Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
https://doi.org/10.5194/amt-2021-319-RC2, 2022

Comment on amt-2021-319

Christoph Thomas (Referee)

Referee comment on "True eddy accumulation – Part 1: Solutions to the problem of non-vanishing mean vertical wind velocity" by Anas Emad and Lukas Siebicke, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-319-RC2, 2022

Review of manuscript: “Advances in the True Eddy Accumulation Method: New theory, implementation, and field results” by Emad & Siebicke, https://doi.org/10.5194/amt-2021-319

Overview statement:

This paper summarizes recent advances in developing the True Eddy Accumulation (TEA) as the only viable alternative to the eddy covariance (EC) method for determining direct ecosystem-scale fluxes for gas and potentially (not addressed here) particulate species for which fast-response analyzers do not exist. The authors are experts in this field and have been leading the development of TEA, I believe they are the only team currently working on it. In this sense, I commend these advances and was eager to learn about the progress this technique has made moving from its infancy to a more refined, potentially operational stage. I do have, however, serious doubts that the manuscript can be published in its current or mildly revised form because its structure does not lend itself for easy reading and application by non-experts of the flux community, and therefore is unlikely to have the impact it deserves. I am convinced of the merit of the TEA method, the quality of the proposed advances, and the expertise of its authors. In my opinion the manuscript suffers from the following substantial issues in descending order of importance:

- The one manuscript combines three papers with three different foci: a) modifying the original TEA equation to non-vanishing mean vertical wind conditions, b) combining multiple flux estimates at time scales smaller than the intended averaging time scale, the author term 'STEA', which offers obvious practical advantages, and c) addressing and quantifying the effect of fixed buffer volumes and spectral water cross sensitivity of the CRDS analyzer for flow-through TEA applications. While all foci are relevant for the TEA method in general, combining them into one manuscript is not only confusing the reader, but causes it to lose focus, and burying important results in a shear endless stream of information. I am opposed to least-publishable-unit papers, and like the idea of one-stop-shopping about TEA, but I believe it contains too many entangling aspects. It's a tough 29-page read.

- The desire to produce TEA fluxes most closely matching those from EC is fundamentally ill-posed, and only relevant in the infancy stage of the method to prove 'yes we can do it'. I sincerely hope we have moved beyond this stage. Interpreting EC fluxes as true ecosystem behavior (as opposed to studying turbulence only) suffers from many known issues, and often invokes strong simplifications to methods and measurements. Instead, the user of the exciting TEA method and reader of the manuscript is interested in quantifying the total flux (ie the left-hand side of Reynold's second postulate, their equation (2)). In this sense, finding a complicated mathematical method to subtract the advective transport (ie flux by the mean flow) from the TEA flux for non-vanishing mean vertical motions conditions in their equation (16) is ill-posed. At least the authors portrait it this way and motivate their correction. In fact, the right-hand side term in (16) may be a necessary correction when the mean vertical motion does not vanish, but it is sold as a TEA equivalent to the WPL correction, which physically is incorrect. To elaborate, I believe the authors misinterpret the existing and well-accepted WPL correction, which states that the simple turbulent covariance term needs to be corrected for i) flux contributions from the contraction-expansion argument of air parcels passing through the fixed sampling volume of a gas analzers, and ii) a finite non-zero mean vertical motion for moist-air flux.
- The use of the transport asymmetry coefficient α_c has theoretical appeal but estimating it from available fast-response scalar concentration/ sonic temperature for gas species, for which TEA is the ONLY available direct flux method, is flawed because of imperfect scalar-scalar similarity. Nobody requires TEA for estimating CO_2 fluxes, for which results from a field study are shown, but fluxes for reactive gas species undergoing photo-chemical changes during eolian transport, and not just through surface sinks and sources (applicable to non-reactive scalar air temperature, carbon dioxide and water vapor concentrations, etc) are very unlikely to obey the invoked scalar-scalar similarity. They are many recent studies on this issue relating to the relaxed eddy accumulation (REA) group of methods, and now by introducing it to TEA through α_c it gets contaminated too. Not sure if this is a true advance. I also struggle with the various definitions of α_c, which may be inconsistent (see detailed comments below) and its physical interpretation.
- The style of the manuscript varies from section to section from equation-rich and full text explanations to bullet-item to-do list instructions. I realize that this diversity may be caused by alternating authors and could be helpful in in the field, but the manuscript would benefit from homogenization. Quite a few equations can easily be omitted to slim down and sharpen focus, but some require mor detailed derivations and explanations.

I believe that the most important 1) could be addressed by splitting the one manuscript into several independent, but related parts (an option other boundary-layer micrometeorological journals offer), similar to a mini-series of publications. The handling editor will know and decide. Please find my additional detailed comments below.

Detailed comments (kept short for clarity)

- Line 32: please add' out of the family of accumulation methods'.
- Line 40: Please see general comment 2) above, I think it the desire to match the EC flux is flawed, but the true (total) flux should be in the center of our attention when conducting ecosystem studies.
- Line 49: REA can lead to both under- and overestimation when compared to the EC flux, so I wouldn't call it just as a 'loss'. The sign of the change depends on the exact REA method used.
- Line 54 and later: Please specify how you define the required 'large dynamic range' of the TEA method. Provide tangible information.
- Line 58: I think a first brief definition of α_c is in order here, while I do have my doubts about its utility (see general comment 3) above). However, introducing it as a coefficient representing the disparity in the vertical transport between up and downdrafts is helpful.
- Line 60ff: Here is the first break from 'sub-manuscript' a to b (see general comment 1) above).
- Equation (3): It is uncommon to see the EC flux mathematically being defined in the time domain, but in reality it is (almost exclusively) done this way. I believe using number N instead of time increments is more universal, but is a matter of taste.
- Line 87: The statement about the merit of the WPL correction is incorrect, see general comment 2) above. Standard 1-D EC-based mass balance methods eliminate this term by setting $\overbar{w} \equiv 0 ms^{-1}$ through rotation, but in reality it is a true physical term which requires accounting. This is what the WPL does. Flux estimates including the WPL term do therefore not 'correct' the covariance flux $\overbar{w\prime c\prime}, but estimate the 'true' full transport $\overbar{wc}$.
- Line 92: What do you mean by offset? Offsets are instrument-specific properties due to improper calibration or referencing. True physical non-zero vertical motions is a different phenomenon. Please be precise in terminology. I find this paragraph confusing, consider omission?
- Line 101 and throughout: use often state 'computations at the end of the averaging interval', but more precise of 'for the averaging interval', since the decision of sampling into up- or down reservoir is done while sampling, and not just at the end.
- Line 102: I recommend using a different symbol than $T_{avg}& for the averaging interval length, as it may be confused with temperature.
- Equation (6): I recommend removing it here, it does not lend anything to this section, and is only needed later in section 2.4 and Appendix A.
- Equation (8) and (9) can be combined, since V is defined in equation (7).
- Line 33: I believe it may be helpful to point out that the magnitude of w needs to be taken first, then comes the averaging. If reversed, the statement does not apply.
- Line 136: See general comment 2) above. While your proposed correction takes a similar mathematical form, you do not discard the advective transport, but flux uncertainty because the assumption of zero mean-vertical velocity is not fulfilled. This difference is important!
- Equations (11) and (12): This is a key step, explain!
- Line 164: add an 'and' between $|w|$ and c.
- Equation (16) and (17): rearranging is fine, but the utility is not obvious to me. Do we need (17)?
- Equation (18): not needed, consider omitting.

- Equations (19) and (24): I struggle in combining these two different definitions: do you mean the magnitude of the terms $flux \uparrow$ and $flux \downarrow$, or do these terms actually preserve their sign? To me, equation (24) is the inverse of (19). However, later in the results section (line 468 ff) you report values of $\alpha& between 0.2 and -0.18 for CO2, so $|\alpha|<1$. I do not see how these values agree with equation (19), which should exceed unity. The numerator $\overbar{c \prime} |w \prime|} has to be larger than the denominator $\overbar{c \prime} w \prime}. Please clarify!
- Equations (20) and (21): without showing how you solve the integrals these equations have little to no utility. Simply stating equations (22) and (23) may suffice.
- Line 194: Sentence is incomplete at a critical point (after the parantheses).
- Equation (24): still struggling, please see earlier comment.
- Equations (25) to (30): this is not needed and a diversion. Please consider removing. I think you should keep the textual description of how to estimate it from quadrant analysis (it is a nicely analytical vehicle indeed), but it exact mathematical form distracts. Actually, I think it is better suited than the analytical approach in equation (23). Maybe toss this? Either way, please revise this section 2.3.1 and slim it down to the essential punchline.
- Section 2.4: submanuscript b) continues…. See general comment 1) above.
- Equation (36): does not lend anything, it simply rewrites the averaging. Consider omitting.
- Section 2.5: submanuscript c) starts… See general comment 1) above. I suggest moving the fundamentals of the time constant discussion from the results section (particularly Fig. 9) to the materials &methods section.
- Line 263: please explain what this statement means.
- Line 274ff: Please include only the periods for which data are presented, anything else is irrelevant.
- Figure 2: Caption: pneumatic schematic? Not hydraulic suggesting you move incompressible fluids.
- Line 304: 2. Does this mean, TEA sampling needs to start sampling wind velocities 2 days prior to taking the first measurements, or one discards the first 2 days in post-processing?
- Line 320, Fig 3: How large is the variability (ie one form of error) for the repeated samples of the same sampling volume? It appears it may be on the order of 0.5 ppm? Interesting and important to know.
- Line 341ff: How do you correct for water vapor cross sensitivity as the wapor concentration remains identical? Maybe I do not understand the method correctly, please explain.
- Line 353: Please do not use the word 'deadband' here, as it is commonly used in the REA/ TEA community for discarding samples of small flux contribution (HREA) or small vertical velocity. This may lead to confusion, how about 'line flushing volume' or similar?
- Line 367: what is a dynamic flux unit? Please explain.
- Section 2.8.4: Much of this is very technical and difficult to understand. These details distract from the true advances you propose, see general comment 1) above. For me, this belongs to submanuscript c).
- Line 391: Iinear detrending, really? Why?
- Section 2.10, line 400: you despike the time series of final computed fluxes excluding everything exceeding 2σ? This can only be justified by assuming a well behaved biological functioning of the carbon uptake, but not by turbulence transport. How much does that obscure true TEA flux uncertainty?
- Section 2.11: so many filters and flags, I wonder what the TEA fluxes look like without applying all those….
- Section 3.1.1: please see general comment 3) above, and detailed comment t). I struggle with estimating numerical values for α and relating them to your observations. Invoking scalar similarity weighs heavily on this TEA 'advance'. Have you

investigated the scalar-scalar similarity for your field experiments? Even among conservative scalars it changes dramatically with time.

- Section 3.2: Much is repetition from section 2.8, please only report results and their discussion here.
- Line 525f: Please explain this to the reader, it is technical jargon.
- Line 539: Replace ‘stable’ by ‘stationary’.
- Line 545ff: Could the results be reversed here between long- and short-term rotation window?
- Section 3.3.3: is this truly needed?
- Line 578ff: Comparing the absolute uncertainties across studies is not useful as the site conditions and sampling methods are very different. This needs to be communicated in terms of relative flux estimate uncertainty.
- Section 3.4: as this appears to be a key goal for the authors, I would like to see a comparison of their full new TEA method (equation 34) with the traditional TEA method (not including the proposed corrections), as well as the magnitude of the correction terms (second and third term in equation 16) separately. This will ultimately demonstrate the utility of the proposed corrections.

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