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Biogeosciences
Discussions

Interactive comment on "Net soil–atmosphere fluxes mask patterns in gross production and consumption of nitrous oxide and methane in a managed ecosystem" *by* W. H. Yang and W. L. Silver

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The referee's comments are shown in quotes below and followed by our response to each comment.

"I believe that the authors have overstated the importance of N2O reduction in the Conclusion section. As shown in Fig. 2a, the N2O reduction amounts are clearly much less than 50% of the N2O production amounts in all cases."

We agree with the referee that we overstated the importance of N2O reduction in the

Conclusion section by stating that "gross N2O reduction rates were approximately half that of gross N2O production rates" (page 19183, lines 10-11). The statement should have read "approximately one third" to reflect the average N2O yield of 0.68 reported in the results section (page 19177, line 2).

"I don't see where the quantity "methanogenic fraction of C mineralization" is clearly defined; how was it determined/calculated?"

We appreciate the referee pointing out that we had conceptually defined the methanogenic fraction of C mineralization but not provided a description of how we determined this index. It was calculated as the gross CH4 production rate divided by the sum of the gross CH4 production rate and CO2 production rates. We can add this description to the methods section.

"I am aware of the discussion and questions regarding the underlying assumptions of the isotope dilution method used here. The method was originally published by Yang et al. 2011. Glob. Change Biol., 17, 3577–3588. doi:10.1111/j.1365-2486.2011.02481.x. which was followed by a letter from Well and Butterbach-Bahl (Global Change Biol. 2013, 19:133-135) and then by the authors' response (19:985-987). It would take some time to fully investigate the issues discussed in these letters. Because this is a relatively new and not widely used method, I do wonder if some reference to these latter two publications should be made in the current manuscript to alert the reader to these issues."

We had not referenced the Well and Butterbach-Bahl 2013 letter and the Yang et al. 2013 response because the methodological issues raised in the letter were actually addressed in the Yang et al. 2011 paper describing the pool dilution method applied to gross N2O fluxes. However, we can certainly cite the letter and response in this manuscript so that readers are aware of this discussion.

"Soil pH has been shown in some studies to affect nosZ activity. Was soil pH considered in this study?"

Soil pH was not considered in this study because it was assumed to not vary dramatically over the course of the growing season.

Interactive comment on Biogeosciences Discuss., 12, 19167, 2015.