

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

Anonymous Referee #1

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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.

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

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

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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.

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

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

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