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Biogeosciences Discussions

Interactive comment on "Turbulence characteristics in grassland canopies and implications for tracer transport" *by* E. Nemitz et al.

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General: The authors report on within-canopy turbulence and Radon measurements for a grassland from which they develop a number of turbulence statistics required for modelling mass and energy transfer between the grassland ecosystem and the atmosphere. While similar analysis are relatively abundant for tall (forest) canopies where within-canopy turbulence measurements are much easier, this is one of the few studies for short, dense canopies and this study thus fills an existing gap. The paper is well written, data and analysis appear sound to me, and the literature is discussed appropriately. The fact that one quarter of the figures is referred to in the discussion

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Interactive Discussion
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(and not the results) is a bit unusual, but justified as these are incorporated smoothly in the discussion. Accordingly, I have only a few minor comments, listed below, and suggest the manuscript be accepted for Biogeosciences with minor revisions.

Detailed comments: (1) p. 439, l. 19: would not 2d footprint models also require profiles of siqma w and Tl ? (2) p. 446, l. 10: explain g and Zi (3) p. 446, l. 15: G is often used as an acronym for the soil heat flux; in order to avoid confusion why not use H0 or something similar to indicate the soil surface sensible heat flux (4) p. 448, l. 3: which criteria were applied to assess whether the miniature sonic anemometer could still be used or not ? (5) p. 450, l. 21: the Massman & Weil (1999) model has several adjustable parameters – how were they chosen ? for a fair comparison with the empirical sigmoid functions one should think about optimising the free parameters against the data; also I wonder whether the raw LAD data (with a relatively coarse vertical resolution) were used as input for the model or whether a smooth function has been fit too the LAD data; how often was the LAD profile measured anyway during the growth of the canopy and after the cut ? (6) p. 451, l. 13-15: this should go to the discussion section (7) p. 452, l. 12: how was Ra(z) derived ? (8) p. 454, l. 11: this is also in contrast to Wohlfahrt & Cernusca (2002), who investigated a denser grassland canopy and found a secondary maximum in the lowermost quarter of the canopy (9) p. 454, l. 19: Massman & Weil (1999) model – again the issue with the adjustable parameters ?! (10) p. 457, l. 10: Leuning et al. (2000) investigated rice – this typo appears many times in the ms, also sometimes 1999 is quoted instead of 2000 (11) p. 458, l. 6: Massman & Weil (1999) (12) p. 461, l. 21: how much did u* vary among the 8 sonic anemometers ? (13) p. 462, l. 18: here it might be worth mentioning that Wohlfahrt (2004) found that the formulation of Tl does not affect the prediction of within-canopy scalar profiles and above-canopy fluxes a lot. (14) P. 463, l. 6: how much plant matter is there at the ground ? I would expect very little plant matter in a managed grassland, as the canopy is usually cut way before senescence and therefore little litter fall occurs (in contrast to an abandoned grassland) (15) Fig. 2: Leuning et al. (2000); in the text it is mentioned that the miniature sonic was not used below 0.15m –

BGD

6, S13–S15, 2009

here symbols seem to go very close to the soil surface – is this impression due to the normalisation of the vertical axis ? (16) Fig. 3: fewer x-axis ticks in panel (b) (17) Fig. 13: correct bugs in figure legend (18) Fig. 14: to which probability levels refer the dashed and dotted lines ? (19) Fig. 15: is there some particular meaning associated with the horizontal line in the temperature panel ?

Interactive comment on Biogeosciences Discuss., 6, 437, 2009.