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Discussions

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

T. Moore (Referee)

tim.moore@mcgill.ca

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This manuscript is a collation and analysis of a large number of N2O fluxes over several years at an experimental peatland where N has been applied, as NH3, NH4 or NO3, in a system designed to simulate the increases in atmospheric N deposition encountered in parts of Europe. N2O flux is notoriously difficult to measure reliably, because of high spatial and temporal varaibility and its small magnitude, which relates back to the complex processes of nitrification and denitrification which occur in soils. The paper examines the observed fluxes of N2O from the three treatment types, in terms of their magnitude (in particular to the 1% IPCC factor) as well as environmental/ecological controls. The primary conclusions are that that N2O fluxes are small (with individual values mainly encompassing zero) from the NH4 and NO3 treatments, but under high levels of NH3 deposition, larger fluxes are observed in the footprint from the NH3

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source. This is ascribed primarily to the detrimental effect of high NH3 deposition on the peatland vegetation, resulting in a reduced capture of added NH3 and, presumably through nitrification and then denitrification, the emission of N2O.

A large data set of N2O chamber fluxes (the exact number is not stated, but it must be several thousand....) was assembled along with ancillary environmental information, and subjected to statistical analysis to determine what is all means. In effect, this provides further support and a more detailed anaysis to the pattern detected earlier by Sheppard et al. (2013). The overall conclusion is that increased addition of NH4 and NO3 (even to such high levels as 60 kg/ha/yr) are unlikely to stimulate increased N2O emission, whereas small ‘hotspots’ of NH3 depsosition (I guess around manure piles) may result in up to 10% of the added N being lost as N2O. Quite how significant these hotspots would be remains unknown, and are probably small, being restricted to very specific locations and small footprints.

Specific comments: The study is devoid of specific mechanisms for N2O production 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 unkown 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?

The sources of N_2O remain a 'black box', a story unto itself. We need to get the N into N_2O 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 NH_4 to NO_3 will be slow, and it may be that natural rates of denitrification are also slow. But does addition of NO_3 speed up denitrification rates, or has the soil pH been raised by NH_3 to stimulate the microbial population? At the Mer Bleue peatland we examined denitrification rates which were small naturally but when we added NO_3 and a labile carbon source, there was substantial N_2O production. We also observed no significant N_2O emission from fertilized plots, with up to 64 kg N (as NH_4NO_3)/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.

Finally, it is interesting that a substantial proportion of the N_2O 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 N_2O consumption (essentially denitrification to N_2) and under what conditions. Our work (Frasier et al. 2010) suggested that N_2O consumption can occur, but mainly under anoxic conditions with a large N_2O pool and very little NO_3. Although I realise it is not part of your remit for this paper, it would be interesting to know under what conditions N_2O consumption occured. Chapuis-Lardy L, Wrage N, Metay A, Chotte JL, Bernoux M. 2007. Soils, a sink for N_2O? A review. Global Change Biol 13:1–17. Frasier, R., S. Ullah and T.R. Moore 2010. Nitrous oxide consumption potentials of well-drained forest soils of southern Québec, Canada. Geomicrobiology 27: 53-60.

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.

Tim Moore

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-70/bg-2016-70-RC1-supplement.pdf

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