General comment on the paper 'Contribution of advection to the carbon budget measured by eddy covariance at a steep mountain slope in Switzerland' by S. Etzold, N. Buchmann and W. Eugster

The paper from Sophia Etzold and colleagues presents a new CO_2 advection experiment carried out on a steep mountain slope. In addition to turbulent and storage fluxes, they calculated vertical and horizontal advection by using a very simple set up. They also measured CO_2 efflux from the soil by an automated chamber. Results are presented with the interesting perspective of agreement among differently computed CO_2 fluxes. They conclude that a simplified horizontal advection computation, where only the first 2 meters above ground are considered, can improve the quality of the overall CO_2 exchange measurements, enhancing the agreement among measured CO_2 flux values, while the vertical advection addition is not useful, giving only a lot of scatter.

The paper is well structured and excellently written; the figures are generally clear. However, the experimental set up showed some weaknesses. I recommend the paper for publication with two major requirements: 1) to clarify if their findings are empirical, or they follow a physical principle, in order to understand in which perspective their simple set-up could be applied at other sites to correct for advection; 2) to point out the limits, in terms of accuracy and precision, related to the simplifications introduced in their set up, including representativeness of horizontal sampling points, and computations.

Specific comments

Page (P) 1638, Line (L) 23: 'The vertical profile is running along the flux tower and continuing downward…' It is not clear what the authors mean for vertical, probably the height above the sea level. It is really uncommon such definition, physical properties of the surface boundary layer change mostly as a function of height above ground. The vertical advection is computed in a different (streamline) reference coordinate system, the horizontal advection along the slope. I recommend making an effort in order to be consistent.

P 1639 L11: I cannot understand if the air was sampled for 30 s or 10 min at each inlet. The authors should also mention the tubing material and if a dead band was used between two following measurements: measured concentration values are in fact easily contaminated by the previous measurement in case of absorbtion of CO_2 by the tubing material. Uncertainty arising from the number of repetitions done for each sampling point in each measurement interval should be also mentioned, see Heinesch et al., 2007.

P1639 L27: '(MeteoSwiss 2008)': Please give a web link for this indication, and also for P1638 L3. Again, it seems that the authors use elevation above the sea level instead of height above ground.

P1641, Section 2.6: I recommend presenting, in the revised version of the manuscript, the complete derivation of the equations used to compute the CO_2 advection, possibly moving from the mass conservation equation. Although the equations used are the oldest and the most largely applied for advection computation, I recommend anyway pointing out the simplifications introduced, for instance assuming that $\bar{u}\frac{\partial \bar{c}}{\partial x} + \bar{v}\frac{\partial \bar{c}}{\partial y} + \bar{w}\frac{\partial \bar{c}}{\partial z}$ equals $\partial\frac{\overline{u\,c}}{\partial x} + \partial\frac{\overline{v\,c}}{\partial y} + \partial\frac{\overline{w\,c}}{\partial z}$, and considering the advection measured at 1 meter above ground as representative of the overall horizontal advection. In addition, as indicated by Andy Kowalski in the interactive comment, I recommend discussing the effect of the simplification introduced by computing advection from the CO_2 concentrations measured in wet air instead of, more correctly, as CO_2 mixing ratio. There is the chance that some of these simplifications are balancing. I recommend taking advantage from the paper by Sun et al. (2007), from the recently published papers related to the ADVEX campaign where these issues are

treated, e.g. Montagnani et al. (2010), and from the paper by Kowalski and Serrano-Ortiz (2007). I would also find interesting if the authors would like to try alternative computation procedures, for instance following Vickers and Mahrt (2006) for the vertical advection computation, or following Kowalski indications for the mixing ratio.

A note about acronyms used: horizontal advection, as defined in the Etzold et al. paper, does not represent the same flux as defined in previous literature, since it is calculated only in 2 dimensions and only in the first 2 meters above ground. For sake of clarity, I recommend the use of a different acronym than $F_{HA,}$ for instance F_{HA2m}.

P1643, Section 2.8.3: Energy balance closure is not the first choice tool to identify advection. For instance, in the ADVEX campaign (Feigenwinter et al., 2008), a good energy balance closure was found also at sites strongly affected by advection (Moderow et al., 2009). A technical note: placing only two soil heat flux sensors side by site we obtain little information about spatial heterogeneity in heat flux.

P1645 L13: 'The temperature profile…' I recommend, in the revised version of the manuscript, to show and discuss only the physical properties measured along the tower. I think that there is not any physical reason for having a temperature maximum at 5 meters above ground in stable conditions, so the Authors probably refer to their prosecution of vertical (?) profile along the slope, confounding the reader. Please also mention if the temperature sensors used for the profile were intercalibrated, screened and aspirated.

P1646 L15: Probably a verb is missing here.

P1646 L22: 'the stable nocturnal boundary layer (SNBL) started to grow…'. This is realistic, but not consistent with Figure 5b.

P1646 L25: 'SNBL has reached the sites elevation…' I cannot understand. The authors believe that the SNBL depth is a function of elevation above sea level, and not of the distance above the ground? Please check and clarify.

P1647 L9: 'Thus, for further analyses we had to remove night-time fluxes below -5 $\mu molm^{-2}$ s^{-1}'. If only a side of the probablity density distribution is removed, bias is introduced in the average. I think that the despiking chriterion described at P1644 L17 ('Outliers in variable x were defined as values outside the $\pm 3\sigma$ range of the empirical distribution of x') is better.

P1651 L9: 'Thus, an integration height larger than 2 m would not have been representative for our measurements…'. I do not agree with this point. To be physically correrct, horizontal advection measurements have to be integrated in the vertical profile (Staebler and Fitzjarrald, 2004). Instead, I think the Authors should consider the large uncertainties in vertical advection computation (Leuning et al., 2008), and the simplifications applied in the computational approach used, see my note to Section 2.6.

It is realistic that the advection flux has its maximum at 1 m above ground in a sloping terrain, but it is hardly believable that it is limited to that height, excepted at locations very close to the mountain ridge, see Aubinet et al. (2005) for a conceptual model and Feigenwinter et al. (2008) for experimental data. Although I'm open to a different experimental evidence, in my view the proposed method to compute CO_2 advection can be acceptable only if it is considered an empirical proxy to a complex phenomenon, challenging to quantify even by using the best possible experimental set up.

P1667, Figure 4C: I recommend redrawing this panel, removing the azimuth averages, and presenting all measured values. In fact, a 180° averaged value can be given by south winds but also by north winds, ranging around 359°-1°.

P1668, Figure 5: I recommend redrawing these figures, reporting heights above ground, or along the tower, only.

P1669, Figure 6: Is here represented the height above ground?

P 1672, Figure 9. This figure is not very clear, there are probably too many informations in a single panel representing 5 different averages. Give also the units for u*, I guess m s^{-1}.

References cited in my notes, not reported in the Etzold et al. text:

Kowalski, A.S. and Serrano-Ortiz P. (2007). On the relationship between the eddy covariance, the turbulent flux, and surface exchange for a trace gas such as CO2. Boundary-Layer Meteorology, 124:129–141.

Moderow U. et al., (2009). Available energy and energy balance closure at four coniferous forest sites across Europe. Theor. App. Clim.

Montagnani L., G. Manca, E. Canepa, E. Georgieva (2010). Assessing the method-specific differences in quantification of CO2 advection at three forest sites during the ADVEX campaign. Agricultural and Forest Meteorology. DOI: 10.1016/j.agrformet.2010.01.013. In press.

Sun J. et al. (2007). CO2 transport over complex terrain, Agricultural and Forest Meteorology, 145, 1-21.

Vickers, D. and Mahrt, L. (2006). Contrasting mean vertical motion from tilt correction methods and mass continuity, Agricultural and Forest Meteorology, 138, 93-103.