Biogeosciences Discussions, 2, S912–S916, 2005
www.biogeosciences.net/bgd/2/S912/
European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Nitrogen oxides emission from two beech forests subjected to different nitrogen loads" *by* B. Kitzler et al.

B. Kitzler et al.

Received and published: 25 January 2006

Biogeosciences Discussions, 2, S652-S654, 2005

Interactive comment on "Nitrogen oxides emission from two beech forests subjected to different nitrogen loads" by B. Kitzler et al.

We are grateful for the comments of the reviewer and did our best to answer the questions. For easy reading we underlined the questions and suggestions of the reviewers and answered directly beneath.

Response to the Anonymous Referee #1 General comments:

The paper reports continuous measurements over two climatically contrasting years of NO and N2O emission from the soils of two beech forests receiving different amounts of nitrogen from the atmosphere. The paper thus reports a very large amount of data. The seasonal variations in the emissions were found to be closely related to the soil

temperature and short-time events of high emission could be related to rainfall and sudden changes in soil moisture. It has thus been demonstrated that in order to provide a reliable annual estimate of nitrogen oxides emissions, continuous long-term measurements are needed. It was also demonstrated that nitrogen deposition had a strong impact on nitrogen oxides emissions. The use of an advanced time series model (GARCH) has improved the reliability of predictions and has shown that some parameters have a lag in their effects on nitrogen oxides emissions. The paper provides a lot of useful information and is innovative in the use of the GARCH model. It is definitely within the scope of Biogeosciences and certainly merits publication.

Specific comments:

p. 1386, l. 19: I miss some more information about the (dynamic) chambers used for NO emission measurements such as size and flow rate (it seems to be 1 l/min given on page 1387). The sentence “The chambers were closed for 5 min when steady state was reached” is not immediately understandable. The chambers have to be closed for some time before steady state is reached. This time depends on the flow rate and the volume of the chamber. A measurement should then be made for some time during the period of steady state.

We included now more information on the methodology of NO emission measurements.

P:1386; l: 16:

“The detection limit of the NOx-analyzer (HORIBA APNA-360) was 1 ppbv NO or 0.6 μg NO-N m-2 h-1. The calculated error was high for fluxes near the detection limit (˜130%), whereas it was low for high NO fluxes (˜3%). A median error was calculated to be (˜15%) and (˜35%) for NO and NO2, respectively. Air samples were taken from six stainless steel chambers (Area: 0.03 m2; Vol: 3.27l; flow rate: 1l min-1) connected to the NOx-analyzer via PTFE tubing (inner diameter: 4mm; length: 10m). The closing (Plexiglas lid) of the dynamic chambers is initiated by a motor. One of the six dynamic

BGD

2, S912–S916, 2005

chambers was used as a reference chamber by sealing the opening to the soil through a Plexiglas pane. The chambers were closed for 5min within which steady state was reached."

p. 1386, l. 20: I think it is possible to discuss in some more detail, the difference between using synthetic air for the NO flux chambers (year 1) and correcting for O3 (year 2).

We inserted in section 2.3.

"Considering the basically bi-directional nature of NO exchange, the operation of dynamic chambers under "zero-gas" conditions (NO free air) can lead to an overestimation of NO fluxes due to exposing the enclosed soil to low NO concentrations (Ludwig et al. 2001). As the NO fluxes measured from our chambers are very low and NO concentrations are under calculated compensation points of 50 ppbv (SW) and 24 ppbv (KL) (at 15°C) determined in a laboratory experiment the error by using "zero-gas" was neglible in our case."

p. 1394: N2O emissions: I miss some information (and discussion) about the difference in N2O fluxes measured by the automatic system (AGPS) and the manual boxes. Measurements with the automatic system were made at 6 a.m. (where the flux is supposed to be largest). At what time of the day were the other measurements made (presumably sometime during mid-day or early afternoon)? What were the differences between the fluxes measured by the two systems? Were the annual estimates based on manual chambers or automatic chambers? Theoretically the AGPS should give a higher flux due to its ability to measure short-term high flux events after rain. In addition, the measurements were made at 6 a.m. where the emission was normally found to be highest (although soil temperature is probably lowest at this time).

Measurements with the AGPS were taken at 6 am (during summer) and at 1 pm (in spring and autumn). We changed the time in spring and autumn to avoid freezing of the lid in the morning. By taking several diurnal measurements, where samples were taken

BGD
2, S912–S916, 2005

EGU

every 2 hours, we could see that between 4:00 and 10:00am and again in the afternoon emissions were higher. But even here a high variability was observed and it couldn't be generalized that highest emissions occur at 6 am. Manual samples were taken in the forenoon (around 10:00) in Schottenwald and in the afternoon in Klausenleopoldsdorf where emissions were supposed to be similar to those in the morning.

The annual emissions at page 1399 l 15, 17 are based on mean emissions calculated from the mean flux of manual and automatic chambers. For the GARCH models we also calculated annual estimates from manual chambers (Table 4). In contrast to theory, AGPS gave slightly lower fluxes indicating that temporal variability affects annual estimates more than spatial variability.

p.1395, l. 28: Was a similar lag found for parameters controlling N2O emission? If not, I would like some discussion of the possible differences.

We also found the same lag for N2O emissions from the manual chambers (we inserted this lag now in the manuscript) but only at SW where we measured in a biweekly interval. As in KL measurements were carried out once a month we couldn't detect a lag at this site as time between the samplings was too long.

p. 1402, l. 19: Some extra information is given here about the spatial variations. Very little is given under results. I would like to see some more details about this. A statement is also given in the Conclusions (p. 1403, l. 11). Which results have lead to the statement about the necessary scale of the measurements?

NO emissions:

We inserted an extra statement about the variation of NO fluxes between the chambers, although with the standard error it can be already seen that a higher variation was detected in the second year. "In this year the variation of NO fluxes between the chambers was higher compared to the fluxes measured in the first year."

To avoid confusion, we also changed the sentence at page 1402, l. 19:

BGD

2, S912–S916, 2005

Interactive Comment

“Simulation results were at their best for the year 2002, when variations between the chambers were small due to continuously high soil moisture. As a result of soil desiccation in summer 2003 variation was higher and the fit between simulated and measured values was lower.”

N2O emissions:

The statement in the conclusion (p. 1403, l. 11) regards previously published measurements Meger (1997), Hahn et al. (2000) and Zechmeister-Boltenstern et al. (2002) where N2O emissions were significantly higher. We compared our results with results from emission rates from these studies which were conducted at the same site after movement the chambers of 100m. Due to different soil moisture conditions between the plots and possibly different light conditions, as nearby a windbreak occurred, spatial variation can be high.

We inserted in the discussion:

“Landscape-scale patterns of N2O production/emission are controlled by topography through its influence on fundamental hydrologic and pedologic processes as reported previously by Corre, (1996).“

p. 1402, l. 21: The statement about measurements in the soil under each chamber is a little bit strange. It would probably make the fit between model and observations better, but it would not be useful for true predictions, since these would be based on a much less dense set of measurements.

We agree with the referee and deleted this statement.

Interactive comment on Biogeosciences Discussions, 2, 1381, 2005.