**Biogeosciences
Discussions**

Interactive comment on "Contribution of advection to the carbon budget measured by eddy covariance at a steep mountain slope forest in Switzerland" *by* S. Etzold et al.

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This manuscript reports on efforts to measure the net ecosystem carbon balance of a hillside forest in terms of both eddy covariance turbulent fluxes and also exchanges represented by storage and advection processes. Measuring all such terms to complete the aerodynamic assessment of a boundary-layer "control volume" represents an extreme experimental challenge, particularly in forested terrain. Thus, although complementary to the CARBOEUROPE ADVEX special issue currently in press (in Agricultural and Forest Meteorology) and similarly site-specific, these data are seminal and highly valuable in the developing assessment of exchange processes on spatial scales

exceeding those traditional defined by micrometeorological flux-tower research. Their publication should represent a welcome addition to the growing state of knowledge regarding these processes, once however a presentational error is corrected regarding the appropriate definition (and measurement) of advection.

The error has to do with defining advection in terms of the CO_2 density gradient (with units specified as micromols m-4), rather than in terms of the CO_2 mixing ratio gradient (units of m-1) as in the cited Feigenwinter (2004) article. As presented, both equations (1) and (2) define advection as the integrated product of the windspeed with the CO_2 density gradient. Rather, both should be kinematic flux densities (requiring scaling according to the mean air density to correspond to true flux densities), defined as the integrated product of the windspeed with the CO_2 mixing ratio gradient. This becomes clear when noting that, unless the temperature is measured at each of the 12 gas inlets (and this is not so stated in section 2), the authors lack the data to determine the gradients in CO_2 density (and most likely have not tried to do so), because air density information is lost in the sampling tubes (via thermal adjustment), prior to being measured by their closed-path Li-7000.

Should the authors indeed possess such temperature data, and wish to define their budget equations in terms of a mass balance (rather than in terms of mixing ratio conservation, using true advection), then they should be careful to apply the so-called "WPL" corrections to every relevant term in their control volume mass balance, as described by Finnigan (2009; Agricultural and Forest Meteorology, 149, 725-729). Far simpler, however, would be to specify conservation of the mixing ratio and so rewrite equations (1) and (2) to define kinematic advection as described above. Indeed, I believe that this is already consistent with the analyses that the authors have applied, and so should have no affect whatsoever on their results and conclusions, but rather only reframe the underlying theory in the most appropriate terms.

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