Biogeosciences Discuss., 4, S2737–S2741, 2008
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Biogeosciences Discussions

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Interactive Comment

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 paper presents some interesting and potentially useful results on advective flows and their influence on eddy covariance derived estimates of NEE. But I do not think the paper can be published in its present form. It needs a major revision before publication. However, I will not repeat the criticisms that the paper has already received (most of which I agree with). Rather I will focus my comments on my two most important concerns.

Reply: We would like to thank the referee for his review that helped us to improve the manuscript.

Referee comment: 1 Equation (1) does need some explanation. (A) Prior to introducing Equation (1) the authors attempt to justify it by citing Finnigan (1999), Finnigan et

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al. (2003), and Feigenwinter et al. (2004). But none of these citations actually discuss Equation (1) as posed in the current manuscript. Finnigan (1999), Finnigan et al. (2003), and Feigenwinter et al. (2004) discuss the equation of continuity (mass conservation) in terms of mass density; whereas, Equation (1) is the equation of continuity after the WPL or density terms have been included, so strictly speaking Equation (1) is related to the equation of continuity expressed in terms of CO2 dry-air mixing ratio. It is not the same as the equation of continuity (mass density), because there are assumptions concerning the conservation of dry air that are made when deriving Equation (1) that are not made when deriving the equation of continuity (mass density). (B) It is probably inappropriate to cite Finnigan (1999), Finnigan et al. (2003), and Feigenwinter et al. (2004) as authors of Equation (1) or as authors of the equation of continuity (mass density). There are other papers that are more appropriate. In fact it is probably unnecessary to cite anyone concerning the equation of continuity (mass density) because it has been generally accepted as true (for the last several decades anyway).

Reply: (A) and (B) The discussion about the formulation of the boundary-layer budget equation has been made in the reply to the comment by Dr. Kowalski. Please refer to this reply concerning your remarks A and B.

Referee comment: (C) The authors define NEE with Equation (1). As such NEE is actually comprised of two terms: It is the sum of the vertically integrated canopy CO2 source term and the diffusional flux of CO2 emanating from the soil. Both terms have dimensional units of flux, but only one is a true flux (soil respiratory flux) and only one is a true source term (canopy photosynthesis and respiration). I think it is extremely important to be clear about this distinction. (Note my experience and reading of the literature indicate that the authors are not the only ones who seem to be confused about this issue.)

Reply: (C) The requested formal distinction between the true source term and the soil respiratory flux has been introduced when describing the term NEE.

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Referee comment: (D) My criticism may seem a bit pedantic, but at a minimum it indicates the authors’ lack of precision that many of the other readers have also complained about. Unfortunately, if the authors are confused or unclear about the basic starting point of their analysis, it may cause the reader to discount the entire paper.

Reply: -

Referee comment: 2 The revisions need to state clearly that the main directions of flow (NE and SW) are perpendicular to the slope (NW). In my first reading of the manuscript I overlooked this point, so I am grateful to Ralf Staebler for commenting on this directional issue. But this engenders another concern. How common is this situation within the flux community and how applicable are these Vielsalm results to other sites? Many of the FluxNet sites are likely to be more concerned about anabatic and katabatic flows, rather than cross valley flows, which seem to be more prevalent at Vielsalm. The authors should put their results into a larger context. Are there any other sites that have similar flow characteristics or is Vielsalm unique in its flow pattern? Furthermore, how significant is the directional shear at Vielsalm? Are the author aware of any observational or modeling data to give some indication if it is significant or not? I do know that valley flows have been modeled and studied for many years so I suspect that there is literature available to (at least partly) address some of these issues. It may be helpful to the authors if they examined the following references: Mountain Meteorology, Fundamentals and Applications [edited by C David Whiteman and published by Oxford University Press in 2000] and Atmospheric Processes over Complex Terrain [edited by William Blumen and published by the American Meteorological Society in 1990]. Either of these books may help the authors interpret their results and the observed flow patterns in terms of the larger-scale valley flow dynamics.

Reply: The information about wind directions of the flow aloft have been added to the section 2.4. We have also precise that the selection of the gravitational flow events were made on the basis of the decoupling of wind directions between above and below

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canopy and that this way of working is applicable only due to the particular orientation of the slope compared to the main wind directions of the flow aloft and to the depth of the gravitational layer being less than the forest height. Suggestions about the way to deal with other, more complicated situations have been made by citing the work of Staebler and Fitzjarrald (2005) who have compared the relative importance of the three forces acting on the subcanopy flow (the vertical stress divergence, the buoyancy term and the Bernouilli pressure gradient) to help identifying the type of flow in the subcanopy. About the directional shear and the general flow pattern on the site: 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. We also want to stress that the site is situated on the side of an open valley with a small slope (3%), far away from a mountainous topography. We are thus not convinced that spending much effort on interpreting larger-scale valley flow would bring useful elements to help understanding the observations at the local scale. 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. (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 local 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 the section 2.4.

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