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Biogeosciences Discussions

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

E. Nemitz et al.

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We thank Georg Wohlfahrt and the three anonymous referees for their careful reading of the manuscript and the detailed constructive comments which have helped to improve the manuscript. In the following we answer the more critical points made by the referees one by one. The referees' original text is printed in italics.

Anonymous Referee #1

In addition to the already cited references, I found one older reference on turbulent diffusivities in a maize canopy that may be of interest to the authors and readers (Druilhet, A. (1970) Détermination de la diffusivité turbulente dans les premieres mètres au-dessus du sol a partir de la diffusion du thoron. In : Techniques d étude des facteurs *physiques de la biosphère, I.N.R.A. Publ. 70-4, Institute Nationale de la Recherche Agronomique, 149, rue de Grenelle, Paris-7^e). It is based on Rn-220 measurements. Profiles are S-shaped during the night, while they are similar to the shapes in Figure 13c at midday.*

We have added this citation and mentioned their results in the context of our study: "This non-linearity of K_f near the ground is further supported e.g. by K_f profiles in maize measured by Druilhet (1970), who derived s-shaped profiles during the night, from thoron measurements at eight heights."

G. Wohlfahrt (Referee)

The fact that one quarter of the figures is referred to in the discussion (and not the results) is a bit unusual, but justified as these are incorporated smoothly in the discussion.

We are aware that this may be seen as a little unconventional. The format originates from the fact that we felt that significant discussion of the initial rounds was needed to proceed to the higher level analysis of the results. We appreciate that Georg agreed that we have succeeded in making it read right. We have moved a few paragraphs up to the Methods section as suggested by another Referee (see below).

(1) p. 439, l. 19: would not 2d footprint models also require profiles of siqma w and Tl ?

The vertical profile of sigma w is needed wherever a vertical structure of the footprint within the canopy is calculated. Typically 2D footprint models refer to two horizontal dimensions, but, yes, if one of the dimensions were the vertical dimension, Georg would be right. We have changed the wording '3D footprint models' to 'vertically resolved footprint models'.

(2) p. 446, l. 10: explain g and Zi

A definition of g has been added and the (already existing) definition of zi has been moved to earlier in the text.

(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

We believe we follow the general convention here. G is normally used for the ground heat flux. We agree that G is also sometimes (erroneously) used for the soil heat flux at some depth in the soil.

(4) p. 448, l. 3: which criteria were applied to assess whether the miniature sonic anemometer could still be used or not?

Anemometer measurements were actually made down to a height of 5 cm, at which point the anemometer cage almost touches the ground. Lower heights are physically not possible. Radon measurements were made up to a height of 15 cm. We agree that the current sentence implies that Rn measurements started where the anemometer measurements stopped and we have clarified this in the revised manuscript.

(5) p. 450, l. 21: the Massman & Weil (1999) model has several adjustable parameter; 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?

We have added the model parameters used for this study to the text. We agree that the magnitude of sigma w predicted by the model depends on the parameters chosen have added a caveat at two places in the manuscript, pointing out that a choice of a smaller length scale would have led to better agreement. However, the increase of TL near the ground is model inherent and this is now discussed in the manuscript. Only

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the tall canopy is modelled here and this was already mature and its LAI did not change greatly during the study. While a smooth LAI function was not derived, this is expected to have had only minor impacts on the model results.

(6) p. 451, l. 13-15: this should go to the discussion section

This has now been moved.

(7) p. 452, l. 12: how was Ra(z) derived ?

The equation actually implied how Ra(z) was derived, i.e. by integration of 1/Kf. This has been clarified in the revised manuscript and a reference has been added: "The diffusive transport (t_d) from the ground to the main measurement height may be estimated from the aerodynamic resistance ($R_a(z)$), which can be identified by integration of the reciprocal $K_f(z)$ with height (e.g. Garland, 1977):"

(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

Many thanks for pointing out this reference we had previously missed and have now included in the discussion.

(9) p. 454, l. 19: Massman & Weil (1999) model; again the issue with the adjustable parameters ?!

See above.

(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

Corrected.

(11) p. 458, l. 6: Massman & Weil (1999)

Corrected here and elsewhere.

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(12) p. 461, l. 21: how much did u vary among the 8 sonic anemometers?*

The companion paper treats this in detail. The median relative standard deviation of 30-minute values was 13.8%, but the standard deviation of the campaign averages of the different instruments was as low as 0.7%, indicating that most variability was due to turbulence statistics rather than systematic differences between anemometers. Thus the average of 8 anemometers should provide a robust estimate. A brief comment has been added to the manuscript.

(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.

We have worked this reference into the revised manuscript at two alternative places: in the introduction, to point out that the formulation of TL is important in some, but not in all situations, and in the discussion as this paper also suggests that TL does not increase towards the ground.

(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)

There is always substantial leaf litter in agricultural grasslands and the role of the leaf litter in emitting NH3 was confirmed through chamber experiments and bioassay measurements of the leaf NH4+ content.

(15) Fig. 2: Leuning et al. (2000); in the text it is mentioned that the miniature sonic was not used below 0.15m; here symbols seem to go very close to the soil surface; is this impression due to the normalisation of the vertical axis?

0.15 m was the maximum height for the Rn measurements, not the minimum height for the anemometer measurements, see above.

(16) Fig. 3: fewer x-axis ticks in panel (b)

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Corrected.

(17) Fig. 13: correct bugs in figure legend

Done.

(18) Fig. 14: to which probability levels refer the dashed and dotted lines?

These are 95% confidence and prediction bands as now indicated in the caption.

(19) Fig. 15: is there some particular meaning associated with the horizontal line in the temperature panel?

No, not sure where this came from. Corrected.

Anonymous Referee #3

1. Strict meteorologists call L as Obukhov length and not as Monin-Obukhov length;

theory behind is Monin-Obukhov theory.

True, although Monin-Obukhov length is used more often than Obukhov length. This has been corrected.

2. Only windy conditions, u $>$ 0.2 m/s, was used in Fig.2; the friction velocity limit is certainly reasonable why exactly this value?*

We admit that the use of this cut-off is rather arbitrary. We feel it is rather conservative, based on the results of the intercomparison of the eight anemometers.

3. How as the soil heat flux G measured?

This is described in more detail in the companion paper of the same special issue. A brief comment has been added to this manuscript.

4. Reference Rinne et al: replace "Helln" by "Hellen"

This was correct in the submitted manuscript and must have gone wrong at the editorial

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phase. We will make sure this is correct in the proofs.

5. Fig. 4: explain sigma_w,n in the fig caption.

Explanation has been added.

6. Fig. 10: mention that the curves are 3rd polynomials, I guess so.

Yes, they are. Information has been added.

Anonymous Referee #4

The authors do need to recheck their reference list - some references are either missing or misquoted.

Yes, this was also pointed out by Georg above and has partially been amended in the response above. Further inconsistencies are addressed as raised by this Referee below.

(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.

This is true and worth pointing out.

(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.

Many thanks for the reviewer, we were looking for a better reference and have gratefully adopted this one.

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(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?

At 0.8 m canopy height, this grassland was actually quite tall, facilitating the anemometer measurements. Care was taken that the sonic anemometer was pointed into the prevailing wind to avoid sheltering from the mount and mast. The hotwire anemometer measurements require less space, average over a smaller volume and are potentially faster. The good agreement indicates, however, that the sonic anemometer was suitable to resolve the turbulence.

(4) Page 446, lines 20-23. Please define tau-L (Langrangian time scale?).

This symbol was already introduced in the introduction, but as it apparently adds readability we have re-introduced it as recommended by the Referee.

(5) Page 447, Eq. 7. "T" in denominator is not defined (averaging time?).

Yes, definition added.

(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.

The new Eq. (7) had disappeared during the editorial process and has been re-added. This defines Si,H. The introduction of t1/2 has been expanded and an example has been provided in the revised manuscript.

(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?

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The measured soil water content decreased gradually during the campaign from 15 to 11%, with only small precipitation events. In general this was a warm, dry period, without heavy precipitation events which would have caused significant changes in porosity.

(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).

We agree that this would be a more natural sequence and have moved and adjusted the paragraph accordingly.

(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.

Addressed in response to Georg's comment above, a sign definition has been added.

(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.

Many thanks for spotting the errors in the reference list. The Leuning reference has been corrected as pointed out by Georg above. The Raupach references have been added to the literature list.

(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).

The wording 'of stability' has been added to the sentence for further clarification. The citation of the work of Launiainen et al. referred to their in-canopy work, but this sentence has been moved in response to another Referee.

(12) Page 452, line 21. Is this above-canopy u?*

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Yes, although a u* could be defined for the ground surface, all values of u* used here relate to the u* above the canopy. This has been clarified in the definition of u* and again in the sentenced referred to by the Referee.

(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.*

L has now been added to the figure legend. As expected for windy periods (large u*), stability is near-neutral. During pre-review one of the reviewers actually suggested removing the current information from the legend. Thus it is nice to see that this Referee agrees that this kind of information is useful.

(14) Page 453, line 19. This should be referring to Figure 12b (not 13b).

That is absolutely correct and has been changed.

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.

Ok, we have changed the wording for v.

(15) Page 453, the three numbered bullets, please denote the different canopy levels being described by giving a range of z/hc.

Information added.

(16) Page 454, line 1. Should this be bullet #4 (continued from the previous page)?

Also, denote "bottom of the canopy" with z/hc $<$ *??.*

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We have turned this paragraph into a fourth bullet with height range as suggested by the Referee.

(17) Page 454, line 2. Incorrect Figure number. Is this Figure 8?

Yes, this should have read ‘Figure 8d’.

(18) Page 454, line 7. H1/2 appears to decrease at the lowest level in Figure 12a, not increase.

Following on from the description of the conditions at the middle of the canopy (previous bullet), it first increases and then decreases again towards the ground. This has been clarified in the revised manuscript.

(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).

Corrected in response to Georg’s comments above.

(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?

Since the open (no bars) sonic anemometer side tended to point into the wind, we think that the lack of grass made a larger impact.

(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).

A comment to this regard has been added to the manuscript, together with the proposed reference.

(22) Page 456, line 1. 62% of what? The total measurement periods?

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62% of the time, clarified.

(23) Page 457, line 26-27. tauL is not defined in Eq. (6) as variant with height as it is described here.

The height dependence has been added to Eq. 6 for consistency.

(24) Page 458, line 23-24. Is the x-axis in Figure 14 sigma w or (sigma w)ˆ2?

The figure is correct, the text has been adjusted to match the figure more closely.

(25) Page 459, line 15,tauL, not "TL".

Corrected.

(26) Page 459, line 19. "... is larger..."

Well spotted. Corrected.

(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.

This is a very good point and definitely worth mentioning in the manuscript. We have added some text to the motivation in the introduction.

(28) Page 461, line 23 to Page 462, line 3. Should this not be described in the Methods Section, (section 2.3)?

We have moved the technical aspects of this section (up to line 3 on Page 463) to the

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methods section, but have left the development of the argument here in the Discussion section.

(29) Page 462, line 23. "... measurements at the canopy scale..."

Corrected.

(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.

This has been simplified as suggested by the Referee.

(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.

We have decided to keep the short description of the failed measurements, as we were not sure how to describe these in a single sentence. However, this reads now more naturally as it has been moved to the methods section.

(32) Figure 2, check references in the caption.

Corrected, see also reply to Georg's comment.

(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).*

We have changed the lines used for the isopleths. w* is only defined for convective conditions and cannot be calculated for conditions where G (as defined here) was upwards, but have included it where possible and we have also added an explanation why it is not always reported to the figure caption.

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(34) Fig. 11 caption, this analyzes data from Fig. 8a-d, not 9a-d.

Corrected.

(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.

We felt that this type of fit was best representing the data, but there is no physical reason for this particular shape. However, the same applies to the fits provided in Figs. 2, 3, 9 and 10, to which this Referee does not object. We have therefore decided to retain the fit curves to guide the eye of the reader.

(36) Figure 13, Check references in the caption.

Corrected.

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