increase in emissions was found after biochar addition, can you comment on the differences respect to your study? Were the soils different in these studies, and what about the biochars? Did the biochars used in these studies had low H:Corg atomic ratio and C:N ratio as in your case? These comparisons might be useful to define future field studies.*

Response: We will include more of such comparisons with other studies. However we can only speculate on possible mechanisms as long as we don’t know about which

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properties are important. We will leave larger efforts towards this direction to meta studies to come.

Comment: *The clarity of the figures needs to be improved. It is rather difficult to discern between treatments: symbols/letters are very small and not clear.*

Response: Thank you for this comment. We will improve clarity of the figures.

Interactive comment on SOIL Discuss., 2, 793, 2015.