

## ***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.***

### **Anonymous Referee #2**

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The manuscript “Biochar effect on soil nitrous oxide emissions from a maize field with lime-adjusted pH treatment” reports a field study analyzing the impact of biochar or lime amendment on N<sub>2</sub>O emissions from a maize field. There are not many field studies on this topic. This study also calculates emission factors and maize yields, which is even less frequent. The experiments are well-performed; the paper is well-organized and well-written. I would recommend its publication after some corrections.

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.

Abstract. It is in general ok, but it could be improved. For instance, it is stated that “lab-

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oratory incubations have shown significantly reduced N<sub>2</sub>O emissions from soil when mixed with biochar”. This is true in average, but there are many laboratory studies that also found an increase in N<sub>2</sub>O emissions after biochar treatment, and this seems to be linked to the specific mechanism leading to N<sub>2</sub>O formation.

Please include in the abstract what kind of N fertilizer was applied, since this could have important implications for the N<sub>2</sub>O formation pathway. Include also average soil pH before and after liming.

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 N<sub>2</sub>O 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”.

Materials & methods. I am not convinced about removing data. Even if this won't probably change the final conclusions, low CO<sub>2</sub> flux does not necessarily mean that the lids were not functioning properly.

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 N<sub>2</sub>O 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.

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:C<sub>org</sub> atomic ratio and C:N ratio as in your

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case? These comparisons might be useful to define future field studies.

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.

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

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