

Interactive comment on “Seasonal variation of gross nitrification rates at three differently treated long-term fertilisations sites” by C. F. Stange and H.-U. Neue

Anonymous Referee #2

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Seasonal variation of gross nitrification rates at three differently treated long-term fertilisations sites C. F. Stange and H.-U. Neue

The manuscript is certainly within the scope of Biogeosciences Discussions. The title doesn't accurately reflect all that the manuscript tries to portray if we look at the objectives which were to validate the BaPS nitrification numbers against a 15N pool dilution technique, to see what influence soil climate had on sites with three different N histories AND to parameterize a model approach to assess soil temperature and moisture on gross nitrification in soils.

The manuscript is generally well written and I have only a few grammatical sugges-

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tions (see pdf sent to editor). The work is accurately performed and could be readily repeated by others.

However, I feel that the manuscript tries to do too much and would be a better read if it focused on only some of these objectives. At the moment the discussion becomes a very long read.

Is the work on the N₂O leakage rate essential for this manuscript? It is worth perhaps briefly noting the result and then referring to the reference of Ambus 2005. But after this I would suggest deleting this since N₂O production was not correlated with gross nitrification and this could remove almost 2 pages of discussion.

The conclusions present 'no conclusion' as to the validity of measuring gross nitrification with BaPS versus ¹⁵N pool dilution, but instead the first paragraph provides almost a quick overview of BaPS. In the second paragraph of the conclusions soil temperature is noted to be an important driver in seasonal gross nitrification. With respect I would consider that this is nothing new. The remainder of the conclusions do not link with the original objectives.

Perhaps the authors need to rephrase their objectives and ask what it is that their data shows that is novel. I would suggest that the modelling comparison is of great interest and that this along with the BaPS/¹⁵N pool dilution should be the main focus of the manuscript.

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