

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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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 variability 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 it all means. In effect, this provides further support and a more detailed analysis 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 deposition (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 calculated 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?

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The sources of N₂O remain a 'black box', a story unto itself. We need to get the N into N₂O through either nitrification and 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.

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 occurred. Chapuis-Lardy L, Wrage N, Metay A, Chotte JL, Bernoux M. 2007. Soils, a sink for N₂O? 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

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-70/bg-2016-70-RC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-70, 2016.

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