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Interactive comment on “Dependence of CO₂ advection patterns on wind direction on a gentle forested slope” by B. Heinesch et al.

B. Heinesch et al.

Received and published: 3 March 2008

Referee comment: 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

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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.

Reply: First, we would like to recall that, to our knowledge, there is no model available yet able to describe the advective fluxes under stable conditions in conditions of gravitational flows. This study relies on experimental data showing good coherence between above canopy w, slope flow behaviour and horizontal CO₂ gradient measurements close to the ground. We propose a mechanism that fits the observations and that make sense. This mechanism is probably oversimplified but is in qualitative agreement with a simple model developed in Aubinet et al. (2005), can be tested on other sites showing similar flow characteristics than Vielsalm and can be tested in future modelling of these situations. Given the complexity of the problem and the lack of modelling tools available for these particular atmospheric conditions, we think that this study can be useful in the understanding of advection events in presence of gravitational flows. The failure in the CO₂ balance shown in figure 10 is not necessarily an argument to reject the whole analysis. We agree that there are obviously quantitative problems in the evaluation of the required variables necessary for the computation of advection. And we decided to show this disappointing balance because we have arguments to isolate the most important source of error at our site and because this point is mainly eludated in previous publications computing the CO₂ balance including advective terms. Despite this quantitative problem, after careful uncertainty analysis (see Heinesch et al., 2007, BLM) on these measurements, we believe at least in the qualitative information obtained. Concerning a 8220;serious error 8230; misuse of the continuity equation8221;; we do not find additional information about a possible misuse of the continuity equation in the major comments so it is difficult for us to reply to this comment. If this remark is similar to that of Dr. Kowalski concerning the interpretation

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of the boundary-layer budget equation, please see our reply to this comment and note that there is no consequence on the computations and on the numerical results of our analysis.

Referee comment: 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.

Reply: The VAI (and not the LAI as mentioned erroneously in our initial 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. The quality of the figure 1 (map of the site) has been improved. The position of the main tower relatively to the two main subplots is now more clearly visible. The sector directions have been added on figure 1a (description of the transect).

Because different arguments are mixed in point ii., we will list them.

Referee comment: 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

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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.

Reply: 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. (ii) If these trunk-space wind directions in stable conditions were strongly influenced by the directional wind shear, these wind direction in the trunk-space 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: 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.

Reply: We recognize that there is a link between w_{40m} and $u_{3m,proj}$ even for the SW sector contrarily to what was said in the text. But in Fig. 4, the slope of the two curves are different (-0.11 against -0.26). The sentence 8220;8230; and does not depend on the velocity in the trunk space8221; has been changed in 8220;8230; and only weakly depends on the velocity in the trunk space8221;.

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Referee comment: 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).

Reply: In view of w and their error bars in the SW sector, we think that they cannot be considered as negative. We agree that there can be complex factors influencing the wind field. We have reinforced the discussion on a possible impact of a pressure gradient due to land cover heterogeneity in section 3.2. We do not pretend to be able to explain in detail what happens under gravitational flow events and to prove the causality relation between w_{40m} and $u_{3m,p}$ but the observations are there to show that there is a link between w , slope flow behaviour and horizontal CO_2 gradient measurements close to the ground. We propose a mechanism that fits the observations and that make sense. This mechanism can be tested on other sites showing similar flow characteristics than Vielsalm.

Referee comment: iii. It is rather odd in my opinion that authors assuming the presence of only gravitational flow below canopy as a major factor transporting CO_2 , 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 CO_2 concentration is small above canopy (that is why

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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.

Reply: When we stated that the gravitational flow below the canopy is a major factor contribution to CO₂ transport, this does not mean that FVA is negligible. Vertical velocities and vertical CO₂ gradients can be observed in presence of gravitational flows and lead to FVA. The CO₂ transport by the mean flow (advection) occurs in the horizontal as well as in the vertical direction. The limitation of the vertical integration of FHA to the trunk-space was argued in section 2.4. We do not understand why "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)". This assertion should be developed to be understandable. In addition, why do FVA and FHA should have necessarily opposite signs? May be this assertion is coming from modelling of flow over hills (Katul et al., 2006, BLM) but these authors stated in their conclusions that: "these model calculations were conducted, (i) for idealized conditions, including steady state and neutral flows, constant air temperature and vapour pressure deficit within the canopy, and constant forest floor respiration, and (ii) using an analytical model for u and w accompanied with several simplifications (see FB04). Caution must be exercised in any extrapolation of these model results to specific field conditions". Experimental data have shown that these fluxes can have the same signs in certain conditions (for example: Feigenwinter et al., 2008, AFM: site of Renon, figure 8).

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Minor comments:

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

Reply: The eddy-covariance method is now cited.

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

Reply: The figures have been interchanged and improved.

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

Reply: The sentence has been reformulated.

Referee comment: 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

Reply: The whole paragraph has been reformulated.

Referee comment: 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.

Reply: The values and units of the fitting parameters are now in the text and not in the figures.

Referee comment: 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.

Reply: Because the canopy height is not clearly defined, it would be confusing to indicate it in the figure. The approximate depth of the gravitational layer has been added.

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Referee comment: 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.

Reply: An additional point has been added for the u profile at 22m. This point is not used in the fitting procedure but gives an idea of the shape of the measured profile between 14m and 22m. The normalization heights are now recalled in the caption of the figure.

Referee comment: 8. Fig 2. Symbols are not explained.

Reply: Indeed. The definition of the symbols has been added in the caption.

Referee comment: 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.

Reply: You are right and this information was missing and provoked big confusion for Dr. Eugster. Please see the reply to his general suggestion n°1.

Referee comment: 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.

Reply: You probably missed something about the flow directions but the slope and the slope flow are from the SE to the NW, thus the slope flows must be indicated with a cross in this figure.

Referee comment: 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 CO₂ for SW sector is larger than for NE sector.

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Reply: This figure has been modified following your suggestion and is indeed more readable now.

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

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