Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-9-RC1, 2018

Interactive comment on "Regulation of N_2O emissions from acid organic soil drained for agriculture: Effects of land use and season" *by* Arezoo Taghizadeh-Toosi et al.

Anonymous Referee #1

Received and published: 8 May 2018

Taghizadeh-Toosi and others study N2O emissions from a raised bog in northern Denmark. The study was performed competently but there are many highly speculative statements and the authors seem to continuously want to extend inference beyond what the data allow. Re-writing the paper to emphasize findings versus more speculative concepts that can be addressed in future studies would represent an improvement. Minor comments follow:

14: why is the soil "potentially" acid sulfate? (see also 81).

Lots of speculative statements in the abstract. "probably competition from plants for available N", "iron sulfides were probably the source", "appear to be important controls".

Printer-friendly version

Discussion paper

These statements need to be supported or not made at all.

The Introduction is well-written, but would be improved if the “high emissions” on line 68 were described quantitatively, and a hypothesis shouldn’t say “possibly” as this is less straightforward to falsify.

103: the soil diffusion probes should be described in more detail rather than merely referring the reader to Petersen, 2014.

Why could precipitation not be measured at the site? If it wasn’t measured, that’s ok, but I can’t think of a technical reason why it wouldn’t be able to be measured.

Are fertilization rates typical for the land management practices? And why were measurements only made during morning hours? Is there a diurnal pattern in N2O flux that may be missed as a consequence?

Just spell out weight on line 181.

On 234, was time of day an important factor? Or perhaps better yet temperature? I see these being included later but why not in the GLMM’s?

On 252 how are the properties optimal?

315 why is water table depth not shown? This will help the interpretation.

Try to avoid superlatives like ‘dramatic’ on line 340. Also, qualitative statements like ‘low’ on 374 and elsewhere are difficult to interpret and need to be removed or made quantitative.

The passage ‘spring of 2000-6000 ug N2O m-2 h-1’ is confusing on line 371.

‘were. . .occurring’ on 372 needs to be re-worded.

The argument on 435 about grass and competition with available N needs to be revisited. This is likely the case but you can’t definitively say it here, only note that it is consistent with the notion (and the Schothorst 1977 reference seems to me to be a bit of a stretch to use in its justification). Perhaps the competition is an important course of future study and that results point toward it. Likewise the statement on 459 is highly speculative about the minearalization of N in potato crop residues. Grasslands also have lots of residues from deceased grass.

The major finding of the graphical model on 468 is that N2O diffuses if concentration at depth is the most important factor. This is not a novel finding.

'Rainfall most likely triggered' could be tested using the dataset.

The passage on 519 is entirely speculative. The (speculative) section 4.5 is well-written, but it seems like the authors want to push their findings past the ability of the data to make them. This needs to be reconciled. AOA activity is a hypothesis for future studies, not to argue for given other studies including those from deep-sea water columns!

Figure 1 is not informative. Please make a real map.

Many if not all figures would benefit from readable font sizes.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-9, 2018.