Atmos. Meas. Tech. Discuss., 2, C313–C319, 2009
www.atmos-meas-tech-discuss.net/2/C313/2009/

Atmospheric Measurement Techniques Discussions

Interactive comment on "A Relaxed Eddy Accumulation (REA)-GC/MS system for the determination of halocarbon fluxes" *by* K. E. Hornsby et al.

K. Hornsby

keh114@york.ac.uk

Received and published: 17 June 2009

Example time series of up- and down-draft concentrations from one day Plotted in Figure 1 are the concentrations observed in the upadraft and the downdraft for CH_2Br_2 for the 10^{th} to the 11^{th} of September along witht the limit of detection and tide height. There is a much larger level of variation in the downdraft compared to the updraft. the increases observed in the downdraft occur after there has been an increase in the updraft, but do not appear to be closely related to tide height.

the Conclusion section should be rewritten as currently the first conclusion is quite trivial and the second one not based on the results of this study The reviewer is right the comments in the second paragraph about the causes of the flux variations are really speculation.We will rephrase it to suggest these be investigated in future work using our validated and trialled new method. Also we will cahnge the heading to a summary of the work presented.

Page 955, lines 23-25: Here the authors mention that the dead-band width could be increased or decreased with the size of eddies observed. From this description it is not quite clear to me how this was done. Was the dead-band width made proportional to σw as is often the case? This will be rephrased to say the system did not have a value hardwired into the program for the deadband. "The deadband flow limit could be manually increased or decreased within the control program depending on the likely size of the eddies to be observed."

Page 956, lines 6-7: The authors mention that the REA flux measurements must generally be corrected for density fluctuations due to the water vapour. Webb et al., whom the authors cite discusses only on eddy covariance measurements and I am not sure how their results apply to REA, which is partially a parameterized flux measurement method. Furthermore, I doubt that any mass flow controller can adjust to the density fluctuations in the timescales of surface layer turbulence, which would be needed were this argument valid.

This reference will be changed to (Pattey et al. 1992) who extended Webb et al's work for REA. We have used a mass flow meter which only measured the mass flow rate used to accurately calculate the volume trapped in each sample. The MFM corrected for the temperature within the correct time scale. The relative humidity was 6

Page 956, lines 14-17: "The fast switch valves had a response time of 0.1 s so the data from the sonic anemometer were averaged and recorded at the same rate to prevent lag within the system which would create mixing of the air masses and distort the results." I do not exactly understand this sentence. This will be altered to The fast switch valves had a response time of 0.1 s so the data from the sonic anemometer were averaged and recorded at the same rate. The fast response time was required to prevent lag within the system which would create mixing of the air masses and distort the results.

Page 957, line 6: Here it is mentioned that the traps could take 3-4 liters without break-through. What was the typical sample volume of the REA system? The average volume of an REA sample was 3 litres, comfortably below the break through volume.

Page 958, line 16: "The estimated errors..." How exactly were the errors estimated? The errors of the online air and tube analyses were estimated using a Gaussian propagation of errors and are shown in Table 1.

Page 958, line 25: "2.4 Foot print calculations". Should this be "Footprint calculations"? At least this way it is spelled in the text. Same applies to the tithe of chapter 3.4. Remove space

Pages 958 and 959, lines 27-1: Does the approximation by Schmid take account the stability? Also, in some footprint models the footprint is independent of wind speed as the u* scales linearly with it. Is this the case with this model? The Schmid model requires the Obukhov length as an input, which it uses to take account of stability effects. The upwind extent of the footprint is not directly a function a wind speed, except in as far as high wind speed results in larger magnitude Obukhov length scales through the influence of higher friction velocity mediated by the roughness length. The input variables determining the upwind extent of the footprint are Zm/Z0 (ratio of measurement height to surface roughness length) and Zm/L (ratio of measurement height to Obukhov length). The effect of wind speed is therefore included through the use of the Obukhov length but the dependence is, as the referee correctly points out, indirect.

Page 960, 25-29: Here it is argued that as the dead-band width is increased, the sensitivity of the system decreases due to the smaller volume sampled. However, also the concentration difference increases at the same time, so that the conclusion is not so straight forward. If, in addition, the sample flow rate can be adjusted, the increase of dead-band width improves the sensitivity of the system. Although the concentration difference would increase the absolute concentration in the sample would decrease meaning both concentrations would be below the limit of detection. The sample flow is restricted by the fragility of the sorbent traps which proved unreliable at higher flow rates, so an increase of flow rate cannot be used to increase sensitivity. That only leaves an increase in sample time which would result in poorer data coverage.

Pages 960 and 961, lines 29-2: "In order to both prevent mixing of up and down-draft air and maintain a relatively small deadband flow rate, the REA system was modified so that each flow path had an individual inlet to prevent mixing (Fig. 1b)." I think this is rather trivial. In the original design a common inlet was used, as used by Bowling et al (1998), to ensure that the air mass sampled was the one measured by the sonic anemometer. However, simulations tests showed that the common inlet increased the level of mixing between the air masses sufficiently to rendered the flux useless. (inserted above the highlighted section)

Page 961, line 4: It is mentioned that some target compounds were highly photolabile. What were their atmospheric lifetimes at the conditions during the measurements? Literature values for the photolysis lifetime for these compounds are quoted starting on line 10 of page 962.

Page 961, lines 15-16: "A limit of calculable flux (LOCF) is calculated from the product of the percentage precision (Table 1) and the highest concentration in the tube pair." What is the justification for this? This needs better description. Insert this paragraph is to be inserted above the quoted line . In REA, the concentration observed in the down draft is subtracted from the concentration in the up draft. Consequently, the precision of an REA measurement is the propagated precision for two tubes. Providing that the ΔC is greater than the precision multiplied by the higher concentration from the two tubes, the flux is said to be real. This can be considered the limit of calculable flux (LOCF). This is used to ascertain whether the calculated flux can be said to be real in the same way a limit of detection is used for concentration measurements. If the down draft concentration is larger then the updraft flux and the LOCF are negative.

Page 962, line 10: Deposition during low wind speeds is mentioned here. Were the periods with low turbulence filtered from the data using e.g. suitable u* criteria? Removal of any data would result in low data coverage so there was no u* criteria.

Page 964, lines 3-4: "...as the wind speed increases a larger area is sampled". I already pointed out previously that the wind speed does not necessarily lead to longer footprint. "...as the wind speed increases a larger area was sampled" see previous response.

Page 965, lines 11-14: "Because these simulation tests showed that significant mixing occurred except at high deadband flow limits the REA system was modified to incorporate separate inlets for each flow path (updraft, downdraft and deadband) through the system such that mixing of the air masses could not occur". This is rather trivial and common sense. This line has been includes as a summury of the design process.

Page 965, lines 14-16: "The introduction of a dryer before trapping and the use of mass flow meter negated the need to correct for the density effect due to changes in sensible and latent heat fluxes". This is not shown in the paper. Will be removed from Summary

Figure 2: The labels of the figure are unreadable. Please see figure 2

Bowling, D. R., Turnipseed, A. A., Delany, A. C., Baldocchi, D. D., Greenberg, J. P., and Monson,R. K.: The use of relaxed eddy accumulation to measure biosphere-atmosphere exchange of isoprene and other biological trace gases, Oecologia, 116, 306–315, 1998.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 951, 2009.

AMTD
2, C313–C319, 2009

Interactive Comment

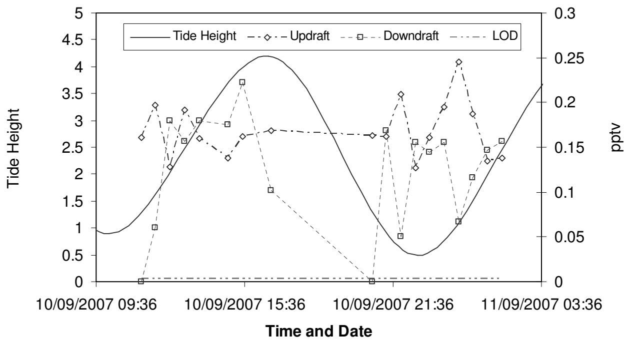

Fig. 1. Updraft and downdraft concentrations of CH2Br2 plotted with tide height and limit of detection

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

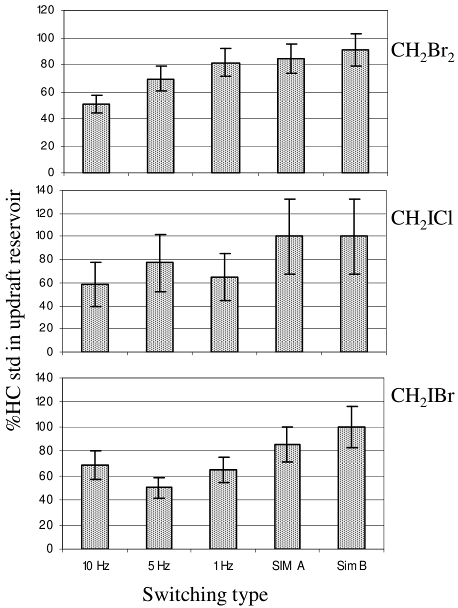

Fig. 2. Results from the simulation tests carried out on the initial REA system.