Atmos. Chem. Phys. Discuss., 8, S11629–S11633, 2009
www.atmos-chem-phys-discuss.net/8/S11629/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "BVOC ecosystem flux measurements at a high latitude wetland site" *by* T. Holst et al.

Anonymous Referee #2

Received and published: 13 March 2009

GENERAL COMMENTS

The authors present a 50-day dataset of continuous BVOC concentration and fluxes obtained at a high-latitude wetland site. I am quite enthusiastic about the unique and interesting flux dataset presented in this manuscript and the data evaluation seems to be of high quality. The results fully match the scope of the journal and deserve publication. However, I am somewhat disappointed about the presentation and discussion of the results that are lenghty and not well structured. From a scientific point of view, I generally do not like it when Results and Discussion are mixed, but I can still accept it as long as the contents are arranged in a clear systematic way. Especially in Sections 3.4, this is not really the case. I thus suggest the following modifications:

(1) The presentation and discussion of winddirections and footprints (Sect. 3.1) is not of any use here as long as no vegetation map or other spatial information about the surroundings of the measurement site are shown (or if the surrounding area is homogeneous). So this part should be either omitted or completed with a map showing the vegetation pattern around the measurement site.

(2) It makes not much sense to me to discuss the BVOC concentration (especially for isoprene and methanol) before the respective fluxes are presented. Obviously there is a very strong relation between the observed concentration and fluxes of isoprene (maybe also methanol?) that should be discussed. For this purpose I also suggest to combine Figs. 6, 7, and 8 to one stacked figure. This would also facilitate the comparison of the seasonal changes in the fluxes and the environmental parameters.

(3) In section 3.4, the description of seasonal and diurnal flux variation, analysis of dependencies on environmental parameters (T, T48, PAR, and H) and fitting of emission algorithms are strongly mixed. I suggest to re-arrange Section 3.4 into the following two sub-sections: 1. Seasonal and diurnal variation of BVOC fluxes, descriptive analysis (with Figs.8 and 11a-d) 2. Dependence of BVOC fluxes on environmental parameters (with Figs.9, 10, 11e/f, 12) For the second part, it is important to clearly distinguish and compare analyses of the full dataset and of selected sub-periods. I do not understand, why a standard emission factor (isoprene) for the entire period was determined but not for the sub-periods, which obviously represent different development stages of the vegetation. It would also be helpful to mark the selected sub-periods in Fig. 6/7/8.

(4) The site description in Section 2.1 should be improved. Some important information is missing or introduced only in the discussion (which is not adequate). - wet, semi-wet and dry areas are mentioned but it is not shown of what size and in what distance to the measurement location they are (cf. comment 1). - typical or average values of canopy height and LAI should be given - describe development of vegetation during the measurement period (growth,senescence)

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SPECIFIC COMMENTS

p.34 line 15: Explain what "palsas" are.

p.36 line 11: I suggest to move the important information about the measurement period to the beginning of the Section.

p.37 line 10: Is the noise level at the analogue output of the PTR-MS similar or worse compared to the digital output?

p.38 line 4: What fraction of data had to be rejected due to this criterion?

p.39 line 1: The meaning of this sentence is not clear. ("...60% of the measured fluxes were emitted..."?).

p.39 line 3: In the text, the footprint calculation is refered to "Wilson and Swaters (1991)", but in Fig. 2 to "Schuepp et al. (1990)". Which one is right? Why was the footprint calculation based on a quite old method and not a newer one? As far as I remember, the simple model by Schuepp et al. does only consider neutral conditions.

p.40 line 10f. "larger eddies" and "lower frequency range" is very unspecific here. Give typical limits.

p.40 line 14f. When comparing the damping effect with literature results, it has to be considered that it strongly depends on the measurement height and the windspeed.

p.40 Section 3.2.2: The effect of reduced sample number in the DEC approach had been (theoretically) assessed already by Lenschow et al. (1994, "How long is long enough...", J.Atmos.Oceanic Techn.,11,661-673). It would be interesting to compare the results to this original work.

p.40 line 25f. Why are the effects for w'T' and std(w)/u* mixed here? I doubt if the analysis of std(w)/u* (this is not a flux!) can be considered as representative for the behaviour of scalar BVOC fluxes. I suggest to show instead the results for w'T' in Fig. 5!

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p.41 line 17-20: The quantitative contributions to the damping effect strongly depend on the instrumental setup (e.g. measurement height, tube dimensions and flow rate) and might deviate from the cited studies. At least the physical tube damping effect (by mixing in the tube flow) could be easily estimated for the present setup using theoretical transfer functions e.g. by Lenschow and Raupach (JGR, 1991).

p.42 line 18: Did the authors also look for other BVOC compounds at the site beside the mentioned 4 species (e.g. monoterpenes)? If yes, it would be useful for the community also to report on masses/compounds not found at the site.

p.43 line 12f. While I see the advantage of using carbon mass related units for BVOC fluxes, I do not see it for concentrations.

p.43 line 14: replace "comparably" by a more specific expression.

p.45 line 26-29: The discussion of the relationship between methanol fluxes (deposition) and sensible heat flux is scattered in different pieces (additionally occuring on p.48 line 11f. and on p.49 line 20f.). These pieces should be combined to one focussed discussion paragraph (see also general comments above).

p.46 line 16f.: Since the temperature response function CT1 is an important issue in the present study, its definition (formula of original and modified emission algorithm) should be shown here.

p.46 line 26: How exactly is the standard emission factor "Is" defined when the dependence on T48 is included. Obviously this new standard emission factor is not comparable to the original one. I also doubt whether the dependence on T48 is a mechanistic effect or a spurious correlation here. Obviously there was a development (senescence) of the vegetation during the observation period in parallel to the generally decreasing average temperatures. In my view it would be elucidating to see how the common standard emission factor (according to Guenther et al.) varies with time when fitted to different sub-periods (e.g. the four sub-periods described in the text).

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p.47 line 14: rephrase to "The estimate for Is (at 20°C) obtained using the Guenther et al. algorithm..."

p.47 line 14-28: I suggest to compile the various values for the standard emission factors (both for 20°C and 30°C) in a Table. Listing the values for both standard temperature would also satisfy the short comment by J.Rinne.

p.48 line 2: "...the correlation with these ..."

p.48 line 3-5: I do not understand the meaning of this sentence. Isoprene emission is also strongly correlated with assimilation whithout being limited by stomatal conductance!

p.49 line 7: explain this statement in more detail. Presumably the light distribution in the canopy depends on the LAI profile of the vegetation, for which no information is given here.

p.49 line 20/21: What is the possible reason (and potential use) of this linear relationship. Please discuss.

p.50 line 18: "...rather than to ..."

p.50 line 21: specify: "BVOC emission responses that had been found already by ..."

Fig. 4: It is not clear, for which quantity the spectral analysis was calculated here (is it the sensible heat flux?) Explain the meaning of std(u) in the y-axis title. Is this the horizontal windspeed like in the x-axis titel?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 21129, 2008.

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