

Interactive comment on “The relationship between ammonia emissions from a poultry farm and soil NO and N₂O fluxes from a downwind source” by U. Skiba et al.

U. Skiba et al.

Received and published: 21 December 2005

Response to referee 1

1) 'Only two N₂O chambers were used': N₂O is spatially very variable, and we should have installed three rather than two chambers per site, especially as fluxes were only measured monthly. However one consolation is that the same trend of decreasing N₂O emissions with decreasing NH₃ concentration and same magnitude of the fluxes was observed from the same forest in 1995 (5 dates) and 1997 (3 dates) (Skiba et al, 1998), when the same chambers, this time three replicates/site and the same measurement strategy was employed. The discussion section has been amended accordingly.

2) 'Emission factors' The referee will be aware that the emission factor for volatile emissions from indirect N application is based on very few data sets. The comment on un-

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

certainty suggests a 50% uncertainty, there appears to be just as much uncertainty in the certainty of the uncertainty as in the estimate itself. For this reason any calculation and presentation of EF's is valuable, even if uncertain. I have changed the calculation of EF's from a % of N emitted from N deposited wet and dry to a % of the wet deposited N only in order to address referee 2's concern on the calculation of dry N deposition rates. I have changed the final paragraph to:

'On average the NO emissions expressed as a fraction of the elevated N deposited as throughfall were 39 % (at 15 m), 30 % (at 25 m) and 17 % (at 45 m) downwind of the farm, whereas for N₂O the emissions were 3.5 % (at 15 m), 9.8 % (at 25 m), 9.5 % (at 45 m) and 4.1% (at 270 m) downwind of the farm. Due to high spatial and temporal variability the uncertainty in these emission factors is high, 50 % for NO and 80 % for N₂O. However these emission fractions exceed the emission factor of 1% (± 50%) advised by the IPCC for N₂O emissions resulting from atmospheric N deposition (Houghton et al., 2001) for some of the sites'.

3) 'Soil extractable NH₄ and NO₃' Referee 1 correctly pointed out that soil extractable NH₄ and NO₃ concentration measurements are only available on one occasion. These data (average and stdev) are merely observed in the result section without any further interpretation in the discussion section, this is hardly a case of 'over interpretation'. We have discussed the relationship between N deposition and soil extractable NH₄ in greater detail and have published such relationships for 2 poultry and 1 pig farm in the past (Skiba et al, 1998) and more recently established a linear relationship between these parameters for a range of mineral soils in GB (Skiba et al, 2004). I have changed the appropriate sentence (line 15 in the discussion section) accordingly: 'The side effect, however, is that the enhanced N deposition CAN increase THE SOIL MINERAL N CONTENT (SKIBA ET AL., 2004) and consequently the emission of the atmospheric pollutants N₂O and NO and CAN increase the risk of loss or change in biodiversity'.

4) 'NO and soil temperature' Referee 1 has questioned the mention of activation energies: The Arrhenius plots were drawn to establish the significance of the relationship

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

between NO and soil temperature. The reason for quoting activation energies is now given in the discussion section:

'The much larger activation energy calculated for the relationship between soil temperature and NO flux at site C, suggests that conditions were less optimal for NO to be produced and emitted compared to the closer sites receiving larger rates of N deposition (Skiba et al, 1998)'.

Skiba, U., Hargreaves, K.J., Smith, K.A. and Fowler, D. 1992. Fluxes of nitric and nitrous oxides from agricultural soils in a cool temperate climates, Atmos. Environ. 26A, 2477-2488.

5) 'Merger with Pilegaard's synthesis paper and length of paper' Why does referee 1 not like short papers? Our message comes across and more words would only dilute this message. A merger with Pilegaards paper would be wrong, as the two papers operate at very difficult scales. We had discussed this issue within NOFRETETE, but felt that it would be best to present fluxes from forests along a European deposition gradient, and those in very close vicinity of a large N deposition point source in two separate papers. Lots of the more detailed data presented here would be lost in the synthesis paper. Not reporting our inconclusive results on N₂O would be wrong, too. We should never hide and ignore 'bad' results. The observation of reasonable fits of NO fluxes with environmental variables, but not such relationships between N₂O and environmental variables is a very important observation.

Interactive comment on Biogeosciences Discussions, 2, 977, 2005.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper