

Interactive comment on “Nitrous oxide emissions from a beech forest floor measured by eddy covariance and soil enclosure techniques” by M. Pihlatie et al.

M. Pihlatie et al.

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Comments to Paul Leahy

We thank Paul Leahy for valuable comments to improve our manuscript. We have addressed each of the comment below. To separate referee comments from the responses the text of the referee is written inside quotation marks.

– General Comments

“However there is a large uncertainty associated with the EC measurement and I am not sure that the data gathered is sufficient to enable an accurate comparison between the chamber and EC measurements to be made. For example, in the text (p. 595 lines 8-9) it is stated that 85% of the EC flux originates within 60 m of the mast and 50% within 15 m. Therefore the region in which the chambers are situated, which lies

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between these limits, only contributes 35%. This combined with the fact that all the chambers lie within a single octant limits the validity of the comparison. The conclusions may need to be modified to reflect these issues.”

The authors agree with Paul Leahy about the fact that the area where the chambers located contributes less to the EC flux than the closest tens of meters to the EC mast. This reduced the comparability between the two techniques. However, we consider the area where the chambers located was visibly similar to the surrounding areas. Also, in a previous study Ambus et al. (2001) found no difference in the N₂O emissions between dry and wet areas of the forest. Hence, we can fairly confidently say that the ecosystem level variation in the N₂O emissions is small and the soil chambers represent reasonably well the whole ecosystem. We have modified the discussion and conclusions to better reflect this issue.

We have also modified the aims of the study according to the suggestions of the anonymous referee 2. In the present manuscript the main aim was to obtain information on the spatial and temporal variability in the N₂O emissions. With the current data a full comparison of the two techniques is not possible and the applicability of the EC trunk-space N₂O measurements cannot be fully tested.

Specific Comments

“The descriptions of the wind directions in the text (p. 592 line 9) and Figure 7 appear to be inconsistent. The text states that 2-4 $\mu\text{g N m}^{-2}\text{h}^{-1}$ was observed from the SE direction. However figure 7 shows 2-4 $\mu\text{g N m}^{-2}\text{h}^{-1}$ from the wind sector centred at 225. In Figure 1 the sector labelled 225 is located to the SW of the EC mast. p.583 lines 1-5.”

The inconsistency in the description of the wind directions was corrected.

“There is increasing evidence of some forest soils acting as N₂O sinks e.g. Goossens et al, Nutr. Cycl. Agroecosys., 2001. Perhaps this should be mentioned?”

Indeed, the forest soil in this study may have acted as a sink for N₂O at times. This possible source of variation in N₂O fluxes has been included in the discussion.

“p.584 line 6. I agree with anonymous referee 3 - are there any more recent examples of the use of EC in the trunk space? Is the requirement of a horizontally homogeneous upwind fetch satisfied in this case?”

The area surrounding the eddy covariance mast is homogeneous for approximately 100 meters to each direction. We consider that the requirements for horizontally homogeneous upwind fetch were satisfied in the study site.

“p.586 line 23. Do the particular characteristics of the below-canopy cospectra affect the choice of averaging period?”

Below a forest canopy eddies contributing to the flux are generally smaller than above the canopy and hence even shorter than 30-minute averaging periods could be used. However, as we conducted the measurement close to the detection limit of the system we used 30-min averaging period and averaged the calculated fluxes over one day periods to reduce the random noise in the data.

“p. 587 line 2. Can the 0.07 ms⁻¹ threshold be justified? It may not be necessary to show the data but an explanation would be desirable.”

The use of the criterion for the standard deviation of vertical wind speed was justified in more details in the present manuscript.

“p.587 line 10. Is the Lagrangian stochastic technique suitable for application in the trunk space, given the different nature of the flow from an "open" area?”

In footprint simulations we used the Lagrangian stochastic model by Thomson (1987) which assumes Gaussian turbulence statistics. Inside a forest canopy, however, turbulence is highly non-Gaussian. According to comparison by Rannik et al. (2003) between non-Gaussian and Gaussian parameterisations of the Flesch and Wilson (1992) model, non-Gaussian turbulence statistics tend to move footprint peak further away

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from the measurement point. Observation level in this comparison was 1.5 times forest canopy height.

“p.590 line 1. change to “The mean wind speed...””

Was changed as suggested.

“p.590 line 5. The higher contribution of small-scale eddies in the trunk space sounds reasonable, but are there any other sources which support this?”

The discussion on the spectra was omitted following the suggestions of anonymous referee 2. However, in a manuscript submitted to Boreal Environment Research by Launiainen et al., the spectra of wind components and scalars measured below a boreal pine forest canopy are presented and they show a similar behaviour than presented in Kaimal and Finnigan (1994, Atmospheric Boundary Layer Flows, Oxford University Press).

“p.590 line 20. Quantify these temporal variations here? How do they compare to temporal fluctuations in the EC measurements?”

The variability whether it was temporal or spatial was quantified with coefficients of variation where possible. These variations were compared between different techniques and discussed in the paper.

“p.590 line 27. The large contribution of the hot spot is worthy of note, but I think that excluding it is probably not justified due to the small number of measurement plots.”

The authors agree that excluding the hot spot chamber is not acceptable. However, discussion on its contribution to the emissions gives additional information on the nature of soil N₂O emissions.

“p.591 line 25. Quantify the weak dependency with (e.g.) a correlation coefficient.
p.592 line 5. Also provide a quantification of this dependency.”

Correlation coefficients were provided in the text for dependencies of soil mineral N vs.

N₂O emissions and soil moisture vs. N₂O emissions.

“p.595 line 24. What is the spatial coefficient of variation of the manual chamber measurements? This would support the conclusion.”

The spatial coefficient of variation of the manual chambers was included in the results section and also discussed later in the discussion.

“Figure 1. What is the height of the buildings? Are they likely to significantly perturb the flow?”

The height of the buildings is approximately 2.5 meters. Since they are located approximately 30 meters away from the eddy covariance mast, we did not consider them to disturb the flow at least when they were not up-wind from the EC mast.

“Figure 3. Shorten caption to "Nitrous oxide emissions measured by eddy covariance (EC) and chamber techniques...". What is nEC, the number of EC averaging periods?”

The caption shortened and nEC explained in the figure caption.

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