Biogeosciences Discuss., 4, S2253–S2257, 2007
www.biogeosciences-discuss.net/4/S2253/2007/

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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Received and published: 27 December 2007

This paper is based on experimental material collected under conditions of gentle forested slope is an attempt to understand the effect of advection produced by gravitational flow on CO2 patterns. I admit the complicity of this goal, although the authors in their analysis have used simplified, unacceptable, from my point of view, assumptions that made their efforts absolutely senseless at least at this stage. The title of the paper corresponds well to the subject of analysis but does not, in my opinion, correspond to what has been measured in reality. I think that any effect of gentle slope on CO2 advection patterns is fully concealed by the mixture of different kinds of vegetation surrounding the experimental plot. Ignoring the tree stand composition in data analysis (authors have mentioned this effect in passing) and simultaneously emphasizing the slope effect as the main factor forming CO2 pattern results in failure that we can see in fig 10, for example. Another serious error in the analysis presented by authors is a misuse of the continuity equation and, as a consequence of Eqs. 1 and 2 (see major comments for details).

In my opinion the paper is not acceptable for publication in present form. However, taking into account that the paper contains potentially useful information for the eddy flux community I recommend reconsidering this paper for publication in BG after revision according to the following comments.

Major comments

i. As any paper based on experimental data and aimed at a solution of advection problems this paper should provide readers with more comprehensive and clear information about the site. It is important for both better understanding of real situation in consideration and further modelling efforts. Are LAI for both kinds of canopies similar and equal 5? Note that the reference "Laitat et al. 1999" is not easily accessible for readers. Fig. 1b is of the very low quality and should be redrawn. It would be also useful to indicate in the figure the position of the main tower relatively to two main sub-plots. "At interface" does not describe the real situation clearly enough. For clarity please indicate in Fig 1a the sector directions.

ii. As it was mentioned by W. Eugster (see his comment 4) there are serious problems in interpretation of the experimental data due to the presence of (at least) two different tree stands on the site. I am not sure whether the height difference of 9 m is able to produce any rotor, that W. Eugster speculated about, but the fact that the wind directions above and below canopy are different was not taken into account by the authors. Both theoretical consideration and experimental evidence show that the angle between these two directions could be larger than 30 degree (e.g. Smith et al. 1972, BLM; Kondo and Akashi, 1976, BLM). Thus, the question is how do wind velocity at the reference height of 1 m and vertical velocity above the canopy relate to each other. All discussions on page 4239 (lines 16-24) are very vague (for example, in fig. 4 I can see that for both sectors the functions are almost parallel. That contradicts to what the authors have said earlier in the paper. On the other hand the fact that the vertical velocities are negative along the flow from lower to higher vegetation (SW sector) indicates that there should be a number of factors causing such behaviour which were not considered by the authors - e.g. above mentioned wind direction turn, complex stand composition to name the few (probably the rotor as well)). Another question is whether it is reasonable to establish any relationships between these two characteristics (wind velocity at the reference height of 1 m and vertical velocity above the canopy) at all. I guess that if we have a decoupling of above and below canopy conditions due to air stability, which is actually the subject of the present study, the vertical velocity at the canopy height is a result of upper- and above-canopy flow pattern and not of below-canopy one. This leads to the next erroneous result in this paper (see iii).

iii. It is rather odd in my opinion that authors assuming the presence of only gravitational flow below canopy as a major factor transporting CO2, still consider the vertical transport throughout the canopy layer. If, according to their assumption there is no any transports accept the one below the canopy, then what is the reason for vertical velocity? Therefore, in estimations of FVA and FHA the authors should definitely integrate all terms in Eqs. 1 and 2 through the whole h-layer. The assumption that the horizontal gradient in CO2 concentration is small above canopy (that is why the authors limited integration by trunk space, see fig.2 for the product of f(z)g(z)) is definitely wrong because the vertical velocity provides considerable gradient of CO2 above and within the canopy (from fig. 10c I can see that FVA and FHA are in opposite phase as it must be). If the authors suppose that everywhere in their stand the situation is similar to that indicated in fig. 2, then the vertical velocity should be completely excluded from consideration (this situation is typical only for uniform conditions) and any other assumption will be wrong by definition. The assumption that CO2 sources are stronger upstream in the control volume (Page 4242, lines 7-9) would not be helpful as well.

BGD

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Interactive Comment

Minor comments

I fully agree with all remarks given by W. Eugster (see his minor comments) and have also several my own ones

1. Page 4230, line 24 Clarify please, what is the method you speak about? I assume that it is the Eddy Covariance method.

2. Pages 4231-4232 Describe Fig.1 in the text in the right order - a, b not b, a; or interchange the figures.

3. Page 4234, lines 18-19 - two "may be" is too many for two lines.

4. Page 4236 lines 6-10. The decoupling between above and below-canopy space is caused by stable atmospheric conditions and not by gravitational flow.

5. Page 4236, line 21. It is not superfluous to indicate units and values of adjustable parameters, or at least to tell a reader that he can find them in fig. 2.

6. Page 4237, line 1. At this point the reader has probably forgotten what the considered tree stand characteristics are. Put in fig. 2 some indication of canopy layer, please.

7. Page 4237, lines 8-9. It is not obvious that the Beta function "clearly" underestimates U at the top of the gravitational layer? Provide a wind profile in the figure to illustrate that. To avoid any uncertainties with the reference height used for normalization of profile function indicate its value directly in fig. 2 and make corresponding remark in its caption.

8. Fig 2. Symbols are not explained.

9. Fig. 6. I can only speculate that here the differences between temperatures and their values at the reference height, h, are presented. Please, clarify this in the caption.

10. Fig. 8. The direction of flow from the sheet (if I correctly understood that grey

colour indicates SW sector flow) should be indicated by circle with point inside, but not with the cross.

11. Fig. 9. It would be better to set the same position of 0 for both vertical axes. In the present state the figure gives an impression that delta CO_2 for SW sector is larger than for NE sector.

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