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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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Regional carbon dioxide and energy fluxes from airborne observations using flight-path segmentation based on landscape characteristics. O.S. Vellinga, R.W.A. Hutjes, J.A. Elbers, A.A.M. Holtslag, and P. Kabat. Biogeosciences Discussions 6, 10479-10517

This is a worthy attack on the difficult problem of rendering airborne flux measurements quantitatively useful over heterogeneous and moderately complex landscape. Though, as the authors recognize, work remains to be done to bring airborne flux measurement to full quantitative utility, this paper is a significant and instructive contribution. The resulting flux distributions credibly reflect the aggregate surface conditions over a quite

complex distribution of land use and terrain forms. Choosing to focus their analysis on midday, the authors apply the airborne flux measurements at their greatest effectiveness in spatial parsing of air-surface exchange : when the signal is strongest and closest to temporal stationarity. The usefulness of this paper, at least in part, is in the encouraging success it demonstrates in representing the spatial patterns of flux in a complicated environment. It is also useful in clarifying the challenges remaining to be solved on the path to quantitatively useful airborne measurement of fluxes.

There are several specific items on which I have comments. 1.Page 10446, lines 5 – 7 and Fig. 3 show a lot of climbing and descending to maintain approximately constant altitude above ground. Granted the validity of that goal and the exaggerated vertical scale in the figure, the question remains concerning the influence on the reported vertical wind of such maneuvers and their attendant changes in flight configuration, e.g. upwash. Such influence can be assessed by comparing the measured vertical velocity of the probe to the inferred vertical wind. Correlation would indicate trouble. There may be a statistical way to mitigate the effect : see Garman et al., (2008, Boundary-Layer Meteorology 126, 461 – 476). See also Item 11. 2.Fig. 2, in the same vein displays multiple sharp corners in the track. How were the turns actually executed from one segment to the next? The procedure should be declared in text or caption. Presumably for the sharper corners a loop was made, and data acquired during the loop were rejected from the analysis. If any data acquired during turns were used, however, a comment analogous to 1 applies. 3.A flight altitude of 85 m above ground (page 10486, lines 7 and 8), understandably necessary in such terrain and population density, is nevertheless rather high. The authors assumed flux divergence negligible, but quantitative utility of the data is significantly diminished lacking the confirmation given by a sample of fluxes from a "representative" surface site independent of the airplane. From 150 m above the ground in Oklahoma (granted, nearly twice as high) we found divergence in reported sensible heat flux sufficiently strong and inexplicable to render the airborne data useless (at least so far). 4.Was any quantitative estimate made of the blending height? This depends, for one thing, on the scale of the heterogeneity. Our data showed a flight altitude of 30 m and less over Illinois to be below the blending height because the distinction between the two major crops had comparable magnitude to that observed from fixed sites on the surface. The pattern in Illinois was enormously simpler than that found in southwest France. The scale of the features in Illinois was several hundred meters. I expect the complex cultivation patterns (CCP) in France to have individual scales rather smaller than this. Most likely 85 m is above the blending height for such features. If so, the airplane was sampling the composite flux, justifying the authors' aggregation of these smaller features. Finding distinguishable signatures from the different composite classes supports the hypothesis that 85 m was beneath the blending height on those classes' scales. Appropriate surface measurements would greatly enhance the quantitative usefulness of this result. An empirically supported blending-height model, as a function of mixing strength and surface-feature scale could remove a lot of this hand waving. So far, Mahrt (2000, Boundary-Layer Meteorology 96, 33-62) is the only one I know who has attempted this. 5.Finding mixing depth by airborne profile (page 10486, line 10) is actually quite reliableâĂŤsignificantly more so than by radiosonde. The airplane is moving rapidly horizontally through the air during its climb. It samples much more of the entrainment layer than does the balloon rising vertically, but drifting largely with the same feature horizontally. 6.Page 10487 : I found no reference to the base state from which the turbulent departures were determined. If it is a mean over 2000 m, it's probably too short, as discovered during the FIFExperiment. If the base state is an average (or trend) over a whole segment then no problem, especially since the fluxes were ultimately averaged over entire segments. Were cospectra between departure quantities plotted to evaluate the range of scales over which there was flux? See also Item 12. 7.Page 10488, lines 10 – 15 : The exercise described appears vital to the validity of the conclusions, given the complexity of the underlying land use, but the discussion confuses me, perhaps because the percentages given are not as important to the story as they appear from their position in the text. I see the major story of Fig. 4 to be : 1) the fractional distribution of land use within the footprint was compared with the fractional distribution over the region

it was supposed to represent, 2) the dominant land uses in both were MG, WM, WC, CCP(WC) and CCP(SC), 3) MG and WM didn't match particularly well, but there is a quantitative assessment of the discrepancy of which one could hope to make use. 8.Page 10489, line 18 : Support by reference of the assertion that the Bowen ratio and evaporative fraction are closer to constant in time than are the individual fluxes would be useful here. 9.By my opinion, at least, the expressions in line 17 would be less confusing if they were not in parentheses. The outer parentheses though actually accessories to the text appear instead to be part of the expressions. 10.Page 10491, line 4 : "Error bars" are mentioned, but do these represent error in measurement or variability between and within runs? 11.Page 10496, line 5 : Here is recognized the problem I mentioned in Item 1. Not only segments 4 and 5, but also segments 7 and 8 and segments 9 and 10 exhibit strong changes in height from one to the next. Another question concerns the continual climb throughout the length of segment 9, a total gain of about 170 m. Examination of the relation between the vertical component of the probe's velocity and the vertical component of the reported wind will help assess the degree of contamination by departure from ideal conditions. With luck, the contamination will be small and/or removeable. 12.Page 10496, line 10 : Here is recognized the problem I mentioned in Item 6. The subsequent averaging of the fluxes into longer segments compensates for the shorter averaging windows only if the base state involves an average over the greater length. The process of splitting a turbulent departure off of a base state is explicitly or implicitly a high-pass filter. It is at this splitting stage that the longest admitted scale is determined for the turbulence. Perhaps a base-state scale of 2000 m is necessary in the presence of relatively deterministic mesoscale flows produced by the terrain. Probably the only way to know this is to have stationary observations located on the basis of theory and retained in place long enough to identify any predictable patterns. Hopefully these could be successfully modeled by a dynamic mesoscale model. This is the challenge of turbulence measurement in complex terrain.

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