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Biogeosciences Discussions

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

Anonymous Referee #4

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General Comments: This study presents a fairly detailed account of turbulence within a short grassland canopy. This area of study is often neglected due to the difficulties encountered in making high resolution turbulence measurements within small, dense canopies. However, the authors point out the importance of understanding this transport as many trace gas species have distinct and separate source and sink levels from the soil surface through even this rather short canopy. Furthermore, the transport time within a canopy also has important implications on chemical transformation that may occur within the canopy air space. Overall, this is a well-conceived study (using both sonic anemometry and hot film anemometry for comparison) and the conclusions presented are well-supported by the data analysis. The authors do need to recheck their

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reference list - some references are either missing or misquoted. I recommend this manuscript for publication in Biogeosciences with the following minor revisions:

Specific Comments:

(1) Page 439, lines 9-12. Although, the two studies noted only report a small effect of within-canopy chemistry on isoprene emission, a previous study by Makar et al. (Makar PA, et al., JGR-ATM. 1999, 104, 3581) predicted underestimate of isoprene emission rates of up to 40% chemical processing. (2) Page 439, lines 12-15. The review by Duhl et al (2008) does not really focus on the micrometeorological flux measurements of sesquiterpenes. A better reference to show the importance of reactive loss of these compounds during turbulent transport is: Ciccioli, et al., J. Geophys. Res., 1999, 104, 8077, who saw reduced above-canopy fluxes compared to those predicted from leaf-level measurements. (3) Page 443, first paragraph. Positioning even a miniature sonic anemometer within the grass canopy is difficult at best. The authors discuss how they excluded grass blades, etc., but there is no comments on the possibility of shadowing effects on the wind velocities by the anemometer structure itself. How do the authors know that this is not a significant problem? From comparisons with the hot wire anemometry? (4) Page 446, lines 20-23. Please define tau-L (Langrangian time scale?). (5) Page 447, Eq. 7. "T" in denominator is not defined (averaging time?). (6) Page 447, line 13. Italic "H" is already used for sensible heat flux. Pick a different symbol for hole size. (perhaps Huw?). Also, where is Si,H defined? Also, t1/2 in line 19 is not adequately defined and is presented later in a figure. (7) Page 449, lines 16-18. Were the soil conditions "constant"? For instance, was there significant rainfall during the course of the experiment which could affect the soil water content and porosity, thereby possibly affecting the Rn soil efflux? (8) Page 450, lines 5-10. Since this paragraph describes the physical setup; should this not be moved to the first paragraph in this section (where the measurement heights were previously given). (9) Methods section, general comment. The authors should briefly describe how soil heat flux was measured and the sign convention on this flux, as this is used at several points within

the following analysis and discussion. (10) Page 450, line 20 and Figure 2. There are no Leuning et al., 1999 or Raupach 1989b in the reference list. The Raupach 1989a reference in Figure 2 is also missing. (11) Page 451, lines 13- 14. Please note that it is the "effect of stability" that is more pronounced. Also, note that the enhancements in Launiainen et al. (2007) occurred within the canopy (as opposed to above). (12) Page 452, line 21. Is this above-canopy u*? (13) Page 452, line 20-21 and Figure 8. The periods examined have similar u*, but do they have similar above-canopy stability? It seems reasonable to expect that the two-parametric probability functions at a single height may depend on the stability as well (at least in broad terms: unstable vs. stable vs. neutral). Perhaps some measure of above-canopy stability should also be given in the Figure legends, since it is difficult to judge solely from the time periods given. (14) Page 453, line 19. This should be referring to Figure 12b (not 13b). I would also say that v does not really show significant positive skewness near the top of the canopy. It appears nearly Gaussian, especially in comparison with u. This is similar to other previous studies in larger canopies. (15) Page 453, the three numbered bullets, please denote the different canopy levels being described by giving a range of z/hc. (16) Page 454, line 1. Should this be bullet #4 (continued from the previous page)? Also, denote "bottom of the canopy" with z/hc $<$??. (17) Page 454, line 2. Incorrect Figure number. Is this Figure 8? (18) Page 454, line 7. H1/2 appears to decrease at the lowest level in Figure 12a, not increase. (19) Page 454, line 19-21. Massman and Weil (1999) (not 2000). Again, no Leuning et al., (1999) reference (this appears many times in the manuscript). (20) Page 455, line 1-2. Is the increase in TKE from the anemometer relative to the hot films an indication of shadowing of the transducers, or just exclusion of grass from anemometer sonic path as mentioned? (21) Page 45, section 4.2. There are several other studies of turbulence parameters through taller canopies which are relatively consistent with the current results. (for example: Lee and Black, 1993,which is already referenced, Amiro, 1990, Bound. Lay. Met., 51,99-121). (22) Page 456, line 1. 62% of what? The total measurement periods? (23) Page 457, line 26-27. tauL is not defined in Eq. (6) as variant with height as it is described here.

(24) Page 458, line 23-24. Is the x-axis in Figure 14 sigma w or (sigma w)ˆ2? (25) Page 459, line 15,tauL, not "TL". (26) Page 459, line 19. "... is larger..." (27) Page 461, general comment on within-canopy chemistry. Equally important to understanding how trace gases are moved from the surface through the canopy is the opposite process: how reactive species (or oxidants, such as ozone) are transported downward through the canopy. For example, ozone is taken up within the upper canopy. Combined with restricted transport further down into the canopy, this should lead to significantly lower oxidant concentrations available to drive chemical reactions. For the example of ozone-driven NO conversion to NO2, there may be significantly longer time deep within the canopy, but there may not be enough ozone to drive the reaction. This is something that has not been looked at in very much detail. (28) Page 461, line 23 to Page 462, line 3. Should this not be described in the Methods Section, (section 2.3)? (29) Page 462, line 23. "... measurements at the canopy scale..." (30) Page 463, line 6-9. The sentence beginning "If this is true.." is too long and contains too many differing ideas. Break into at least 2 smaller sentences. I would suggest breaking it after the phrase "... before the cut...". Then have a sentence of the two contrasting effects that could be occurring. (31) Page 463, second paragraph. I am not sure it is necessary to describe the failed attempt at NH3 profiles within the canopy. It could be mentioned in a single sentence combined within the next paragraph. (32) Figure 2, check references in the caption. (33) Figure 8, The isopleths on the probability distributions are exceedingly difficult to read. It would also be good to include w* on panel (e) to be consistent with panels (d) and (f). (34) Fig. 11 caption, this analyzes data from Fig. 8a-d, not 9a-d. (35) Figure 12. Why is a Gaussian fit used in panels (a), (c) and (d)? Is there some significance to this type of fit? If not, I would suggest removing it and let the reader discern the trends from the data. (36) Figure 13, Check references in the caption.

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