Authors' reply to reviewer comments 1

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Review of the manuscript amt-2021-319 "Advances in the True Eddy Accumulation Method: New theory, implementation, and field results" by Emad and Siebicke (2021)

General comment

We thank the anonymous reviewer for his/her constructive, motivating, and detailed feedback on the manuscript. Please find below our point by point reply to the comments.

- Our answers and comments are in blue.
- Summary of changes to the manuscript are in orange.

As per the suggestion of the second reviewer, Christoph Thomas we have split the manuscript into two papers. Comments related to the second manuscript will be indicated by *second manuscript* at the beginning.

The manuscript was split into two related papers

1. True eddy accumulation - Part 1: Solutions to the problem of non-vanishing mean vertical wind velocity
2. True eddy accumulation - Part 2: Theory and experiment of the short-time eddy accumulation method

General comment

The authors address one of the aerodynamic micrometeorological methods for surface-atmosphere turbulent flux measurement, the eddy accumulation. They propose interesting improvements to the classical method, namely the division of the half-hour averaging period into shorter periods and an approach to account for non-zero vertical wind speed. These are intended to reduce the uncertainties in the calculated fluxes and adapt the eddy accumulation system to the emerging faster-response gas analyzers. A theoretical framework explaining the basics of the method and the substantiating the proposed developments is well presented. The success of the modified eddy accumulation is apparent from the close similarity of the fluxes with the estimates from a collocated eddy-covariance system. I evaluate this work as a high quality contribution, which will certainly be of interest to the micrometeorological and flux measurement communities.

My criticisms are related to the treatment of the nighttime periods, the importance of different crops planted on either side of the EC tower leading to step-change in aerodynamic roughness, the actual extent of the vertical wind problem at the measuremet site since it is very flat, and the insufficient evidence of STEA technique superiority over the classical TEA method.

I recommend acceptance of the manuscript after the below points will be addressed.

Detailed comments

Vertical wind more of a problem in sites with complex topography – this uncertainty needs to be discussed.

> We agree the problem is larger in more complex topography, we have emphasized this in the introduction. However, although our experiment was conducted in an ideal site, we don't think this limits the generality of the results.
>
> The relevancy to complex sites was stressed. A new sentence added: "The magnitude of residual nonzero mean vertical velocity is larger in complex sites (Rannik et al, 2020)"

48: I am at a loss as to what kind of error is implied – mean random error of the 30-min averaging periods, mean systematic error, error on the annual cumulative sum, . . . ? It would be good to clarify how big the problem you are solving really is.

> The error is the mean systematic bias of several 30-min averaging intervals.
>
> *Changes:*
> Changed to mean systematic error.

94-95: the local topographical slope should be among the factors

> *Changes:*
> The topographical slope added to the factors as suggested.

100: "realizations" is unclear, perhaps explain that this means the need to record the fluctuations of scalar concentration at a frequency sufficient to represent the individual flux-transporting eddies, i.e. at frequencies higher than 1 Hz. Please specify how long an averaging period (or range) you mean at line 101.

> *Changes:*
> The sentence changed as suggested. And an example of the length of averaging period was added.

115-116 change "maps" for "ensures the proportionality"

> *Changes:*
> Changed as suggested.

231-232: alpha has already been defined.

> *Changes:*
> Redundant symbols removed as suggested.

Section 2.4: it is not quite clear how the division into shorter periods improves the flux estimates (if it is expected to improve them?) Maybe an introductory sentence explaining this should be added.

> *second manuscript*
> The division into shorter intervals improves the practical implementation of the TEA system in terms of dynamic range and flexibility such as running the system in a continuous flow-through mode.
>
> *Changes:*
> An introductory sentence was added to clarify the motivation for the division into shorter periods.

Section 2.6.1. The site description is missing important details on what crops were planted within the EC footprint during the measurement period, how tall the plants were etc. The photograph reveals that there was a significant change in surface roughness just near the tower, effectively dividing the EC flux source area into two contrasting halves. This would have long-reaching consequences for the surface exchange in general, and the functioning of aerodynamic flux measurement techniques, even if the step-change in roughness creates no significant wall effects affecting the

vertical wind speed. Please clarify these points. My recommendation is that, in case the crops were as shown in the photo and it shows in the wind direction dependent quality criteria of EC fluxes, the following sections should treat the two sectors separately.

We thank the reviewer for raising this important issue. The included photograph was taken during the installation and does not reflect the situation during the measurement period.

Changes:
We added the necessary information about the planted crops in the field during the measurements and their heights.

Fig. 3 and its discussion in the text: I think the explanation of the exact method of choosing the representative concentration for each short period is missing. Is it the mean or median? Are there any QC indicators to reject the poor quality short periods, based on e.g. the properties of distribution of recorded concentrations?

second manuscript
The median was chosen as the representative value. Several quality checks related to the performance and statistical properties of the measured values. They were detailed in Section 2.8.2.

Changes:
Updated Fig. 3 description to indicate that the median is used and referenced the other quality checks used for data selection.

Sections 2.9-2.10: how were the nights treated? From my experience, stable periods lead to the generation of thermal decoupling at 0.5-4 m height in such short vegetation sites, cutting the aerodynamic flux measurement system off from the ground sources of scalars. However, as it is well known, moderately stable periods lead to biases in EC/TEA fluxes even at the absence of decoupling. Can you briefly discuss how the performance of modified TEA differs from EC, and how much night-time data you had to reject for the above reasons.

second manuscript
Night data were indeed the majority of excluded values. Only 33 % of averaging intervals were valid during night-time compared to 70% during day time.

The exclusion of night data data was based on quality flags of (Mauder and Foken 2004) that test for violations of stationarity assumption and integral turbulence statistics.

In terms of performance, since TEA uses the same wind information as EC the only advantage it has is the close path gas analyzer. From a theoretical perspective the two methods have the same performance during nighttime but due to the use of difference gas analyzers, EC fluxes seemed to have more spikes during night-time due to the sensitivity of the open-path gas analyzer to water condensation while TEA closed path-gas analyzer was more robust.

Changes:
We added more information about night-time data quality and the performance difference between the two methods.

Line 493 and whole section 3.1.1: you are saying that there is an upper limit on the absolute value of mean vertical wind speed, up to which the alpha coefficient correction is still effective. At the same time, as your experiments takes place in a very flat ground (ignoring the surface roughness changes which require clarification), that one is led to think that nonzero vertical wind speed is of less concern for this site than it is, say, for a mountain site in, or a flat site with a discontinuous canopy consisting of large trees. Please provide some

We agree, for this site, the correction is minimal. This paragraph was intended to offer an example of using this new formulation for defining conditions where the error in the flux becomes significant.

More context was added to clarify the use of the defined upper limit on $\overline{w}$ as a condition to restrict the error in the flux below a certain threshold.

Figure 5: the neutral periods are not visible – covered by the other symbols?

Indeed, most of them were covered by other symbols.

Changes:
We changed the transparency and the color to improve the visibility of neutral periods.

503-505: what range are you referring to?

second manuscript
Were are referring here to the dynamic range of the sampling apparatus.

Changes:
We changed the sentence wording to clarify what is meant by the range.

Figure 6b: the representation of volume as area of the boxes is confusing the way it is currently presented; I think it's better to have the bats of the same width and only vary their length.

second manuscript
Both the width and the height are variables. The width of the boxes is the accumulation interval and the height is the average mass flow rate which is also variable in each short averaging interval, their product is the area of the rectangle which represents the accumulated volume that need to be constant for all short intervals.

Changes:
We improved the description of Fig. 6 to indicate why the height and width are variables.

Figure 8 and the related text: It would be important to add the flux calculated using the traditional eddy accumulation approaches, to show that STEA offers superior performance in terms of smaller deviation from EC.

second manuscript
The effect of nonzero vertical wind velocity was very small in our site (< 1.5%) due to the ideal topography of the site and the online rotations and processing of the wind measurements, therefore we chose not to add the uncorrected TEA fluxes to the figures. As for comparing STEA with traditional TEA, the improvements in STEA are with system sampling and implementation and would need a reference TEA system to show the difference, this can not be realized in post processing without making assumptions about the limitations of the sampling apparatus and the type and use of buffer volumes.