

***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.**

Anonymous Referee #2

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General Comments This is an interesting and original experiment aimed at quantifying the effect of atmospheric deposition on rates of NO and N₂O emission by forest soil downwind of a large NH₃ source. One important objective was to derive emission factors (EF) using data from a horizontal gradient, but I feel that the uncertainties in both deposition and emission rates are probably too high to confidently derive EF.

Specific Comments The highest uncertainty in atmospheric deposition lies in the dry deposition term, yet the authors provide too few details on how they estimated the cumulative dry dep. flux. Did they use a resistance model, or simply a constant V_d? Did they measure atmospheric turbulent transfer rates? The "concentration-dependent" canopy resistance scheme of Fowler et al. (1998) is a somewhat speculative modelling concept and must be treated as such. The authors write in the results section that "a

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two-fold difference between NH_3 (dry) deposition and the rate of NH_4^+ deposition was observed", yet this is no observation, merely a model estimate. Likewise, the opening line of the discussion asserts that "We have shown that point source emissions of NH_3 significantly increased the rate of NH_3 deposition". Strictly speaking, only NH_4^+ (wet) deposition and throughfall were shown (measured) to be enhanced close to the poultry farm; the step from NH_3 concentrations to dry deposition flux remains conjecture. The linear ($R > 90\%$) correlation of NH_4^+ in throughfall with the modelled NH_3 dry deposition flux is no proof that the magnitude of the modelled dry flux is correct. Perhaps the authors should explore how their estimated EFs would vary if for example the dry deposition model estimate was biased by 20 or 50%, either upwards or downwards, which are realistic error ranges for this inferential approach.

Cumulative or annual emission fluxes N_2O in this study are also inevitably fairly uncertain. For N_2O the measurement frequency was once monthly, but it is not clear whether several samples were taken in each chamber on each measurement day. The role of the automatic chamber, located between site A and B, is likewise not clear: did the authors use the information from the daily flux time series from the autochamber to gap-fill the measured fluxes at the 4 other locations, using appropriate scaling factors? In addition, I wonder if cumulative fluxes might not be biased upwards if measurements are systematically made at midday, given the temperature-driven diurnal variations in emissions. Did the authors use the autochamber to resolve diurnal variations in N_2O fluxes? - this also is not clear from the text. Spatial variability is admittedly extremely difficult to assess, but at least temporal variability should be discussed since the authors did use an automatic chamber. If the continuous flux dataset from the autochamber points to a normal distribution, then it may be argued that random sampling once a month does provide a representative sample of the population. If on the other hand the distribution is log-normal then monthly sampling is more likely to provide a biased cumulative estimate.

For NO the dataset is more extensive than for N_2O , and the detailed diurnal patterns

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such as shown in Fig. 5 do justify further analyses of correlations with soil temperature and moisture. The authors do acknowledge that the paucity of N₂O flux data may be responsible for the apparent lack of correlations, and perhaps the interpretation of N₂O flux data should be much reduced to concentrate on NO.

From a general viewpoint, this experiment provided an interesting framework for qualitatively assessing the influence of a point NH₃ source on NO/N₂O emissions, and provided fluxes were measured at the same time on the same days at all 4 sites A, B, C and D, I have no objection to a comparison of mean measured fluxes between sites. From this perspective, empirical relationships such as shown in Fig. 4 between NO flux and NH₃ concentration are informative and do not require further assumptions regarding NH₃ deposition. However, the quantitative analysis of cumulative fluxes and emission factors is fairly ambitious (especially for N₂O). The discussion should better reflect the high degree of uncertainty involved, precisely because at such a shelter belt site most of the deposited N is contributed by NH₃ dry deposition, which was not measured *stricto sensu*.

Technical Corrections

Title: should be clearer. The expression “from a downwind source” is confusing since there are two types of sources, the poultry farm for NH₃, and the forest soil for NO/N₂O. In addition, either “ammonia” should be written as “NH₃”, or “NO and N₂O” as “nitric oxide and nitrous oxide”, to be mutually consistent. Suggestion: “The relationship between NH₃ emissions from a poultry farm and soil NO and N₂O fluxes at a downwind forest site”

Abstract, 3rd line: “can exceed 40 kg N ha⁻¹ y⁻¹”

Introduction, 5th line: “contribute 60% of”

Site description: it may be a good idea perhaps to provide a local map of the site indicating chamber locations and farm buildings together with prevailing wind direction.

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Methods, p981, 2nd paragraph, 7th line: “ the combined pump VACCUUM”

Results, p983, last paragraph, 4th line: “Average NO FLUXES for the entire study”
p984, 9th line: the unit of SWC is likely NOT g g⁻¹ dry weight, more likely % v/v p 984,
2nd paragraph, 3rd line: “The oscillation AMPLITUDE was much larger”

Fig.1 Caption: “Concentration of ATMOSPHERIC NH₃ (a) ”

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