

Interactive comment on “Regional carbon dioxide and energy fluxes from airborne observations using flight-path segmentation based on landscape characteristics” by O. S. Vellinga et al.

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Received and published: 4 February 2010

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4 February 2010

First of all, we wish to thank the reviewers for the effort involved in detailed and constructive reviews of our paper. In response, we envisage to take the actions presented below before submitting a revised version of our paper.

1 Reply to both C 3330 and C 3730 using numbering of C 3330

1. We are aware that the stationarity test in our current study is not in line with conventions used in the tower based eddy correlation community and that perhaps it has its limitations. An extensive discussion of QA for airborne eddy correlation is beyond the scope of this paper. In fact, we are currently preparing a separate paper dealing specifically with such issues. In the current paper, we used the same method previously applied by Gioli et al. (2004). In the revised paper, we will present a brief discussion along the following lines. The shorter the sections of turbulence data compared to assess stationarity, the more likely it is they will differ, due to stochastic variations in the data. Our stationarity test is based on two sub-windows in a 2-km averaging window for which we calculated the fluxes. These windows consist of only about 60 sec of data. With these short time windows, we have chosen for not too tight stationarity criteria. With these relaxed criteria, we have found that using more than two sub-windows would flag more than

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50% of our data as non-stationary. Therefore, we left the current stationarity test unchanged. Note that variations between windows integrated in the segments (which could also be interpreted as a form of non-stationarity) has explicitly been retained.

2. We completely agree and we have been well aware about it during our study, see also the related paper by largely the same authors referred to already in the text Hutjes et al. (2010). We have tried to elaborate on this in our discussion section in the paper. Apparently this was not clear enough. Therefore, we will try to improve on this.
3. Again, we completely agree and we have been well aware about it during our study. Therefore, we first have plotted all 2-km footprints from all flights on top of each other. From that, the average footprint area seemed very much to be at both sides of the track for the periods under investigation. Therefore, we assumed that footprints symmetrical to both sides of the flight track would give a good approximation of the conditions under study. However, it is clearly only valid in the case in which fluxes are averaged from more than one flight. Due to retrieving average fluxes from more than one flight, we assumed that a displacement height was not necessary here as well. We will include this explanation in the text plus the described figure perhaps as supplementary material
4. also (12). We agree with the reviewers recommendation. Both distributions have been updated. MG is now overestimated by only 8% (instead of 12% in the old case), but is still the 4th most important land-use class in the full domain. WM is now underestimated by only 4% (instead of 9% in the old case), which now the 2nd most important class in the full domain and the 4th most important in the flight domain. We will update the text accordingly, including a note on the minor classes that together constitute < 10% of landscape.
5. 5.1) Data in a 2-km window that overlapped adjacent segments, were not
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removed for calculating the 2-km fluxes and instead contribute to the segment average fluxes. This point has been discussed in the discussion section of our paper, though apparently not clear enough. We will revise our text to make it more clear to the reader.

5.2) In addition to the previous point, no land-use information was included from neighbouring segments to any of the segments. We will specify on the appropriate place in the text.

5.3) On close consideration we can not explain the relatively large uncertainty by overlap with adjacent segments, the contrast in fluxes between these adjacent segments is simply not large enough to explain this. We will follow the reviewers suggestions to check this. If confirmed we will analyse other possible causes like the relatively large topography changes on each end of this segment and report the outcome in the revised paper.

5.4) The larger error bars in IOP 1 are partly influenced by the length of the time period the segment averaged flux represent. During IOP 1, the time difference between the 2-km window flux taken together ranges between 25 min to 1 hr, while during IOP 2, the difference ranges only between 10 to 20 min. See time stamps in Fig 8 and 9. Another reason may be larger contrasts in vegetation density between fields within segments in this season. We will discuss this more explicitly at the appropriate place in the revised text.

6. 6.1) also (6.2) and (6.4). We agree with the reviewers analysis here and will include most of it at the appropriate place in the revised paper.
6.3) The reviewer misread P.10491 ll.27. The correspondence is for the other segments, not for 6 and 7.
6.5) See (5.4)
7. Though station Lamasquère is in a field of winter wheat as is the dominant cover

type of that segment, it is not correct to assume segments 11 and 12 are exclusively of this cover. Therefore, a one-to-one comparison is not meaningful.

8. Also C 3730 remark no. 10. C 3330: Error bars represent the standard deviation of single passes only. We note this in the caption for every relevant figure.
9. Corrected.
10. Added the information at the appropriate place.
11. Corrected.
12. It is indeed better to use only those segments which contain flux data. Therefore, we included a remark in the text with an updated Fig. 4.
13. Issue worth discussing; will be included.
14. CV stands for coefficient of variance. Abbreviation has been replaced by full name.
15. Corrected.
16. 16.1 Also C 3730 remark no. 2.: In general, all flights were performed such that the airplane was kept as level as possible, i.e. banking of the airplane was not allowed to exceed 15 deg. Windows from bends in the flight with banks in excess of this have been excluded from the analyses. We will include this explanation at the appropriate place in the text.

16.2 The reviewer expects a potential difference of data quality depending on the angle between the flight track and mean wind direction. Referring to Desjardins et al. (1989), this could be induced by limited frequency response of the sensors in relation to the possibly more higher apparent frequencies when flying into the wind as compared to when flying downwind. Thus, high frequency

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loss would be higher into the wind than downwind. We believe, this effect will be negligible in our case for the following reasons. We used faster sensors sampled at higher frequencies (50Hz) and flying at lower airspeed, thus, reducing overall high frequency loss compared to Desjardins. We flew in very calm conditions with wind speeds lower than those encountered by Desjardins, thus reducing differences in high frequency loss as a function of the angle between flight and wind direction.

17. Corrected.
18. Standard deviations will be added at the appropriate places as requested by the reviewers.
19. Also (26). We will change time notation into UTC throughout the text for consistency.
20. The table 2 included here, accidentally was an old version based on a (too) simple algorithm to overlay segment outlines on the land cover map, and using only one sided footprints. We will include an updated version, showing an almost constant area/length ratio of 6.
21. All figures will be updated with larger font sizes.
22. Figure 2 will be updated in colour with legends.
23. Figure 2 will be updated showing the river channels.
24. Figure 3 will be updated with thicker lines.
25. Figure 3 shows indeed an example of the flight altitude. We will clarify the caption accordingly.
26. See (19)

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27. See (item 5.4)

2 Specific to C 3730 using same numbering as C 3730

1. A valuable issue has been raised here. Flying in non-ideal conditions (in our case constantly adjusting flying altitude to terrain height) aircraft motion removal might not be perfect and/or potential flow theory is not fully valid. As recommended, we will investigate the difference in correlation between $W_{platform}$ and the inferred W for a straight horizontal leg and for a real flight, and depending on the outcome will report it at the appropriate spot in the paper, or as supplementary material. In addition such a potentially residual 'up-wash effect' is probably a rather low frequency component. However, our objective is not to have a perfect magnitude of W , like in disjunct or relaxed EC-methods, but instead we use W' in the covariance with scalars. By applying axis rotation, as we have done in our study, we rotate the system to force the mean W -component to zero. This means that any bias in W caused by either of the two issues (and we believe they mostly propagate in the form of an offset in the final magnitude of W) does not reflect on our fluxes.

When absolute magnitudes of W are important the reviewer suggests to follow the approach by Garman et al. (2006). However, Garman et al. (2006) note that their correction factors, which have been derived from wind-tunnel tests, are empirical and depend on the specific set up of the air frame - sensor configuration. We would not be able to adopt it without doing our own wind-tunnel tests.

2. See reply to C 3330 (item 16).
3. This study focuses on central-day observations for which we found PBL-depths well in excess of 1000 m. Hence, an average flying altitude of 80 m should be

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well in the surface layer. In this case, any residual divergence is predictable and possibly correctable only for sensible heat, which has a known linear decrease to zero near the PBL top. While for latent heat and CO_2 , divergence depends on many factors: the PBL development, clouds if present, etc. Also, for these scalars not even the sign of the concentration jump between PBL and troposphere across the entrainment zone is constant. This will make a correction hardly applicable for such fluxes, and we considered this aspect beyond the scope of the present work.

However, during CERES'07, we did make some flights designed explicitly to study this divergence. We will present a brief analysis of these flights as supplementary material to our revised paper. Implications of this in terms of possible error magnitudes for our data will be discussed more elaborately than previous in the revised paper.

4. Our statement in P10486 I.8 is an unfortunate choice of words. We agree that we probably are above the blending height of the individual plots. However, we wanted to express here our assumption that we are, indeed, above the blending height of the individual plots, but below the blending height of the individual "segment areas" with their characteristic land-use distribution. As recommended, we will support this with a simple model and reformulate this section in the text.
5. We agree, but discussing this is beyond the scope of the paper.
6. Our base state is the mean over a 2 km window. The reviewer instead prefers to use the trend over the segment. We believe the two approaches in practice will give the same result, as sequential short windows will follow and remove, albeit stepwise rather than continuous, the same trend. We will explain so at the appropriate place in the revised text.
7. The interpretation of P10488 I.10–15 by the reviewer is very much what we intended. We will follow this idea and mention the percentages of differences be-

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tween the predominant classes in both distributions of Fig. 4 at the appropriate place in the paper.

8. References Brutsaert (2005); Crago and Brutsaert (1996); Porporato (2009) added to the text.
9. Replaced parentheses by square brackets.
10. Please, see reply to C 3330 (item 8).
11. Please, see reply to item 1.
12. Please, see reply to item 6.

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