Manuscript title: The high frequency response correction of eddy covariance fluxes. Part 2: the empirical approach and its interdependence with the time-lag estimation

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Second review round

We thank the referee and the editor for their comments. Please find below point-by-point responses to the presented critique. Responses to the comments are in red and the corresponding changes to the manuscript are in blue.

Associate Editor

Comments to the Author:
Please consider the supplementary minor comments provided by Referee #2. These arguments are partly philosophical, but the referee does provide a sound mathematical argument for the pitfalls of covariance maximization.

In my own previous work, I have typically averaged all lag-covariance plots together to derive a single optimum lag for a species and then applied this across all EC calculations. This assumes that the lag is driven by invariant physical aspects of the experiment and avoids the bias issues mentioned by the reviewer; however, I expect that the "right" approach depends on the particular experiment.

I will leave it to the authors to decide whether or how to incorporate the Referee's comments. Science is a dialogue, and publications are an appropriate venue to carry on such debates.

RESPONSE: We thank the editor for his comments. We agree that further research is needed for finding a suitable approach for time lag estimation but argue that this is out of scope of the present article, whose focus is on spectral correction.

Referee #2: Johannes Laubach

General comments

The authors have certainly addressed all the specific criticisms and by that improved the manuscript. Also, I do not have the impression that there are any differences between authors and reviewers on the mathematics.

That said, I would still encourage the authors to make stronger recommendations for change to "established" or "widespread" eddy flux processing procedures. I will elaborate more in the paragraphs below. I believe such strengthening of the messages would increase the impact of the paper and would be fruitful for the "flux community".

There seems to be some remaining disagreement between me and the authors on the relative merits of the cross-covariance maximisation method. While I would call it "dubious" and "flawed", the authors still describe it as a "practical solution". I do not doubt that the authors have understood my line of reasoning, so it comes down to a matter of opinion what degree of known imperfection is still considered acceptable. Ultimately it is the authors' call how to put this. I can only appeal to them to consider a more strongly critical wording, on the following grounds.

I have many years of experience with EC measurements with closed-path analysers near the ground, i.e. 2 m above agricultural surfaces. There, lag times are typically of order 0.3 to 3 s, and I have always found the cross-covariance maximisation method causing more trouble than a fixed lag time, even a somewhat inaccurate one, ever would. For all gases I've looked at (H_2O, CO_2, CH_4, N_2O, NH_3), covariance maximisation introduces frequent unrealistic, widely oscillating, lag time estimates that have nothing to do with instabilities in the sampling system. The reason for that is that correlation coefficients of w with a scalar variable are not huge to begin with (typically 0.2 to 0.4) and when one of the variables is attenuated, the peak of the correlation function becomes rather flat and thus hard to detect. Worse, because it is a "maximisation" procedure, the method selects against near-zero fluxes even when these are true. For example, measuring N_2O fluxes after a long period without any nitrogen inputs to the soil should yield a single-peak flux histogram with a near-zero median. However, with the maximisation method, the histogram becomes double-peaked (one positive, one negative) because zero flux is actively avoided by the method. But this is not a reflection of separate source and sink processes, it is simply due to bias in the individual flux estimates (because random non-zero correlations, of either sign, get "detected" and locked on by the method). Gas fluxes that change sign twice daily, such as that of CO_2, are equally affected by this bias around the sign-change periods.

I would concede that cross-covariance maximisation can be useful as a diagnostic tool, because it allows early detection of clear trends (such as two clocks drifting apart). But still, actual flux values should be calculated with a lag time that is based on the technical flow parameters of the system and not automatically varying from run to run (apart from corrections for known clock drifts).

Hence, my opinion remains that the use of the cross-covariance maximisation method (in its present widespread form) should be discouraged, and if the present authors do not include a recommendation to this effect, they are missing a chance to influence the thinking of the "flux community" in this regard.

The following remarks are deliberately provocative. Please do not take them as personal criticism!

Can the authors perhaps ask themselves why they are reluctant to recommend abandoning the cross-covariance maximisation method? The fact that it is "widespread" is no good reason. If a method is known to be poor, it should be replaced with better ones. In this case, do the authors consider such change too laborious for users? Do they not wish to make this recommendation because, if followed widely, it would remove the need to use their here-presented correction procedure in the future? Do they fear conflict in the scientist networks they are involved in (in particular ICOS with its drive to standardise procedures)? Do they worry about changes in the FLUXNET databases that could ensue from revised processing? I do not expect answers to these questions. They are merely intended to encourage the authors to be bolder in the paper's Discussion and Conclusions sections.

RESPONSE: We thank the referee for thoroughly reviewing our manuscript. We agree with the referee that ideally one would use the physical lag time to shift the time series and that cross-covariance maximization (CCM) is by no means a perfect approach (e.g. due to the issues raised by the reviewer (see also Langford et al., 2015) and due to the findings shown in this manuscript). However, we argue that accurate estimation of physical lag time (and its possible temporal changes) is not as simple as the referee suggest. Our analyses started from the assumption that CCM is used to shift the time series and we wanted to evaluate if this affects the frequency response corrections. The emphasis of our manuscript is on frequency response corrections, not on methods used to estimate the lag time. Hence, we argue that strong suggestions about abandoning CCM cannot be made based on our findings alone since there are other issues (e.g. the ones raised by the reviewer) related to the lag time determination that are not included in our manuscript. Such suggestions should be made in a separate paper focusing specifically on the lag time estimation methods.

Regarding the more "philosophical" comments from the reviewer: Scientists should always be ready to make a "paradigm shift" even thought it would mean a lot of additional work. That is how science moves forward. However, we argue that our study does not entail all the needed information for suggesting a paradigm shift for EC lag time estimation.

CHANGES: Added "(yet sometimes flawed (e.g. Langford et al.,2015))" after "practical" on P5L4. We modified the sentence "Hence, investigation of other means for estimating the signal travel time might be warranted" as "Hence, other means for estimating the physical time lag might be warranted, especially in the case of noisy measurements (Langford et al., 2015). Further research in this topic is required."

Specific comments (line numbers refer to the tracked-changes version)

P 5 L 23 and P 17 L 1

The authors have added information on flow and tube dimensions for Hyytiälä. The tube volume is so small that the total physical lag time is affected at a comparable rate by the time it takes to exchange the air in the volume of the Li-Cor measurement cell. In addition, there is a known processing lag if the LI-7550 Interface Unit is used. The three effects (tube, cell, processing) need to be added to estimate physical lag time.

RESPONSE: There's no discussion related to physical lag times on P5L23 and hence it is uncertain which part of the manuscript the reviewer is referring to.

CHANGES: Added that the processing lag needs to be considered to P17L1: "However, this calculation neglects the additional time lag caused by LI-7550 Interface Unit and hence can be assumed to slightly underestimate t_{phys}."

P 21 L 20-21

I believe the reader should be warned more clearly that the "approximation" is limited to the cases of relatively minor attenuation presented in this study. In particular, it should be noted that in cases where the phase effects cause sign reversals in the cospectrum, the sqrt(H) approach must fail.

CHANGES: Added the following sentence to P12L4: “Hence the approximation of HH_p with sqrt(H) will likely fail under very strong attenuation.”

P 21 L 32

In line with my General Comments, I find the inserted "Hence, investigating..." too weak. Why not say something like "Hence, other means for estimating the physical lag time should be used whenever possible." (Please do not use "signal travel time", that sounds like an electromagnetic phenomenon.)

CHANGES: We modified the sentence as: “Hence, other means for estimating the physical time lag might be warranted, especially in the case of noisy measurements (Langford et al., 2015). Further research on this topic is required.”

References:

Langford, B., Acton, W., Ammann, C., Valach, A. and Nemitz, E.: Eddy-covariance data with low signal-to-noise ratio: time-lag determination, uncertainties and limit of detection, Atmos. Meas. Tech., 8(10), 4197–4213, doi:10.5194/amt-8-4197-2015, 2015.