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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 comments from Andreas Gattinger

General response: We greatly thank Andreas Gattinger for his fruitful comments and the appreciation for our work. We will improve the paper according his suggestions.

Comment 1. The treatment effect: *In its current version the N_2O emissions doesn't follow any significant treatment effect. This is due to the experimental design, where the treatments were defined according to its potential pH effect: "control", "biochar", "lime". In fact with the application of either biochar or lime, soil pH could be significantly increased relative to the control. However, for N_2O emissions the variations from the limed plots were that high that a final treatment effect on level p = 0.05 could not been*

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determined. If the standard error of the N_2O flux curves from liming would have been in the same range as the flux curves determined for the control or biochar plots, there would have been an effect on N_2O emissions. Anyhow, high variations from N_2O fluxes from field measurements are a quite common feature. Therefore, I suggest to report the data in two ways. First, as it is now, along with the observed phenomena. Second, following an experimental design which considers only the treatments "control" and "biochar". For this, the statistical analyses need to be revised, as the treatment "lime" will be removed from the statistical model. This, however, impacts on the objectives and hypotheses, thus they need to be adapted as well. It could be done in a way by saying that this experiment follows two lines: one is to observe any biochar effect on N_2O mitigation, the other one goes for causative research (pH effect) by adding a lime treatment to the experiment. Considering the suggestions made by R. Fuss will be straightforward to improve the statistical approach in general. Adapting the paper in that way impacts on the overall context, meaning that the impact of biochar alone may deserve more attention in the discussions section and that statements for a possible pH effect should be done more cautiously. As a further consequence from the re-arrangement of the paper, I suggest another title: "Effect of biochar and liming on soil nitrous oxide fluxes from a maize field."

Response: We will change the title considering your suggestion. We also see the option to separate the two research questions more strictly. However, this suggestion is in clear contradiction to the review comment by R. Fuss. We therefore decide to give room for both interpretations but prioritize the overall non-significance of treatments, also because this is in line with the generic use of statistical testing.

Comment 2. Crop yields: *The authors present crop yields from maize and its N and P uptake in figures 5 to 7. I suggest to replace the term "plant" by "aboveground biomass" to make it clearer. Furthermore, as already suggested by R. Fuss, I would report N_2O effects as 1) area-scaled and b) as yield-scaled N_2O emissions. This illustrations should ideally follow the same line as explained above, namely for the pH*

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effect (control, biochar, lime) and for biochar effect (control, biochar). The yield-scaled illustration of N_2O emissions provides an even stronger argument for a possible GHG mitigation effect of biochar as it impacts apart from N_2O suppression also on crop growth. These aspects needs stronger consideration in the discussion of the revised paper as well.

Response: We will include yield-scaled N_2O emissions. Like explained in other comments, this number will come with additional uncertainty from yield variability.

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