Biogeosciences Discuss., 3, S594–S598, 2006
www.biogeosciences-discuss.net/3/S594/2006/

Biogeosciences Discussions

Interactive comment on "Nitrogen load and forest type determine the soil emission of nitrogen oxides (NO and N_2O)" *by* K. Pilegaard et al.

K. Pilegaard et al.

Received and published: 11 October 2006

We thank the referee for the very constructive comments. They are certainly helpful to improve our paper.

General comments:

1) "The approach used here is typical for this type of project synthesis. In this case, it was done in spite of the fact that it is well documented, especially for N_2O, that functional relationships between single factors and emission rates are lost in multisite data analysis across regions (the authors refer to the paper by Groffman et al., 2000). So why try again?"

We are aware of the findings of Groffman et al. However, regarding the relationships with single factors, we find that the dataset presented in our paper constitutes a unique

opportunity to investigate the statement by Groffman et al. The reviewer hypothesize that it is "very unlikely to produce significant relations using data from different sites". We believe such a statement needs to be tested in order to verify to which extent cumulated responses to e.g. N-deposition and different moisture regimes influence annual N-gas emissions.

We can provide several other reasons why we are re-doing the analysis of the Groffman et al. paper:
a) this is the most comprehensive dataset for forests in Europe so far established
b) the measurements were made within the same period, i.e. comparable in view of "general" meteorological conditions
c) temporal coverage is high
d) we did not only investigate N_2O but also NO (the Groffman paper is only dealing with N_2O and not with NO, this is a new aspect).

That we finally come to the same conclusion as Groffman et al. is something one needs to acknowledge.

2) “The authors should have focused only on those parameters, which determine the potential for N_2O emissions”

The paper deals with both NO and N_2O and since cycling of both gases are influenced by the same environmental parameters, we found it valuable to analyse the same sets of parameters for both compounds.

3) “Why use emission rates in Fig.2 and the annual emissions on Fig. 5?”

We used annual emission (sum of NO and N_2O) in Fig.5 to facilitate the comparison with annual input.

4) “The differences between the two forest types is suggested to be related to differences in N-inputs, whereas N-inputs were almost equal”

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The average N-input of the two forest types is very similar. The statement of difference between forest types is based on the locations where we have both coniferous and deciduous forest sites (Höglwald, Glencorse and Matra Mountains) and the statistical relationships for the two forest types and N-input.

5) "Figure 3 clearly shows that the C/N ratio dominates the N_2O emission potential, and other factors are - at best - of minor importance"

The forest soil C/N ratio is an integrative response to more environmental parameters among which long term N-inputs are important. Thus, N-deposition supposedly influence annual N_2O emissions at a secondary, distal level, an observation supported by the comparison compiled in the paper.

6) The reviewer suggests to combine Results and Discussion. We find that a short Results section gives a clearer view of the actual findings.

7) The reviewer suggests to remove some of the very general points in the discussion. Since this paper also is meant as an overview of the findings in the NOFRETETE project we have found it valuable to provide an extensive discussion to put our findings in perspective.

8) The reviewer suggests to focus on the most important and innovative parts of the story. In the revised paper we will highlight the new findings from this paper more clearly.

Specific comments:

Title: The reviewer points out that the title is not in accordance with the findings.
We have attempted to give a conclusive title, but agree with the reviewer. We therefore suggest a new title: *Factors controlling regional differences in soil emission of nitrogen oxides (NO and N_2O)*.

"Line 76: What results?"
This refers to the paper by Butterbach et al. 1997. A reference to this paper will be

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included here in the revised version.

"Line 101: Mean rates are not annual emissions. It should also be clear how individual measurements from chamber sampling were aggregated to yield annual rates."
We change the wording in the revised paper to "annual mean emission rates". The annual means were obtained from the full datasets which measurement frequencies are documented in Tables 3 and 4. The annual means are results of the individually measured emission rates taking the time period they represent into account.

"Line 137: Was there any crosscalibration performed to make sure that data from different sites are in fact comparable?"
We did not make an intercomparison of all chambers at one site. Such an intercomparison is extremely difficult unless it is made under exactly the same soil emission conditions. However, all the chamber designs used in the study have been validated in previous studies as documented in the references given in Tables 2 and 3. Other intercomparison studies of chambers of similar designs have shown that the variability between chambers are generally low, provided the normal design criteria has been taken into account (see e.g. Pumpanen et al. 2001 (full reference given in the paper's list of references).

Within the NOFRETETE project several intercomparisons were made:
a) intercomparison between chamber and TDL measurements at the Sorø beech site (Pihlatie et al., Biogeosciences, 2, 377-387, 2005).
b) intercomparison at the Matra Mountains site between the chambers used there and the chambers used in Höglwald (Rosenkranz et al., Plant Soil 286, 301-322, 2006)
c) intercomparisons between semi-automated and manual chambers (Kitzler et al., Biogeosciences, 3, 293-310, 2006, and Kitzler et al., Biogeosciences, 3, 383-395, 2006).

In general it has been found that differences between chamber types are low as compared to flux differences coming from microsite variabilities (which can partly be over-

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come by using larger chambers).

“Line 195: Which ratio?”
This refers to the ratio between the amount of N-oxides emitted and the total N deposition as mentioned in the previous sentence. We will clarify this in the revised manuscript.

“Fig. 2: All points should be identified with names.“
We omitted some of the names in this figure in order not to clutter the figure too much. We will, however, provide a fully annotated figure in the revised version of the paper.

”Fig. 3: In my view, it would be more sound to use two separate linear regressions.“
This is a good suggestion. We can easily provide a linear regression for the sites with C/N $<$ 20. However, there are only three sites with C/N $>$ 20, which makes a linear regression quite unreliable. We provided the log-transformed regression, because other authors (e.g. Klemedtsson, 2005) have reported the relationship as curvilinear regressions. It is well known from the literature that N_2O emissions quite often can be described by a log-normal distribution. The log-normal transforms are therefore maintained. In the revised version of the paper we will keep the log-transformed regression in Table 6, but remove it from Fig. 3, and add linear regressions as suggested to both the table and figure.

”Fig.5: Could be removed.“
We agree that the information given in this figure can be deduced from Fig.2 and Table 5. We included it to facilitate the comparison of nitrogen oxides emission with nitrogen deposition.

Interactive comment on Biogeosciences Discuss., 3, 837, 2006.