Review of the manuscript amt-2018-460 “True eddy accumulation trace gas flux measurements: proof-of-concept” by Lukas Siebicke and Anas Emad

The authors present a new setup for performing True Eddy Accumulation (TEA), where air is sampled separately for updrafts and downdrafts at a flow proportionally to the magnitude of vertical wind speed. This allows for measuring fluxes of constituents where no fast sensors are available that allow direct eddy covariance measurements. Other than Relaxed Eddy Accumulation (REA), TEA is a direct flux method and has therefore less theoretical limitations (e.g. scalar similarity, turbulent characteristics). The main technical advancement that allowed performing the TEA measurements was the development of a mass flow controller, that is capable of regulating the flow at 10 Hz to resolve the turbulent motion. Unfortunately, this work just gives examples of the performance of this new device, but no detailed technical descriptions or a more detailed description of the working principle of the mass flow controllers used. For the advancement of the TEA-technique, it is important that this information will be available in an appropriate way, soon.

Nevertheless, the authors are able to show the superior performance of their TEA-setup with respect to prior attempts in comparison to eddy covariance measurements and perform a thorough error analysis. Furthermore, they discuss in depth the major error sources and give an outlook on how to further improve the performance of the method. The manuscript is generally well written, well structured and the results are illustrated by a sufficient number of figures. Therefore, I support publication after considering the minor revisions given below.

General comments:

The manuscript was originally submitted by one author. Formulations need to be adapted (e.g. I => we).

The authors showed that the flow controllers could reproduce the correct flow with respect to a reference thermal mass flow controller and that they can resolve 10 Hz, but what about a zero offset (leak)? I think it is important to show here that there is really zero airflow if the other channel is sampled.

Specific comments:

P3, L26: R^2 is the coefficient of determination

P8, point 5: As an idea for improvement:

Would it be possible to place a fast thermocouple into the sample airstream and correlate this to the sonic temperature? It might not work for real time correction of the sampling, but give an estimate on the magnitude of decorrelation for post processing.

P9, point 1: As the working principle of the mass flow controller has not been mentioned it is difficult to judge if there are issues in the mass flow control due to the effect of water vapor (e.g. (Lee, 2000). If there is a latent heat flux moisture in the updrafts and downdrafts must be different. Can the authors comment on how this could affect the volume mismatch?

P10, L5: Please provide coordinates.

P14, L16-18: Do not fully understand this sentence please consider revising.

P16, eq. 7: If $\overline{w}$ = 0 => Fc =0. Therefore, it must be either $\left|\overline{w^+}\right|$ or $|\overline{w^-}|$ (as they are of equal magnitude. Please clarify.

P17, eq. 10: Is $\overline{w}$ here the mean vertical velocity (for the averaging period) or is it the "mean of the absolute value of vertical wind velocity". Please clarify.

P18, first paragraph: Would be very valuable to have a schematic sketch of the setup explaining length of inlet lines, position of flow controllers and sampling bags including dead-volumes.

P20, L14: Which type of embedded computer?

P25, L10: How was the reference signal generated? => Description was hidden in the figure caption of Fig. 8. Please include in text as well.

P29: Please enlarge figures 13 and 14. In Fig. 13 the symbols, weather crosses or circles as described in the legend, are not identifiable. In Fig. 14 it is difficult to distinguish the different lines.

Furthermore, from Fig. 14 a) it seems that TEA is systematically underestimating the fluxes during day, which is not so clear looking at panel b and especially the slopes of the linear regressions. Although I understand the reasoning for using standard major axis regression for two independent variables it would be interesting to see how a "standard" linear regression would look like. I think it can be justified to use the EC flux as the "controlled variable" by the statement that EC serves as a reference method.

P30, L19-21: From Fig. 14 it seems that this statement needs to be clarified. The two EC-flux estimates agree much better than the EC and the TEA measurements.

P31, first paragraph: It would be good to name all cases always in the same order or mark them with indices.

P33, Fig. 15: In Panel b the legend is hardly distinguishable from the data-points.

P34, last line: "akin"? Not clear what this word means in this context. Consider revising.

P36, L6:"negative bias" => - 0.08

References:

Lee, X.: Water vapor density effect on measurements of trace gas mixing ratio and flux with a massflow controller, J. Geophys. Res. Atmos., doi:10.1029/2000JD900210, 2000.