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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.

Anonymous Referee #2

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The paper presents an REA desinged to measure halocarbon fluxes. It is well written and the measurement system seems well designed. There are a few issues which should be adjusted before the manuscipt can be accepted for publication in the AMT. In general, when as the paper describes an REA measurement system I would like to see an example time series of up- and down-draft concentrations from one day. From this kind of picture one could evaluate if the behavior of the concentrations looks reasonable. 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.

Page 954, line 32: Here it is mentioned that the value for β coefficient is typically

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0.56. it should be mentioned that the beta-coefficient depends also on the width of the dead-band in relation to standard deviation of vertical wind speed (σ_w). There are varying methods for adjusting the dead-band width. One can for example take the σ_w from previously measured period and set the dead-band width to Aσ_w, where A is a conctant typically around 0.5. Value of β is then calculated for example by equation (4) using data recorded during REA sampling. Another method is to use so called dynamic dead-band which makes β independent of atmospheric conditions (Gronholm et al., 2008).

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 σ_w as is often the case?

Page 956, lines 6-7: The authors mention that the REA flux measurements must generally be corrected for density fluctiations due to the water vapor. 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.

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.

Page 957, line 6: Here it is mentioned that the traps could take 3-4 liters without breakthrough. What was the typical sample volume of the REA system?

Page 958, line 16: "The estimated errors..." How exactly were the errors estimated?

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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.

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?

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.

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.

Page 962, line 4: It is mentioned that some target compounds were highly photolabile. What were their atmospheric lifetimes at the conditions during the measurements?

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.

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?

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.

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 incorpo-

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rate 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.

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.

Figure 2: The labels of the figure are unreadable.

References

Gronholm, T., S. Haapanala, S. Launiainen, J. Rinne, T. Vesala U. Rannik 2008: Dependence of the β coefficient of REA system with dynamic deadband on atmospheric conditions. Environmental Pollution, 152, 597-603.

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