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Interactive comment on "The relationship between ammonia emissions from a poultry farm and soil NO and N_2O fluxes from a downwind source" *by* U. Skiba et al.

F. Meixner (Editor)

meixner@mpch-mainz.mpg.de

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The authors ask referee #1 (in context with referee's #1 comment no.5), why he does not like short papers. The editor feels, that the referee's concern is not with the length of the paper, it is rather with the substance of the paper which referee #1 judges to be not sufficient to merit separate publication.

More generally, yes - conventional journals like short papers (for obvious reasons), but as soon as shortness of a paper comes along with the lack of clarity and/or comprehensibility, then a recommendation like that of referee #1 becomes serious.

Additionally, the editor likes to put further questions / comments on the manuscript bgd-2005-0045.

(1) Have the authors reflected the problem of the considerable underestimation of static chamber derived fluxes when using (shallow?) chambers and long closing times (1 h) as described by Rayment (2000) ?

Rayment, M. B.: Closed chamber systems underestimate soil CO efflux, Europ. J. Soil Sci, 51, 107-110, 2000.

Since the authors reference their static chamber method to Kitzler et al. (2005), they may also consider the corresponding editor's comments to that manuscript (Biogeosciences Discussions, 2, S1381-S1422, 2005) and to the companion paper of Kitzler et al. (Biogeosciences Discussions, 2, S1423-S1455, 2005).

(2) Can the authors guarantee (a) complete mixing of their dynamic chambers, and (b) negligible pressure deficit between ambient and chamber atmospheres?

ad (a): The "steady state" inside dynamic chambers depends on flow rate and volume, but particularly on the turbulent (complete) mixing of the chamber air (see Ludwig 1994). In this context, it should be mentioned, that the fact to install two small fans in the dynamic chamber is by far not a self-evident proof of (complete) turbulent mixing. Using ozone as a test agent and a saturated potassium iodide solution as a perfect ozone absorber (see Galbally and Roy 1980), complete turbulent mixing in a dynamic chamber can be experimentally demonstrated (see Ludwig 1994, Meixner et al. 1997).

ad (b): the pressure difference (dynamic chamber - ambient air) is most critical for under- and overestimation of dynamic chamber derived fluxes. Pressure deficits already in the range of a few tenth of Pa to a few Pa have generally been observed to cause flux overestimation in the order of tens of percent. For further explanation, the authors may have an intensive look (at least) to the following publications:

Reichman, R., Rolston, D.E. (2002) Design and performance of a dynamic gas flux chamber, J. Environ. Qual. 31:1774-1781

Davidson, E.A., Savage, K., Verchot, L.V., Navarro, R. (2002) Minimizing artifacts and

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biases in chamber-based measurements of soil respiration, Agricultural and Forest Meteorology 113:21-37.

(3) "Cumulative monthly concentrations of NH3 and NO2 were measured by passive diffusion. Triplicate alpha samplers for NH3 and diffusion tubes for NO2 were installed at a height of 1.5 m at all sites and were analysed by conductivity within two weeks of collection (Tang et al., 2001)" (page 980, line 14-18 of manuscript bgd-2005- 0045).

While there is convincing evidence in Tang et al (2001) that passive samplers correctly capture NH3 concentrations, there is nothing equivalent for NO2 :

"Work is now currently in progress for NO2 sampling", see page 526 in Tang et al, 2001), and "On the other hand, negative bias reported for longer exposure times can lead to underestimation of NO2 concentration, and more work is required to address the issue of sample stability", see page page 526 in Tang et al, 2001).

As far as the capability of passive samplers to correctly capture ambient NO2 concentrations is concerned, the editor likes to cite again Tang et al (2001): "For NO2 sampling, positive bias also arises from the reaction of NO with O3 within the sampler. The interference from the chemical reaction is severe close to NO sources, with errors up to 30% for curbside locations when using the 'tubetype' sampler. .... In some implementations, there is also a negative bias over long sampling periods caused by the degradation of trapped NO2." (page 513 in Tang et al, 2001).

Therefore, there exist, specifically for NO2, unavoidable positive (and negative) artifacts for NO2 measurements using passive diffusion tubes (e.g. due to "in-tube" chemical reactions (NO+O3) & effects of exposure time). The error might be in the order of tens of percent, if enhanced ambient NO concentrations (several ppb) might be present (like close to the forest floor). There is no information in the present manuscript addressing these problems.

However, as stated by Tang et al (2001) in their conclusions: "Passive diffusion sam-

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plers can be used successfully to monitor NO2 and NH3 concentrations, provided that the methods used have been rigorously tested, validated, and, where necessary, calibrated against recognised reference methods." (page 526 in Tang et al, 2001)

Since the authors must have measured ambient NO2 concentrations (part of the dynamic chamber system), a comparison of NO2 concentrations (diffusion tube vs. chemiluminescence analyzer) will definitely help in this direction.

Furthermore, NO once being emitted from the forest soil is rather rapidly converted to NO2 by ozone (turbulent transport from aloft). The conversion can easily reach 100% particularly in the first few meters above the forest floor (e.g. Rummel et al., 2002). So, it is a rather fair assumption, that part of the NO2 concentration captured by the passive samplers is converted biogenic NO. The authors are kindly asked to consider this fact.

(4) in the context of (3), the editor wonders, why the authors have not compared dynamic chamber NO2 deposition rates (which can be easily be inferred from their dynamic chamber measurements, see Butterbach-Bahl et al., 1997) with those obtained by the passive sampler/fixed deposition velocity approach ("... deposition velocity of 1.5mms-1 was applied for NO2 (Duyzer, pers. comm.)", see page 982 of manuscript bgd-2005-0045)?

(5) for the estimation of dry deposition NO2 fluxes, the authors make use of a constant deposition velocity of 1.5mms-1. Considering the rather complex interaction of in-canopy turbulent transport with vegetation uptake processes and chemical reaction of the NO-NO2-O3 triad (e.g. Meixner et al, 2003) this is a very crude (and not state-of-the-art) approach. By the way, what deposition velocities have been used for NH3 ? The editor feels, that any reader would most likely welcome a bit more precise information in this direction than the present statement, namely "Rates of NH3 deposition to the forest floor were calculated using concentration dependent deposition velocities for NH3 as described by Fowler et al. (1998)".

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References :

Galbally, I.E. and Roy, C.R. (1980). Destruction of ozone at the earth's surface.Quart. J. Roy. Meteor. Soc., 106, 599-620.

Ludwig J (1994) Untersuchungen zum Austausch von Stickoxiden zwischen Biosphaere und Atmosphaere. Ph.D thesis, University of Bayreuth, Germany (see also Butterbach-Bahl et al. (1997), Fluxes of NO and N2O from temperate forest soils: impact of forest type, N deposition and of liming on the NO and N2O emissions, Nutrient Cycling in Agroecosystems 48: 79-90)

Meixner et al. (1997), Preliminary results on nitric oxide emission from a southern African savanna ecosystem, Nutrient Cycling in Agroecosystems, 48, 123-138.

Meixner, F.X., Andreae, M.O., van Dijk, S.M., Gut, U.A., Rummel, U.K., Scheibe, M., Welling, M. (2003), Biosphere-atmosphere exchange of reactive trace gases in a primary rainforest ecosystem : studies on interlinking scales, Report Series in Aerosol Science, 62A, 269-274.

Rummel, U., Ammann, C., Gut, A., Meixner, F.X., Andreae, M.O. (2002), Eddy covariance measurements of nitric oxide flux within an Amazonian rain forest, Journal of Geophysical Research, 107 (D20), 8050, doi:10.1029/2001JD000520

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