Final Response

We thank all three referees and Andy Kowalski for very valuable comments and suggestions on the manuscript. Main criticism from the referees was a complete derivation of the mass balance equations used and description of the introduced simplifications, as well as the use of the CO_2 density gradient rather than the CO_2 mixing ratio for the calculation of advection. We will change the revised manuscript accordingly (see also detailed responses). Furthermore the description of the vertical measurement profile seemed apparently to be confusing. We will make an effort to clarify this.

Detailed Responses

1 Response to Referee #1

1.1 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.

(See also Response 1.14)

Although a three-dimensional approach to quantify advection would be ideal, our two-dimensional approach is still a physical approximation to true conditions. However, a few additional assumptions have to be made: (1) the CO_2 concentration field in the layer where advection occurs has a simple gradient without curvature; (2) the gradient is along the slope direction given by the katabatic drainage flow winds or the anabatic upslope winds.

With this simplification, depicted in Figure 1, the question of uncertainty of our two-dimensional advection estimates reduces to the two questions: (1) by how much is the along-slope gradient of CO_2 concentration underestimated if the wind flow is not exactly parallel to our gradient measurements; and (2) by how much is the along-slope wind component underestimated. In Figure 1 this is shown by the two inlets at A and B corresponding to the measured two-dimensional CO_2 gradient, where the hypothetical wind flows along the gradient from Q to B. Since under these conditions the line QB is perpendicular to the y-axis, geometrical considerations can be used to show that the gradient measured between A and B is equal to that measured between Q and B if the angle β between the two directions approaches 0° (wind flow perfectly parallel to our two-dimensional gradient), or lower than the true gradient whenever b differs from 0°. Since $\Delta c_{QB} = \Delta c_{AB}$, it is the fact that $\Delta x_{AB} \geq \Delta x_{QB}$ that leads to the underestimation of the true gradient. Using simple trigonometry, we yield $\Delta x_{AB} = \Delta x_{QB}/\cos \beta$, and hence

$$\frac{\Delta c_{QB}}{\Delta x_{QB}} = \frac{\Delta c_{AB}}{\Delta x_{AB}} \frac{1}{\cos\beta} \quad . \tag{1}$$

Using these geometrical considerations we find that the gradient measured by our two-dimensional approach is less than ±5% off the real value for β < 17.8°, and it is still within ±10% for β < 24.6°. Thus, our approach is limited to conditions where the true wind flow does not frequently have an angle of more than 18–25° difference with respect to the established sampling gradient.

With respect to the wind speed similar considerations can be made: if the wind is blowing from Q to B, but measurements are made between at A and B, then

$$u_{QB} = \frac{u_{AB}}{\cos\beta} \quad . \tag{2}$$

Thus, if the underestimation of the along-slope wind speed and the underestimation of the along-slope CO2 gradient are combined, the overall underestimation of advection is $1/\cos^2\beta$. Hence, an error up to 5% is made as long as β < 12.6°, and it is within 10% if β < 17.5°. And if the wind is blowing at a 45° degree angle, this simplified advection estimate would be a factor 2 low.

Our suggestion is thus, that if the near-surface wind system in mostly along a two-dimensional gradient similar to the one shown at our forest site, then the simplification that we made should not lead to order of magnitude differences between approximated and true (but unmeasured due to the lack of a three-dimensional approach) horizontal advection.

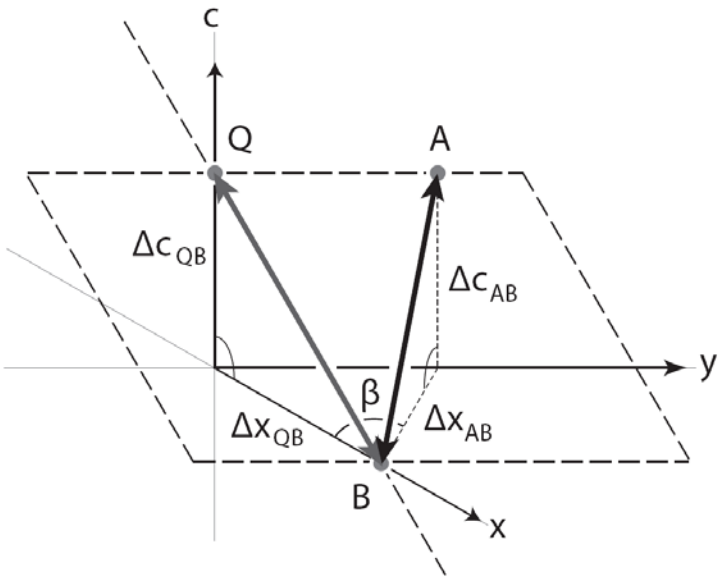

Figure 1. Schematic horizontal CO_2 gradient and the influence of true-along gradient (line Q–B) and actual gradient measurements along line A–B. The two horizontal axes x and y and the concentration axes c for the third dimension are shown. Ideal for a two-dimensional approach to quantify horizontal advection would be if β ≈ 0°.

Specific comments

1.2 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.

This is unclear wording, we wanted to express that the surface-normal profile is starting at the lowest elevation in the local topography (where the drainage flow would experience the lowest point) since the concrete base of the flux tower is a few m above this point. It is exactly what the reviewer thinks of, and we will improve the wording in our revised version to be more clear and consistent.

1.3 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 CO2 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.

We will change and complement the sampling description according to your suggestions. We will add also information on the tubing material (consisting of an inner ethylene copolymer coating, aluminum layer, high-density polyethylene jacket).

1.4 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.

We will add the web links to the reference list.

1.5 P1641, Section 2.6: I recommend presenting, in the revised version of the manuscript, the complete derivation of the equations used to compute the CO2 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 and considering the advection measured at 1 meter above ground as representative of the overall horizontal advection.

We will add this to the revised manuscript.

1.6 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 CO2 concentrations measured in wet air instead of, more correctly, as CO2 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.

We will follow the suggestions by Andy Kowalski (see Response to the interactive comment by A. Kowalski)).

1.7 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 FHA, for instance FHA2m.
We will change the acronyms accordingly, but see also Response 1.14.

1.8 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.
The energy balance closure is usually used as a tool to cross-check the quality of the EC measurements. Based on this premise and reported values from numerous flux sites we state that our EC measurements over sloping terrain are of the same overall quality as measurements conducted over flat terrain. Actually, our results confirm the fact, that the EBC is not very suitable to detect advection. In the revised manuscript we will rephrase the section to point it up.
As the soil heat flux is very small at the Lägeren forest, the error introduced by the spatial under-representativeness of the soil heat flux plates is regarded as minor (see also Section 2.8.3 of the manuscript).

1.9 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.
We will change this accordingly.
Please also mention if the temperature sensors used for the profile were intercalibrated, screened and aspirated.
We relied on the factory specifications for the temperature sensors. The sensors have been calibrated by the factory. The accuracy of the sensors lies within a range of ± 0.3°C. The sensors have been regularly screened.

1.10 P1646 L15: Probably a verb is missing here.
Correct, we will rewrite this sentence.

1.11 P1646 L22: 'the stable nocturnal boundary layer (SNBL) started to grow…'. This is realistic, but not consistent with Figure 5b.
We don't understand the context. Figure 5b is a static figure and presents average values calculated over several hours. The growing SNBL is not represented in this figure and the figure is not referenced in L22.

1.12 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.
There is a scale issue involved here: if we measure on a slope we have the reference plane at the bottom of that slope (either a valley or in this case the valley-like Swiss Plateau), from where a SNBL can grow. On a much smaller scale (and this is most likely what this reviewer was thinking of) we have an SNBL growing above the local topography, that is, where we experience nocturnal cold-air drainage flows. We will reword to clarify the issue, which obviously was confusing the present manuscript text.

1.13 P1647 L9: 'Thus, for further analyses we had to remove night-time fluxes below −5 μmolm−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 criterion described at P1644 L17 ('Outliers in variable x were defined as values outside the ±3s range of the empirical distribution of x') is better.
We will change this for the revised manuscript.

1.14 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 correct, 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 CO2 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.
We performed concentration measurements at discrete levels above the local ground surface, and hence the question is always how to interpolate between the layers. We did not say that advection stops at 1 m above ground, but there needs to be an upper and a lower integration limit for whatever we do. If you consider a theoretical case with a linearly decreasing gradient of CO_2 concentration that reaches the background concentration at 2 m above the ground, then the 1-m height would perfectly be represent the volume-mean concentration difference ($\Delta c/dz$, hence the value at 1/2 the height of Δ is the one that can be used to compute the volume mean). We see that although we thought this is trivial trigonometry we need to be wordier in explanation. If the reviewer considers the Aubinet et al. (2005) and the Feigenwinter et al. (2008) approach to be empirical, then ours is of course as empirical as theirs (however with a physical theoretical background, see also Response *1.1*), because implicit assumptions on the source distribution of CO_2 respiration are made. If we use a 2-d approach along the drainage flow direction we of course assume homogeneously distributed sources all of the same source strength (an assumption that we have to make already in order to claim that this advection estimate is representative at the eddy flux footprint scale). Only a direct measurement of all (!) sources would allow to question this assumption. In our view this is similar to the implicit assumptions made

by Aubinet et al. (2005) and the Feigenwinter et al. (2008), with the exception that they have one degree of freedom more that we have.

1.15 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°.
We agree, and will redraw this figure accordingly.

1.16 P1668, Figure 5: I recommend redrawing these figures, reporting heights above ground, or along the tower, only.
We will redraw this figure accordingly.

1.17 P1669, Figure 6: Is here represented the height above ground?
Correct! We will clarify this point in the revised manuscript.

1.18 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.*
We will adapt this figure in making it clearer and more easily readable.

2 Response to Referee #2

General comments
2.1 However, I agree with the referee1 that the budget equation should be developed more carefully (including all terms) and I have concern about using two different integration heights within one budget equation a priori.
We will adapt the revised manuscript accordingly.

2.2 The structure of the text should be improved in that way that the reader is better taken through the text.
Both other referees stated that the text is very readable and understandable. However, we will try to improve the structure and especially follow your specific suggestions.

Specific comments
2.3 P1638 Site description: I think, it would be useful to get some basic information about the main wind directions. I would also prefer some information (in terms of °) about the upslope and downslope direction of the slope.
We will add those points to the site description of the revised manuscript.

2.4 P1638 Instrumentation: What are the measurements depths for soil heat flux, soil temperature and soil moisture?
Soil heat flux plates were installed at 3 cm soil depths, soil temperature and soil moisture were measured in 5 cm soil depth. We will add this information to the revised manuscript.

2.5 P1638 CO2-concentration profiles: I would prefer if first one profile measurement is completely presented and then the second one. I am also puzzled throughout the text by the used reference heights. Often, it does not become clear

what the respective reference point is. For which area is the topographical minimum representative?
The description of the vertical profile seems to be confusing (see also referee #1 response 1.2). We will rewrite this section.
The Lägeren slope is quite extended and rather homogenous, but with microscale variations. During strong catabatic drainage flows the question of which position on the slope represents the "true" local minimum would be of low importance, but before carrying out the study we were not certain that we would face really strong drainage flows (which would have been against our expectations, given the limited slope length from the Laegeren mountain crest down to our site). Hence, if shallow drainage layers could be expected we aimed at probing the lowest local point in a transect perpendicular to the catabatic drainage flow. This transect is also the axis of the two footprint lobes, and hence this topographical minimum is representative for the footprint of the flux tower. This means: we tried to minimize the unwanted likeliness that substantial CO_2 could bypass our instrumental set-up during nocturnal drainage flows.

2.6 P1640L19-20 I have always problems with the term "correction. I assume the CO2-fluxes (EC-fluxes) are best estimates according to the state of the art. Therefore, I would prefer the term "completed mass balance" following Aubinet et al. (2010).
We agree, and will replace this term as suggested.

2.7 P1641 Integration height: At this point, there is no reason to set the integration height for horizontal advection to 2 m. I have problems with the dimension of the CO2-concentration gradients.
We will rewrite this section and use the mixing ratio of CO_2 instead of the CO_2 density gradient as proposed by A. Kowalski and referee #1 (see also Response to interactive comment of A. Kowalski)

2.8 P1644 Eq. 6: At least it should be noted that storage terms are neglected. Equation number for next equation is missing.
We will correct this.

2.9 P1644L5-15 Some information of this part would be more suitable in section "Site description"(e.g. soil heat flux measurements).
We will change the manuscript according to your suggestion.

2.10 P1644L15-17 It remains open which statistical analyses for which term are carried out with the software package R.
All calculations and statistical analyses were done with the software package R.

2.11 P1645 discussion of Fig. 7b: Unfortunately, a sign convention is not given and should be added. Usually, a positive storage change indicates an increase of the storage i.e. CO2 is added to the storage. Under this assumption, I cannot see a sharp depression of the storage term in the first half of the night. The storage change is smaller but still positive. Therefore, the storage increases and does not decrease. If this holds then also the subsequent discussion should be revised.
We will add a sign convention.
The storage term is usually positive at night (e.g., Baldocchi *et al.* 2000; Aubinet *et al.* 2005; Van Gorsel *et al.* 2007). We agree that the wording “sharp” is exaggerated;

however the storage term is decreasing. Therefore we think that the discussion is still valid. But we will soften the wording in the revised manuscript.

2.12 P1646L25 "That is, the growing SNBL....." . This gives the impression that the SNBL is growing right from the bottom of the valley. I think, this is not what you mean.
This point was also raised by reviewer #1 (1.11) and needs rewording in our revised manuscript. We feel that this reviewer correctly understood our statement, but obviously it was not what he/she expected, indicating that there is a need for a clearer statement.

2.13 P1647L9 How did you define this threshold?
We will follow the suggestions of referee #1 (1.13) in the revised manuscript.

2.14 P1648 I am often mixed by inspected different time periods.
We don't understand this point. The whole study refers to the same time period of May to August 2007.

2.15 P1648L14 "- 1day"; I do not understand this "-1day".
The best correlation of Reco and SR is achieved by a time lag of 1 day. We will rewrite this sentence.

2.16 P1652 different estimates of Reco: I would find it useful if the different Reco estimates would be introduced in the part "Method", explicitly.
We will change the manuscript accordingly.

2.17 Table 1 and Table 2: Do they correspond both to the period May to August 2007?
Yes they do, we will add this information to the table captions.

2.18 Fig. 1 North is not indicated.
North is indicated below the legend.

2.19 Fig. 5 The different heights are confusing. To which period does the Figure correspond?
All averaging figures correspond to the same time period of May to August 2007. We will complement the figure captions accordingly.

2.20 Fig. 8 In my print version the broken lines are black and solid lines indicate medians.
We will correct this.

2.21 Fig. 9 I would transfer main parts of figure caption 9 to the section "Methods" (see remark "different estimates of Reco")
We will change the manuscript accordingly.

2.22 Fig. 10 Figure captions. There are no open circles in my print version, only black and grey circles.
We will correct this.

Minor issues
2.23 P1634L4 The site name is sometimes spelled with "ä" and sometimes with "ae". Please stick to one version.
We will correct the text accordingly.

2.24 P1637L2 "IP" is not introduced.
We will correct the text accordingly.

2.25 P1639L10 "Logan" instead of "Loughborough"?
"Loughborough" is correct, we checked it once more.

2.26 P1648L3 I would remove "sudden" and "short". A peak should be short otherwise it is no peak.
We agree and will remove both words.

3 Response to Referee #3

3.1 Referee #1' suggested 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. Here major concern is if the 2 m-layer integral of horizontal advection flux represents the whole horizontal advection flux. I think that this can be solved by two possible ways.
(1) A simple way is to use as suggested by referee #1 denoting horizontal advection in 2 m-layer instead of using F_{HA}. All discussions would be valid except changing your statements from quantitative into qualitative. F_{HA2m} represents the lower end of whole advection flux because $F_{HA2m} < F_{HA}$ since $U_c > 0$ and $\partial\bar{c}/\partial x > 0$ always at nighttime. We can believe that F_{HA2m} is a large portion of F_{HA} but hard to believe that they are equal because $u_c\partial\bar{c}/\partial x$ is always positive within canopy layer (30m). All features (or discussions) performed by F_{HA2m} is valid for F_{HA} qualitatively. In this way, the explanation for the agreement between u correction and correction should be different, i.e. u* correction is underestimate if $F_{HA2m} < F_{HA}$.*

(2) An alternative way is a state of the art approximation. I guess that there is a superstable layer (Yi, 2008) located between 5 m and 9 m based on Figure 5a and 5b because the Richardson number Ri $\rightarrow \infty$
since $\partial u/\partial z = 0$ (Figure 5a) and $\partial T/\partial x \neq 0$ (Figure 5b). Below the superstable layer, air is relatively neutral (Yi et al., 2005) as demonstrated by Figure 5b. Therefore, you can assume that horizontal CO2 gradients are constant within 5 m layer (Yi et al., 2008).
Thus, you can calculate horizontal advection for the 5 m layer and use it as an approximation of whole horizontal advection flux.

We will modify the manuscript as proposed under point (1), as also suggested by referee #1. We think the patterns in Figure 5a and 5b as described by referee #3 under point (2) indicate not necessarily the existence of a superstable layer, but are a local

effect of the sampling set-up. Apparently the description of the vertical measurement profile was insufficient as also referee #1 and #2 had some misunderstandings. We will rewrite this section. As displayed in Figure 2 of the manuscript the measurements of the vertical profile until 5m above ground are located beneath the tower, whereas the measurements above 5m are located along the tower. This might explain the kink of the vertical wind and temperature profile at a height of 5m.

4 Response to the interactive comment by A. Kowalski

4.1 The error has to do with defining advection in terms of the CO2 density gradient (with units specified as micromols m-4), rather than in terms of the CO2 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 CO2 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 CO2 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 CO2 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.

We agree and will change the revised manuscript according to your suggestions. However, comparing the advection terms computed in both ways did not reveal big differences. Therefore, the obtained results and conclusions will not be affected by a recalculation.

References

Aubinet M, Berbigier P, Bernhofer C *et al.* (2005) Comparing CO_2 storage and advection conditions at night at different carboeuroflux sites. *Boundary-Layer Meteorology,* **116**, 63-94.

Baldocchi D, Finnigan J, Wilson K, Paw U KT, Falge E (2000) On measuring net ecosystem carbon exchange over tall vegetation on complex terrain. *Boundary-Layer Meteorology,* **96**, 257-291.

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