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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.

Received and published: 3 March 2008

Referee comment: This manuscript provides us with more evidence that CO2 advection due to slope flows is a highly complex problem, very site-specific, and often difficult to diagnose. While there is a wealth of information in the data the authors have collected, there are a few issues with the interpretation and the format that will need to be addressed before this can be moved from Discussions into the final resting place. In its current form, the paper is somewhat confusing unless the reader is familiar with previous publications of this group.

Reply: We indeed probably relied too much on our previous publications about advection in Vielsalm. We have now recalled more explicitly (mainly in the section 2.4.) the essential information needed by the reader to follow the presentation of your new results.

Referee comment: What bothered me first was that no clear evidence of the slope flow itself (i.e. downhill) presented, in the form of a graph or a table. I know this has been shown in previous publications, but this paper needs to be able to stand on its own two feet. At least the authors should clearly explain (in section 2.4) what quantitative criteria are used to separate out "gravitational events".

Reply: We have decided not to add a figure describing the wind field but this wind field in unstable, neutral and stable conditions is now presented more explicitly in section 2.4. At the end of section 2.4, we have explained the selection criteria used to separate out gravitational flow events.

Referee comment: In this paper, everything is related to winds above the canopy (except Fig.4), and those are apparently usually perpendicular (i.e. NE and SW) to the direction of the slope (NW). No in-depth explanation is given to physically relate these wind directions aloft to what may be happening below; in fact, the subcanopy flow is assumed to be completely disconnected from the flow aloft (see Fig. 2). On the other hand, relationships between w(h) and subcanopy flows are investigated throughout the manuscript. This is a bit confusing and needs to be more carefully discussed 8211; if these flows are independent, can we safely relate w in the flow aloft to divergences in the subcanopy?

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, when we said that the wind above and below the canopy were decoupled, we were talking about wind directions and it does not mean that any physical coupling between the two is precluded (this is now stated when decoupling is invoked, for example in section 2.4). As already stated in reply to A. Sogachev, there are experimental evidences showing good coherence between above canopy w, slope flow behaviour, temperature profile and horizontal CO_2 gradient measurements close to the ground. We propose a mechanism that fits the observations

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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.

Referee comment: I have a hunch that a lot of what you see in your forest may be driven by along-valley flows (which appears to slope down towards the WSW), not only by the local slope at the tower. I was hoping that the authors would at least speculate on what makes the NE flow scenario physically so different from the SW scenario; why does one produce a T profile inversion and the other one does not? On the local scale, and as stated by Dr. Eugster (and mentioned in your manuscript in 3.2) the step-change in canopy characteristics at your site may have a lot to do with what you observe at the site. The estimated advection magnitudes show that things are probably more complicated than assumed. It is unfortunate that you only have measurements down the slope and not perpendicular to this line; also, given that so much of the advection probably happens rather close to the ground, it would have been good to have anemometer measurements below 3m. Things to keep in mind if you intend to continue your measurements there, as I hope you do!

Reply: These remarks are pertinent. Concerning physical explanations of the differences between the two scenario (SW and NE), a lot have been already done in this manuscript. However, we are not able to explain the mechanism leading to such different situations. This would probably require measurements on a wider scale that are not available in our case and probably on a majority of sites. The possible influences of a step change in canopy characteristic close to the tower are discussed in the reply to general suggestion from Dr. Eugster point 4 and at the end of the section 3.2 of the revised manuscript. Please have a look to these replies. Concerning the design of the experiment, we must admit that we learned a lot from this campaign. With more instrumentation available, we would have built a more flexible 3D setup. However, this set-up limitation was partially circumvent by moving the horizontal transect parallel to the slope to test the representativity of this transect (see Heinesch et al., BLM, 2007). The results obtained (and summarized in the section 2.4) encouraged us to think that the flow and concentration field close to the ground was reasonably bi-dimensional in Vielsalm. In that case, it was reasonable to increase the spatial resolution of the measurements in the dimensions where the main part of advection is expected. Concerning future measurements, the lessons learned from the Vielsalm campaigns have already been useful when designing and performing the ADVEX campaigns (see Feigenwinter et al., AFM, 2008). Because there is a lot of work to be done on this ADVEX database, no further advection campaigns are scheduled for the moment in Vielsalm.

Referee comment: There are also many instances in this manuscript where things are not explained as clearly as they easily could be. One example: caption of Fig 4; vertical velocity above the canopy. Why not indicate the exact height (i.e. 40m)? If you can be specific, you should do so. Another: the NEE estimate in Fig 10 - how was that calculated? These are things that are surely very obvious to you, but to the reader who has not read your other papers this should also be transparent. I am wondering whether reading Aubinet (2007), which I have not seen, would explain much that has been left out, but again, this manuscript needs to be able to stand on its own feet.

Reply: Efforts have been made to be more explicit whenever it was possible. The publication Aubinet (2007) is accepted for publication since many months. This publication being part of a special issue, delays of publication are particularly long but it should be available soon. One reference of this paper (P4242L6) has been converted to Aubinet et al. (2005) which already developed a model predicting the sign of the horizontal gradient that should be observed.

Referee comment: In my opinion this manuscript will be acceptable for publication once all the specific comments of the other reviewers and mine are addressed, and more

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attention is paid to exploring and explaining the possible physical links between what goes on aloft and the flows near the ground.

Reply: We hope to have solved all the issues highlighted in your careful review. We would like to thank you for this review that helped us to improve the manuscript.

Specific comments:

Referee comment: 1. repartition: not sure whether this is recently adopted terminology; if so, I missed that. Just "partition" will do [4231L19, 4246L12]

Reply: “source repartition” has been replaced by “source distribution” on line 4231L19 and 4246L12.

Referee comment: 2. 4233L15-19 Please clarify that you only used data for which the near-ground (3 or 6m) flows were parallel to your slope transect (if I understand this correctly!). It may be worth mentioning that this obviously makes calculating a divergence more justifiable but you are only looking at a selected fraction of all possible cases and therefore will end up with biased results. As mentioned, this can be fixed by setting up a lateral transect for future measurements.

Reply: Your remark about representativity of the subset of data is now included in section 3.1. After filtering, 38% of the initial dataset is kept and we postulate that this subset is representative of the whole dataset.

Referee comment: 3. 4234L5 should be "departures of the instantaneous values from the average"

Reply: The text was changed accordingly.

Referee comment: 4. Eq (1) and (2) have been discussed by the previous reviewers; nothing to add to those semantics.

Reply: Please have a look to the corresponding replies.

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Referee comment: 5. 4236L10-12 I like the way you formulate the separation of dependencies to set up the scale factor approach. Very elegant - I wish I had thought of that!

Reply: We thank the reviewer for this encouraging remark.

Referee comment: 6. 4237L8 as mentioned by another reviewer, it would be good to show data to support your u profile.

Reply: An additional measurement point at 22m has been added. This value is not used in the fit for g(z) because this fit is limited to 20m through the definition of the Beta function (equ. 4) but this additional point gives an idea of the measured profile close to 20m.

Referee comment: 7. Eq(5): you should more clearly define uoref due to its importance in what follows. It is hard to figure out what the reference height is, and it should be shown in Fig 2.

Reply: The reference heights were given unambiguously on P4237L23. They have now been added in the caption of Fig. 2.

Referee comment: 8. 4238L9 you state the uncertainty on k, but not the mean!

Reply: The mean k has been added.

Referee comment: 9. 4238L13 replace "more simple" with "simpler"

Reply: The text was changed accordingly.

Referee comment: 10. 4240 L5: traducing - do you mean "producing"?

Reply: Yes, the text was changed accordingly.

Referee comment: 11. 4241L3: where does that angle (1.7deg) come from?? Is that the average slopevfor NE flows? I thought the slope angle was 3deg.

Reply: The local slope angle is 3% as stated in the site description section (2.1). And

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3% equal 1.7°. We have now given the two units for the slope in the section 2.1.

Referee comment: 12. 4241L5: do you mean "braking"?

Reply: Yes, the text was changed accordingly.

Referee comment: 13. 4242L2: missing a few articles: air circulating in the gravitational flow layer, and it helps to dilute the CO2.

Reply: The text was changed accordingly.

Referee comment: 14. 4243L3: going from -0.05 to -0.1 is a decrease by definition.

Reply: Indeed. The text was changed accordingly.

Referee comment: 15. 4243L78: I think you are talking about delta c, not gradients; but either way, the units (umol/m) are wrong.

Reply: You are right. The units were wrong: umol m-1 has been replaced by umol mol-1.

Referee comment: 16. 4243L16: how was the NEE in Fig. 10 calculated? This is not explained anywhere; obviously not from Eq (1). I presume you used a simple soil temperature model, but please be specific.

Reply: This precision was made more explicit in section 4.1: “The NEE was deduced by a simple empirical soil temperature model calibrated using turbulent and storage measurements selected at high values of friction velocities.”

Referee comment: 17. 4245L18 gravitational layer top: where is this? Please be specific. You may have noticed that if you assume linear w all the way up to h (40m), i.e. dw/dz = w(h)/h, you get du/dz = 0.002 s-1, which isn’t far off your direct estimate. Food for thought (about the depth of the slope flow, and relevant scales etc.)

Reply: The gravitational layer top is estimated to be at 20m (half the canopy height). The factor between w20m,point estimate and w20m,estimated from divergence is four

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and not eight as erroneously mentioned (w20m,point estimate means the value obtained by using the measured point estimate a 40m and postulating a linear vertical profile; w20m,estimated from divergence means the value obtained by the divergence measurements in the trunk-space supposed to be representative of the layer 0-20m). The value of this ratio relies on the comparison of point estimates and divergence estimates including the whole stable dataset (see figure 12 in Heinesch et al., BLM, 2007). We simply want to stress here that there is a huge uncertainty on FVA computations coming from the vertical profile of w; a point that is not seriously discussed in previous publications about direct estimates of local advection.

Referee comment: 18. Fig 1: Reverse a) and b). On slope diagram, indicate the NW direction, and the distance between main tower and transect

Reply: The two figures have been switched. The site map is now first. The NW direction and the distance between the main tower and the transect have been added.

Referee comment: 19. Fig 2: mark the "gravitational layer top"; and the reference height, and make sure it’s all English (avec => with)

Reply: The gravitational layer top is now visualized. The reference heights are recalled in the legend and the fitting parameters are now given in the text.

Referee comment: 20. Fig 3:Wouldn’t you expect w to be negative for all wind directions between 45 and 225o (looking at the topography)? For a perfect 2D slope you’d expect a sinusoidal function. Unless we’re already looking at some form of rotated w? If that’s the case, please make that very clear. Also indicate that these are wind directions at 40m.

Reply: Of course, these are w after tilt corrections otherwise it would be senseless. This was precise on P4238L23. It’s now recalled in the figure caption “… rotated vertical velocity component…”. A ‘40m’ subscript has been added on the axis label to recall the fact

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that this is w above the canopy.

Referee comment: 21. Fig 4: define "surface velocity" - what height?

Reply: “Surface velocity” has been replaced everywhere by “velocity 3m above ground-level”.

Referee comment: 22. Fig 6: Apparently these are temperatures relative to 40m, but you need to specifically state that

Reply: 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 the top of the canopy).

Referee comment: 23. Fig 7: move the x-axis down to the bottom

Reply: Correction done

Referee comment: 24. Fig 9: replace "delta" with its Greek symbol; maybe put NE and SW above the graphs to make it easier for the reader to figure out

Reply: Correction done

Referee comment: 25. Fig 10: make f) the same size as the other panels

Reply: All the panels have now the same size.

Referee comment: 26. Fig 11: not sure whether this is worth a figure on its own; maybe add the integrated divergence to make it more interesting

Reply: The figure 11 has been removed, following your suggestion.

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

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