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Biogeosciences Discussions

Interactive comment on "Dependence of CO_2 advection patterns on wind direction on a gentle forested slope" *by* B. Heinesch et al.

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This discussion paper is actually from a group of people that is highly appreciated in the eddy flux community for their valuable contributions to the advancement of our understanding of advective fluxes in relation to turbulent flux measurements.

I however must admit that I would not accept this paper in its present form. I hope that I did not misinterpret the concept of the discussions papers, but as I understand these should be final papers like those submitted to other journals, not papers where the reviewers and participants of the discussion finalize the paper.

I think that there is a wealth of material available to the authors, but I am unhappy about how it is presented and the confusion it creates to readers such as myself, instead of

providing distilled knowledge to the reader.

There are also technical aspects which I as an Editor would take as a reason to ask the authors to revise the paper and resubmit:

> Figures are partially unfinished for publication Figures do partially not confirm to minimum scientific standards (e.g. axes are labeled across graph in Figs. 4, 7; there are gaps between bars in Fig. 5 where there must not be any gaps; French in place of English lettering in Fig. 2; units of 1/milliseconds instead of m/s in Figs. 4, 9; no indication is given what the error bars denote in Figs. 6 and 7; panels are not aligned with each other in Fig. 10 where the reader would expect equal horizontal distances for equal number of hours, and that these hours are vertically aligned)From Figure 6 one gets the impression that winter conditions with temperatures below freezing are presented; I have not found any such information in the Methods section and actually realized that I had been starting to read the whole results and discussion with the (wrong?) implicit assumptions that the authors present growing season data (but then θ would be well above 0°C).

Thus I stopped reading on page 4241 and file my comments below in hope that they will be helpful for the needed revisions. I will also my annotations in PDF format available to the authors.

General Suggestions

(1) First of all please sort out the issue about when the measurements were carried out. Page 4232, line 20 says “four months in summer 2002”, whereas Figure 6 suggests conditions clearly outside summer. In Switzerland, for example, you do not get negative temperatures in lowland forests before September or so. Recall the definition of θ – if you do not say for which pressure you computed it I assume it is 1000 hPa (standard definition of θ).

(2) Fig. 1: the clearing mentioned on page 4232 is not clearly shown. Moreover, the

BGD
4, S2148–S2151, 2007

EGU

typeface is so small in many figures that it cannot be read without magnifying glas in the print version.

(3) To me it appears that all the synoptic wind conditions that were selected for the analyses are actually more or less parallel to the height contours. That means, that there is an obvious turn in wind direction according to the Ekman spiral, and it would be neccessary to understand when the flow inside the canopy is actually representing the direction that is expected according to the larger-scale pressure differences (that also determine the Ekman spiral), and when there are small-scale pressure differences that are in another direction than the larger scale differences (which would make the interpretation more difficult, but could be relevant at your site)

(4) All the discussion about vertical winds (Fig. 4) could be strongly biased by the fact that you measure at an edge of a taller Douglas fir patch to a less tall beech patch. You do not mention the possibility of having a rotor behind the roughness change when the wind comes from the NE, but your Fig. 6 shows exactly the pattern that you might expect from a rotor when the wind is from the NE: mixing of warmer air above the canopy is driven by this rotor, which does not extend to the forest floor, and thus you see warmer temperatures at heights 5–30 m if wind is from the NE than SW, and in the lowest 5 m you see the reverse, because the surface is cooling, but the rotor does not extend down through the canopy. In the case of SW it appears to be a neutral stratification in the lowest 10 m, indicating a weak mixing due to wind in the lower trunc space, whereas the typcial stable profile only starts at abou 10–20 m.

Minor Comments

4232/13: give more details how you determined LAI; especially, since one sector is Douglas fir, the other beech, I would be surprised to see the same LAI (with one significant decimal!) for both directions!

4233: more details on total hose length, pump strength, excitation time before taking readings, length of period where measurements are taken and averaged (if they are)

BGD

4, S2148–S2151, 2007

would be very helpful for readers

Eq. (1) and (2) seem to be typographically incorrect with inner and outer integrals

4234/17–18: lateral homogeneity does not appear to be a justified assumption given Fig. 1b. . .

4236/15: unclear

4237/1ff: fit does not look good in Fig. 2 – maybe I am not looking at the correct points that should be approximated by the respective curve? I am concerned about the fact that the Beta function shows the maximum in $f(z)$ at 7.5 m, whereas the measurements appear to show such a maximum at 1–3 m. I would argue that the 15% difference is due to mismatch at lowest elevations, not a mismatch at higher elevations (where you probably also expect the vanishing of horizontal gradients).

4240/6: do you really mean “circulation”? If so, then please give a scientific description of the type of circulation. If it is wrong wording, do you mean “types of graviational flows”?

4240/21: I am uncertain about the use of the term “buoyancy”. In my dictionary I find “the ability or tendency to float in water or air or some other fluid”. Is this not used wrongly with stable stratification here? Buoyancy I would expect during unstable stratification.

Give units of variables on p. 4234, lines 3–5

If you wish you can add “Eugster & Etzold, pers. comm.” to the list on page 4231, lines 15–17, we have started to do so (Sophia Etzold presented first results in a poster during the CarboEurope meeting in Poland), and I strongly hoped to get good guidance for her how to analyse her data via this current BGD paper. . .

Interactive comment on Biogeosciences Discuss., 4, 4229, 2007.