Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
https://doi.org/10.5194/amt-2020-479-RC2, 2021

Comment on amt-2020-479

Johannes Laubach (Referee)

Referee comment on "The high-frequency response correction of eddy covariance fluxes – Part 1: An experimental approach and its interdependence with the time-lag estimation" by Olli Peltola et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-479-RC2, 2021

General comments

This manuscript is a valuable contribution aiming to improve the data correction methods for eddy-covariance (EC) measurements of trace gas fluxes. This is highly relevant because the EC method is used at hundreds of sites around the globe, often continuously for many years, to quantify the carbon exchange of vegetation, greenhouse gas source and sinks, and evaporation. The authors treat in detail, both theoretically and with experimental data, how the effects of high-frequency attenuation of gas measurements and of time lags between the gas and wind measurements influence and compound each other. With that, they clear up two points of debate and sometimes confusion (see below) and give guidance how EC gas flux computation algorithms should be organised. Given the widespread use of EC for gas flux measurements, this paper has potential for high citation count.

(1) The first point of the debate clarified here is that, with correctly determined physical lag time between wind and gas (or other scalar) signals, the transfer function for cospectral attenuation is equal to that for the scalar's power spectrum, not to its square root. Even though this has been shown in detail before (Horst 1997), it is worth stating again because the erroneous square root keeps appearing in recent eddy covariance methodology papers, such as Nemitz et al. (2018).

(2) The second point is novel and shows how things change if lag times between wind and scalar signals are determined using covariance maximisation (with uncorrected attenuated data). The covariance maximisation is a dubious yet widespread practice. It overestimates the lag time by including the phase shift effect of the low-pass filtering. The authors show that after covariance maximisation, the cospectral transfer function is no longer equal to that derived from the power spectrum, and they derive a correction to compensate for this. This correction is the truly novel contribution of this paper.

Unfortunately, (2) gets a bit muddled up by the authors claiming that the cospectral transfer function after covariance maximisation is obtained approximately if the square root of the power spectrum's transfer function is used to correct the cospectrum, i.e. by reverting to the original misconception addressed in (1). In other words, one imperfect (or incorrect) processing step would be fortuitously compensated by another. I strongly suggest refraining from putting it this way, because a) in a mathematically exact sense, it is incorrect, and b) the approximation will quickly become inaccurate for lag times exceeding 1 second (outside the range tested in this paper). I explain these reservations further in the Specific Comments.

Overall, I think this manuscript is worthwhile publishing after revision (as detailed below) and with modified conclusions, along the following lines.

In my view, there should be a strong recommendation to discourage usage of the covariance maximisation method in the future. It is known (and nicely illustrated here) to be incorrect, by mixing two separate effects. In addition, it produces erratic unphysical results when fluxes are close to the detection limit (affecting typically most nighttime periods for H2O and the morning/evening transition periods for CO2).
It is not that difficult to determine the physical lag time with other methods. Firstly, its expected value can be constrained by geometrical dimensions of tube and measurement cell, together with pressure and flow rate (which is known or even controlled for many gas analysers, and if not, a flow meter can be added). Lags due to clock mismatches or processing delays need to be included, too. Once the expected lag time is estimated in this way, it can be empirically confirmed (e.g. by popping a balloon filled with synthetic air next to the sonic anemometer and the air intake). Alternatively, the lag time can be determined AFTER obtaining the transfer function for the gas power spectrum and applying the corresponding low-pass filter to produce a degraded temperature spectrum: the correct lag time should be that which maximises the cross-correlation between the degraded temperature time series and the gas time series.
Determining the lag time with any of these methods, followed by the correct cospectral correction (1), should be the preferred procedure.

It is understandable that the authors wish to promote their novel correction, and I concede that it may be useful in some cases, especially where lag time determination with other methods is difficult or not possible any more (historical data). A modified conclusion to that effect would be acceptable.

However, the authors should not recommend reverting, after covariance maximisation, to using the square root of the power-spectral transfer function. That approach reminds me of the Copernican approach of retaining epicycles in the planetary orbits, in order to rescue the tenet of circular motion: a physically wrong and complicated correction of a calculation procedure that is necessary only because the original procedure is based on a flawed assumption. It may be reasonably accurate (as for the data used here) but nonetheless should be abandoned.

Specific comments

Introduction and Theory as far as P 5 L 18 are very well written and I fully agree with the content. One minor point:
In P 3 L 22, the "^" notation is introduced prematurely and should be removed. Its introduction is repeated in P 4 L 15, and it is not used in any numbered equation until (9).

P 5 L 20-32
I contend that the statement "Co_m can be approximated by A_m" (L 20-21) is wrong. Covariance maximisation delivers a lag time t_used which differs from the true t_phys. The mathematical treatment to describe how the time series of w and X shifted by t_used combine with each other is completely analogous to Eqs (2) to (4) with phi_used replacing phi_phys. Eq (4) shows how there is always a frequency-dependent shifting of amplitude between Co_m and Q_m. In other words, it is not possible to make Q_m disappear for all f simultaneously. Hence, while Eq (10) is the correct result for A_m, it is incorrect to equate A_m with maximised Co_m. In fact, your statement on P 12 L 3-5 shows that you are aware of this, hence the text here should be revised to reflect that.
The paragraph starting in L 27 describes your procedure for estimating H_p. It does not stop with assuming H_p = 1/sqrt(H). This is actually the novel part of the Theory section, and it should be written out with numbered equations and clearly stated approximations, ending with the equation for H_p that you are using in practice (e.g. Fig. 4).

P 6 top
In my view, the Theory section should not end here yet because you did actually take the analysis further, in Sections 4.1-4.2. It should at least be anticipated here that empirical analysis was used to get a better estimate of H_p.

P 8 L 15-18 Two comments: Firstly, "Method 1... is implemented e.g. in EddyPro after Hunt et al. (2016)": this is incorrect. The method CAN be implemented there, but does not have to. The user is free to choose which lag time determination is used, and Hunt et al. used a fixed lag time.
Secondly, if EddyPro is mentioned here, then it would be worthwhile clarifying that older versions implemented the "sqrt(H)" transfer function, while later versions implement the correct "H" transfer function. The change was made as a consequence of correspondence between Laubach (Hunt's co-author) and Fratini, as explained in a footnote of Hunt et al. (2016). So, for carrying out Method 3 with EddyPro, users would need to employ an older version.

P 8 L 19-20 "Throughout the study, cross-covariance maximisation was used...": that means that later when assessing CO2 and H2O flux data, no true reference (with correct lag time) is available - which is a pity!
"... as typically done...": I would be curious to know how widespread this practice really is (given the practical problems with small fluxes, when estimated lag times can be all over the place). Of course that is outside the scope of this paper. I'd just like to caution the

authors that not every EC user does follow this practice, and as noted above, the EddyPro software offers alternative choices.

P 9ff
I find Section 4.1 very hard to follow. First, C is frequency-dependent (Fig. 1 top), then it is set constant without clear motivation (Fig. 1 bottom), then wind-speed dependent (Fig. 2). Is it possible to rewrite this section to be less ad-hoc and with more rigour, and perhaps put relevant equations at the end of the Theory section (with a note that empirical coefficients will be determined in the Results)?

P 11 L 11 "as is typically done": cautionary note that this is an assumption about other users (as for P 8 already). Figure 3 is a very convincing illustration why the covariance-maximisation practice should be abandoned, and I'd love to see a statement to that effect!

P 12 L 2-5
"Q_m cannot be nullified": here the authors agree with my earlier comment. The sentence in L 3-5 should be moved to the Theory, below Eq (10).
"H H_p not exactly equal to sqrt(H)": in fact, the appearance of negative values means that the two expressions are fundamentally incompatible. As Fig. (4) shows, increasing the time lag has the effect of shifting the negative region towards lower frequencies, where it causes greater flux losses. The approximation as sqrt(H) then becomes inadequate quite quickly, with big effects in practice for "sticky" gases like H2O and NH3, where tau can easily exceed 1 second.
Since you actually have a method to compute H_p, with results shown in Fig. 4, I do not understand why you keep repeating the point about (inaccurate) resemblence to sqrt(H).

P 13 L 1 "somewhat underestimated CF": better quantify (about 5 %?)

P 13 L 9-10, Fig. 5: I do not find Fig. 5c useful. The dependence of t_lpf on tau has been extensively covered in Section 4.1.

P 14, Table 2: How come that Methods 2 and 3 have relative differences of order +/-1 % when Fig. 5a suggests correction factors about 5 % below the reference? Does that mean the largest fluxes systematically had the smallest corrections? Does the extended dataset behave differently to the smaller sample underlying Fig. 5? Some explanation of this is needed.

P 15, Fig. 6: Why does Hyytiälä not show any negative values for Methods 2 and 3? Was the chosen tau range too small to simulate noticeable flux losses?

P 17-18
Figs 7 and 8 are interesting. Unfortunately, as stated before, there is no true reference available because t_phys was not determined with any other method than covariance maximisation.
It may be useful in these figures (and perhaps already in Fig 4, too) to show some averaged or typical cospectrum in a second panel above or below the transfer functions, to give the reader an idea which cospectral regions were affected by the low-pass filtering. It seems that with the data used here it was a relatively minor part, hence flux losses were generally small. While this situation is highly desirable (meaning the experimental setup was near-optimal), it is not always achievable, and a different dataset with greater flux losses may lead to comparison statistics quite different to those in Tables 2 and 3. The last few lines on P 18 already hint towards that; perhaps make this discussion point a little bit stronger. This suggestion is based on my own experience at agricultural sites with mast height restrictions (2 m) to allow for irrigators moving overhead. There, particularly for H2O the flux corrections can be substantial, of order 30 to 50 %, in which case the correct shape of the transfer functions matters a lot more than in your datasets.

P 20-22:
It would help the reader if the "Summary and Conclusions" section was shortened greatly, to "Conclusions" only, with clear recommendations on future data processing (less is more!).
Of the listed conclusions, the first should end with a recommendation to abandon the covariance maximisation method wherever possible (P 20 L 31).
The second (P 21 top) should be shortened to its first 3-and-a-half lines ("... caused by covariance maximisation"). The statement that "(H H_p) can be approximated with sqrt(H)" should be removed because it becomes highly questionable when the negative region of the transfer function reaches into lower, flux-carrying, frequencies. The content of L 5-9 is unnecessary repetition of points made in Section 4.4.
The third (P 21 L 10-17) does not lead to a clear recommendation (other than reinforcing that covariance maximisation is best avoided), so I suggest removing. The fourth (L 18-22) only repeats the second, remove. Instead, consider adding a recommendation to check whether past Fluxnet datasets have been processed with a consistent method combination for determining lag time and transfer functions. Where an erroneous mix has been applied, the data should be reprocessed. A statement on the expected fractional changes from such reprocessing could be added (based on your Results, but somewhat speculatively with respect to other EC setups).
The last conclusion (L 23 to end) could be condensed and rephrased as an "outlook" towards which other aspects of cospectral corrections require further investigation.

Minor technical comments

P 2 L 10 "trough" should be "through" (before "tubes")

P 2 L 15-19 (and possibly later): For easy reading, I suggest using one of the pairs "low-

pass"/"high-pass" and "high-frequency/low-frequency", but not mixing the two.

P 6 L 11 & 12 It is usual practice to give town/city of the manufacturer, too, not just the country.

P 6 L 17 remove hyphen between "Sphagnum" and "species".

P 6 L 18 replace "with the height" with "with a height".

P 8 L 6 insert "to" between "prior" and "utilising".

P 18 Table 3 caption: "was used" should be "were used".