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Biogeosciences Discussions

Interactive comment on "Nitrous oxide emissions from a peatbog after thirteen years of experimental nitrogen deposition" *by* Sarah R. Leeson et al.

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1 Referee 1 - Tim Moore

We thank the referee for their thorough reading of the manuscript. We address their points *(shown in italics)* below.

A large data set of N2O chamber fluxes (the exact number is not stated ...

The total number is now stated.

Specific comments: The study is devoid of specific mechanisms for N2O pro-

duction and emission, being concentrated (quite reasonably) on the relationships between treatments, environmental drivers and observed flux. The general argument, as I read it, is as long as vegetation is there, it will take up the deposited N, resulting in no significant emission. Given the work done at Whim, perhaps this could be fleshed out a bit. What is the annual N uptake rate at Whim? This might be cacluated from the C budget (about which quite a bit is known) and some assumptions of C:N ratio. An unknown is N2 fixation, as well as fluvial N losses, though you have DOC export and most N will be in the organic form. Given your addition rates of up to 60 kg/ha/yr of NH4 or NO3 and fluvial N losses (perhaps 3 kg/ha/yr, more if the elevated solution N forms get leached out), can these be accounted for in vegetation uptake (given your vegetation data) or peat storage? Can you add anything more to the Sheppard et al. (2013) Figure 7, based on data in 2009/10, whereas your results are based on fluxes through 2015?

We can expand on this a little, but we err on the side of caution, as many of the terms listed are only poorly estimated. Unfortunately, we don't have data to update the N budget shown in Sheppard et al. (2013) Figure 7. Also, estimating fluxes from the change in stocks of N is very prone to errors in bulk density, C & N concentrations and sampling error. The analysis presented in Sheppard et al. (2013) Figure 7 implies that N accumulation in the peat and vegetation has been considerably larger than the known N addition. This is implausible, and we think must be due to sampling error, so we have to be cautious in drawing conclusions from this data.

The sources of N2O remain a black box, a story unto itself. We need to get the N into N2O through either nitrification of denitrification (competing against plant uptake). Does the peat have a high nitrification and denitrification potential? Given the low pH, I suspect that nitrification of NH4 to NO3 will be slow, and it may be that natural rates of denitrification are also slow. But does addition of NO3 speed up denitrification rates, or has the soil pH been raised by NH3 to stimulate the microbial population? At

the Mer Bleue peatland we examined denitrification rates which were small naturally but when we added NO3 and a labile carbon source, there was substantial N2O production. We also observed no significant N2O emission from fertilized plots, with up to 64 kg N (as NH4NO3)/ha/yr. Perhaps there are no data to draw upon, but it would be worthwhile commenting on how these microbial processes may explain your observed result.

We agree this is an interesting topic, which we discuss only cursorily because of lack of data; unfortunately, we don't have any measurements of nitrification and denitrification rates or potentials. However, we now add some further discussion of how these underlying microbial processes may explain our results.

Finally, it is interesting that a substantial proportion of the N2O flux measurements suggested a consumption, though many had errors which overlapped zero. A few years ago, Chapuis-Lardy et al. (2007) drew attention that the process may occur but scientists had dismissed it as error. Since then, there has been some examination of the possibility of N2O consumption (essentially denitrification to N2) and under what conditions. Our work (Frasier et al. 2010) suggested that N2O consumption can occur, but mainly under anoxic conditions with a large N2O pool and very little NO3. Although I realise it is not part of your remit for this paper, it would be interesting to know under what conditions N2O consumption occured. Chapuis-Lardy L, Wrage N, Metay A, Chotte JL, Bernoux M. 2007. Soils, a sink for N2O? A review. Global Change Biol 13:1ï£¡17. Frasier, R., S. Ullah and T.R. Moore 2010. Nitrous oxide consumption potentials of well-drained forest soils of southern Quebec, Canada. Geomicrobiology 27: 53-60.

We did look for a pattern explaining N_2O consumption in the data, but there is nothing very clear. The main problem is that the negative fluxes are so small compared to the measurement error, we can't be sure they are real. We now make

reference to the Chapuis-Lardy et al. (2007) and Frasier et al. (2010) work, but we think it is pushing the limit of our observations to say much more about this.

Technical comments: I felt that the manuscript could have been clearer if some aspects were better described and more careful proof-reading had been done. I have annotated the pdf with comments and suggestions to address this.

We thank the referee for the very careful proof-reading, and have made all the changes suggested.

2 Referee 2 - D. Li

Response to comments by Referee 2

This manuscript presents N2O emissions from a peatbog following 13 years of simulated wet or dry N deposition. Compared to most studies in which very high doses of N were applied, this study adopted much mild N does. The estimation or prediction of N2O emission is a challenge largely due to notoriously high spatial and temporal variation and complex controlling factors as well. By providing long-term responses of N2O fluxes to mild dry and wet N deposition, the dataset of the manuscript is undoubtedly important and interesting. The manuscript is generally well written and the methodology is fine. However, I doubt whether the manuscript provided enough novelty relative to its companion paper, i.e., Sheppard et al. (2013). The main results of both papers are similar, or the same, i.e., N2O emission was stimulated by ammonia but not by ammonium or nitrate. The previous study covered a period of eight years of N addition, but this manuscript reported the results over another five

years. Whether this difference supports a new publication in Biogeosciences needs to be well addressed. Similarly, the three objectives were mostly covered in the previous paper. So they should not be used as the main objectives.

Our paper is a very substantial advance on Sheppard et al. (2013). The focus of that paper was on change in the vegetation cover and the fate of the added N. As regards N2O fluxes, there was only a single bar chart, only two sentences in the Results section, and no appropriate statistical analysis. Those data constitutes only 13 % of the data set analysed in our paper. We present N2O fluxes over the full range of the NH3 transect, whereas Sheppard et al.(2013) had only a single location. We apply a sophisticated mixed-effect statistical model, which accounts for the hierarchical structure of the data (chambers nested within plots within blocks, repeated measurements over time).

Specific comments: Statistical analysis Page 4, Lines 14-16: How did you judge that the four points were outlying measurements?

Visually, these points were clearly out-lying. Any formal test identifies these points as outliers. We can include the test results in the revision.

Results *Please present only the results or description of data in the result section and exclude any discussion.*

We have moved some of the text as suggested.

Page 5, Lines 16-18: Did you exclude the measurements which were close to the detect limits of the technique? If not, there should be large uncertainty in the data since most of measurements were close to the detect limits.

No, we did not exclude these measurements. The uncertainty is shown explic-

itly in Fig 3 in the form of the 95 % CIs in each measurement.

Discussion
Page 6, Line 9: The reference should be cited as Sheppard et al. (2013).

This has now been corrected.

Page 6, Line 14: NH4+NO3 is misused as NH4+NO3. In addition, there are lots of similar misuses, such as NO3- as NO3, especially in the figure titles.

These have now been corrected.

Page 7, Line 2: but both have limited capacities for uptake of what?

Changed to “both have limited capacities for uptake of nitrate”.

Page 7, Line 4: Data in (Sheppard et al., 2013, Figure 7)? This is wrong in edit.

This has now been corrected.

Page 7, Line 5: What do you mean by saying belowground vegetation? Roots? I can’t see that there is such information in Figure 7 in terms of all the additional N deposited on the wet treatment plots accumulated in the top 10 cm of peat and belowground vegetation. In the method section, N accumulation in vegetation was not presented. In addition, I checked the article (i.e., Sheppard et al., 2013), it seemed that there also was no such information.

What we actually meant was the data that were used in Sheppard et al., 2013

Figure 7, rather than the Figure itself. Summing the two classes which they refer to as "peat" and "vegetation", and comparing with the control, we can estimate how much N has been immobilised. We have now expanded the text to make this clearer.

Page 7, Line 5: If most of the added N was accumulated in the top 10 cm of peat, there should be substantial N2O production. Is there evidence showing that no N2O production in the peat layer?

The N is accumulated in organic form, and therefore not readily available to microbes, hence we say it is immobilised. Beyond the data presented in the paper, it is not clear what other evidence for N2O production the referee refers to.

Figure 3: This figure is about the responses to different forms of N inputs. It is confusing that 1) all the panels showed dry and wet N deposition, 2) when ammonium and nitrate were applied, the rates should be 16, 32 and 64 kg N ha-1 yr-1, 3) Dotted lines show the emission predicted with the IPCC default emission factor, but where is the dotted line?

We feel this figure is appropriate to the structure of the data. 1) all the panels show dry and wet N deposition because all the plots receive both dry and wet N deposition, albeit that some of this is ambient deposition. 2) total ambient deposition is 8 kg N ha-1 yr-1, made up of approximately 4 kg N ha-1 yr-1 ammonium and 4 kg N ha-1 yr-1 nitrate. If 56 kg N ha-1 yr-1 nitrate is applied, the total nitrate-N deposition is estimated to be 60 kg N ha-1 yr-1. 3) The dotted lines are clearly visible, but may have been mistaken for the x axis. The caption now makews this clear.

Figure 4: The figure should be stand alone, so please provide the necessary explanation.

Some additional text is now added to the caption to make this clearer.

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