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Biogeosciences Discussions

Interactive comment on "Methanol exchange between grassland and the atmosphere" *by* A. Brunner et al.

Anonymous Referee #1

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General Comments

The authors present and analyse an extensive dataset of methane fluxes measured over two grasslands with contrasting management and thus species composition. The paper significantly adds to the literature as it investigates exchange over a lesser studied ecosystem and uses a state-of-the-art micrometeorological technique. The paper is very straight forward, well laid out and easy to read with an authoritative introduction. I only have a few minor comments to improve this good manuscript further.

Specific Comments

P130, l12. From the information provided (tube diameter 3.5 mm, 4 lpm) I calculate a Reynolds number of 1600, which is more laminar than turbulent. This is an unfortu-

nate choice, given the tube length of 30 m and the low measurement height, and it is therefore surprising that flux losses were as low as 25 to 55%.

P131, l15. Why do the authors decide to repeat the same measurement value rather than match it up with the spot measurement of the associated vertical wind speed, using a disjunct sampling protocol? I assume that this may be so that the authors can use spectral analysis techniques. However, this approach induces significant further flux loss which may also have contributed to the estimated losses of 25 to 55%.

P132, last sentence. I am not sure I understand this sentence.

Section 2.3. I am surprised that the authors did not appear to have filtered the flux data according to fetch requirements, atmospheric stability and non-stationarity. Given the small extent of the fetch there are likely to be conditions at which most of the footprint is situated outside the field. It is clear that, since night-time fluxes are small, even large relative errors at night will not greatly influence the average flux. However, since the contribution of the field to the measured flux is presumably smallest when Rg is small, there may be a bias on the parameterisations. I therefore suggest, plotting less reliable fluxes in grey in Figs. 3 & 7 and excluding them from the plots used to derive parameterisations.

P133, l15. Could the authors please specify whether this is single-sided or double-sided LAI.

Fig. 4. It would be nice to see a polar plot of the concentration also. Is there evidence for contribution from nearby anthropogenic sources such as the near-by motorway?

Fig.5. Have the authors tried to explain the residual of the regressions between F(MeOH) and Rg (and F(H2O)) with another meteorological variable. Could temperature explain some of the variability? Or is growth stage the main effect apart from Rg/F(H2O)?

Normalisation by LAI. As with other parameters such as canopy resistances, one would

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not necessarily expect the emission to scale linearly with LAI, for example because of shading effects. In addition, F(H2O) will already, to some extent, include some of the LAI dependence, while Rg does not. While the authors first assess the fluxes after normalisation through deviding by LAI, they later decide to implement LAI in a different functional relationship (Eq. 3). This introduces some inconsistency into the argument. It would be much more straight-forward to plot either the ratio F(MeOH)/F(H2O) or, preferably, F(MeOH)/Rg against LAI to derive the functional relationship on LAI.

Rg vs F(MeOH). In my opinion, Rg would be the preferable scaler as it is (a) more readily available, e.g. when predicting MeOH fluxes in models, (b) is a more basic parameter and (c) F(H2O) also depends on soil water. The question depends to some extent on what is driving the MeOH flux. Does F(MeOH) respond primarily to stomatal conductance or is it associated with the water flux itself. Obviously, the water flux does not just depend on stomatal conductance, but also e.g. on surface temperature. The authors could make an attempt to estimate stomatal conductance from F(H2O) during dry periods in an attempt to learn more about the process of MeOH emission.

P138, l8. How homogeneous is the plant species composition of the extensive field. Could heterogeneity have contributed to the smaller correlation coefficient?

P140, l9. Why would the emission of a more soluble compound be more closely controlled by stomatal conductance? Intuitively I would have assumed the opposite? Also, the statement “The magnitude of daytime emissions also depends on the rate of methanol production within the leaves.” needs to be backed up by references or evidence.

P141, l14. Is this slower release consistent with the dynamic model?

Technical Comments and Spelling Mistakes

General: the authors should make sure that the font of the symbols in the text matches those of the equations. For example, c(t) in line 23 on page 131 is non-cursive, while

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in Eq. (1) it is cursive. Cursive symbols may be preferable?

P127, l8: add comma after “In addition,”.

P127, l12: specify more clearly “removal processes for methanol from the atmosphere are oxidation ...”

P128, l14. Either “during the summer of 2004.” Or “during summer 2004.”

P128, l18. “at the ecosystem scale”

P129, l2. I believe, the site is now also a Supersite of the new NitroEurope programme, which could be mentioned for completeness.

P134, l2. “highest methanol concentrations to coincide with ...”

P134, l22. Delete ‘respectively’, which is not correctly used here.

P134, l26. ditto

P139, l18. “the same order of magnitude”

Eq. (4). The authors need to define y0 = 0.00962 or they need to state the units in which F(MeOH) and F(H2O) should enter Eq. (4).

P143, l24. “methanol emission compares well with our results.”

P144, l12. “on a day-to-day basis, the diurnal”

P144, l14 “In the longer term ... of the extensive field remained relatively constant over the ...”

Caption to Fig. 1. “Left: Position of the monitor ...”

Fig. 11. The symbol of F(MeOH),cal is invisible in my copy of the figure legend.

Interactive comment on Biogeosciences Discuss., 4, 125, 2007.