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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.

Anonymous Referee #3

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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.

Major Comments

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 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 CO_2 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). (**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 CO_2 source term and the diffusional flux of CO_2 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.) (**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.

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.

BGD

4, S2325–S2327, 2008

Interactive Comment