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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.

B. Heinesch et al.

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Referee comment: 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 _ 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.

Reply: We thank the reviewer for the acknowledgment of our previous contributions to the understanding of advective fluxes. We hope to have solved all the presentation problems. The overall quality of the figures has been greatly improved. All comments made on this point have been taken into account.

Referee comment: (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  – if you do not say for which pressure you computed it I assume it is 1000 hPa (standard definition of ).

Reply: Measurements were conducted only during summer conditions as stated unambiguously in the section 2.2. Confusion arose due to figure 6 presenting vertical temperature profiles in terms of temperature difference relative to the top of the canopy

BGD

4, S2711–S2718, 2008

Interactive Comment

(Theta_z-Theta_h) without clearly stating it. It's now explicit in the legend and in Fig 6 that these are potential air temperature relative to 34m (the uppermost measurement available and approximately the top of the canopy). The potential temperature was computed following Stull 1988, page 8. It is a first order approximation using the dry adiabatic lapse rate. As this is common practice, this information has not been added to the text.

Referee comment: (2) Fig. 1: the clearing mentioned on page 4232 is not clearly shown. Moreover, the typeface is so small in many figures that it cannot be read without magnifying glas in the print version.

Reply: The quality of Fig 1b has been improved. It's now a stand-alone figure to avoid reduction of its size. Its position has been switched with the presentation of the experimental set-up.

Referee comment: (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 necessary 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)

Reply: First, the direction of the ambient wind is obviously determined by large scale pressure gradients at a regional scale and not by the topography at the local or the valley scale. These main wind directions (SW or NE) are simply the wind directions characteristic of the entire region. Second, we have good arguments to think that below-canopy flows in stable atmospheric conditions are mainly driven by the presence of the slope. (i) The alignment of the wind close to the ground with the slope direction is closely associated with increasing stability and decreasing net radiation.

BGD

4, S2711–S2718, 2008

Interactive Comment

(ii) If these trunk-space wind directions in stable conditions where strongly influenced by the directional wind shear, these wind direction would be very different for NE ambient winds compared to SW ambient winds. This is not the case, as in stable conditions, the below canopy wind direction is fairly well aligned with the slope regardless of the wind direction aloft. (iii) No obvious and systematic turn in wind direction when going down inside the canopy was observed in neutral or unstable conditions (see fig 2 in Aubinet et al., 2003, BLM), contrarily to what was found for some other forests. It means that the influence of directional shear induced by the Ekman spiral mechanism associated to a drag force due to the canopy is limited at our site. These arguments are now developed in section 2.4.

Referee comment: (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 trunk space, whereas the typcial stable profile only starts at abou 10–20 m.

Reply: The possibility of an impact of the land-cover heterogeneity on the flow pattern was already considered on P4241L22-26 "It is not completely clear, however, why these particular situations are associated with wind direction or what the cause is of the downward vertical movement in the NE sector. As local buoyancy associated with continuity equation can be invoked as described above, the impact of a pressure gradient due to land cover heterogeneity could also be invoked. Indeed, due to the smaller

BGD

4, S2711–S2718, 2008

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Interactive Discussion

Discussion Paper

tree height in the beech sub-plot than in the Douglas sub-plot, it is possible that streamlines above the canopy would be deflected downward when wind blows from the NE sector"). We have now evoke more clearly the possibility of a rotor behind the height transition Your interpretation of the temperature profile is hampered by the misinterpretation discussed on point 1 so we will not discuss it here (these are not temperature but temperature difference relative to the top of the canopy (Theta_z-Theta_h)). But anyway, we agree that some other mechanisms than acceleration of the slope flow can play a role in the creation of a downward air movement above the canopy. For example, w40m could be generated by a rotor and then influence the slope flow, reversing the causality link between the two variables. The main point here was to show the coherence between w, slope flow behaviour and horizontal CO2 gradient measurements close to the ground.

Minor comments:

Referee comment: 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!

Reply: The VAI (and not the LAI as mentioned erroneously in our text) was deduced from LAI2000 measurements (Aubinet et al., 2001, AFM) and from PAR measurements above and below the canopy (transect of eight sensors, Aubinet et al., 2002, AFM). It was found to be stable during the full leaf development season and similar between the two subplots (that does not mean of course that the structure of the canopies are similar). These precisions have been added to the text in section 2.1. No measurements of VAI were available during our advection experiment. That's why we relied on measurements made in previous years.

Referee comment: 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) would be very helpful for readers

Reply: The requested details about the experimental details of the gas multiplexer have been added.

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

Reply: The integrals were changed accordingly.

Referee comment: 4234/3-5: Give units of variables on p. 4234, lines 3-5

Reply: The units were added for the CO2 mixing ratio and the molar volume of dry air.

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

Reply: As shown in Fig 1b, the slope is quite uniform in a radius of 500m around the tower and the horizontal transect. Moreover, we work only on events when the sub-canopy flow is more or less aligned with the slope direction. Finally, we have shown previously (Heinesch et al., 2007, BLM) that the [CO2] evolution along a streamline was quite regular and reproducible from one streamline to another at the same height. With these arguments, we think it's reasonable to work with a 2D set-up. Of course, we would prefer a 3D set-up but you probably understand very well that this would require a huge amount of material.

Referee comment: 4236/15: unclear for a short period or for all possible events?

Reply: This was explicitly explained on 4237L2-5.

Referee comment: 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 gradi-

BGD

4, S2711–S2718, 2008

Interactive Comment

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Interactive Discussion

Discussion Paper

ents).

Reply: We agree on the general remark that the fit on the u vertical profile is not totally convincing (the possibility of a mismatch at lowest elevations is now evoked in the text). That was the reason for the sensitivity test on g of the choice of the profile function. On the other hand, a theoretical shape for the u profile on forested slope in conditions of gravitational flows is not available. To our knowledge, the only publication proposing such a modelling is Yi et al. 2005, JGR and requires detailed informations on the canopy structure which are not available in our study. We thus need to fit 3 measurements over the gravitational layer depth with an empirical function containing 3 fitting parameters. Introducing more sophistication here is senseless. The size of the points has been enlarged and French spelling has been corrected.

Referee comment: 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 gravitational flows"?

Reply: "Circulation" has been replaced by "gravitational flows".

Referee comment: 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.

Reply: Previous studies investigating slope flows commonly used the term "buoyancy" to describe the forcing that causes relatively colder air to flow downslope (Mahrt, Journal of Atmospheric Science , 1982; Staebler, AFM, 2005 among others). We thus propose to keep this terminology.

Referee comment: 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.

Reply: We would prefer not to cite a personal communication in the introduction part of my paper. The citation (Leuning et al. pers. comm.) is present there due to the similarity of the final results obtained concerning FVA which reinforce the discussion of my paper. The poster of Mrs Etzold at the Poznan CEIP meeting was presenting preliminary results and not yet the final product of your research in which we are, of course, highly interested.

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