Manuscript: Title: The high frequency response correction of eddy covariance fluxes. Part 1: an experimental approach for analysing noisy measurements of small fluxes

Author(s): Toprak Aslan et al.

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Reviewer's comments are shown in **bold black**, while authors' responses are in red.

Reviewer 1 # Marc Aubinet

General comments

This paper is the first of two papers discussing different spectral corrections procedures for low pass filtering effects in eddy covariance systems. I read and reviewed both and found the present paper difficult to understand without reading the second (Peltola et al, also on AMT discussions). I thus recommend the authors to change the paper order and put the Peltola paper, which better stands alone, in first position and this one in second. I also made my reviews in this order and, for people who would be interested, I also recommend to read my review of the Peltola paper before this one.

This paper addresses two questions related to low pass frequency corrections for eddy covariance systems: one is the problem of power spectra contamination by white noise and the resulting difficulty to determine transfer function time constants when using the spectral approach (PSA); the second is the impact of transfer function shape and of time lag when using a cospectral approach (CSA).

Having never applied the PSA on low signal to noise ratios personally, I have no direct experience of the impact of noise on power spectra but I can imagine that this problem may be critical under low SNR. The method proposed by the authors seems to provide more accurate and less dispersed time constant estimates than the classical method proposed by Ibrom et al. 2007 (I suppose that Andreas supports this, as he is co-author of the paper). However, I didn't find the method very convincing as flux estimates obtained with this new approach did not appear much more accurate (Figure 8).

In addition, I wonder about the feasibility of applying routinely this approach on "real world data" as the fit has to provide two parameters which could create convergence problems.

Above all, this makes me again wonder why one persists to follow the PSA while the CSA approach is not affected by noise.
I'm much more reluctant about the second comparison. The authors apply the CSA and compare three approaches, two based on non-synchronized cospectra and using either a Lorentzian curve or its square root and the third based on synchronized (time lag adjusted) cospectra using the square root of a Lorentzian. I was first puzzled by the use of non-synchronised cospectra, that appears a priori nonsense. In the present case, anyway, as the original (not attenuated) time series are not lagged, I suppose that the first approach (CSAH) could be thought as an application of the classical Lorentzian transfer function

on a set on which the time lag introduced by signal attenuation would have been ignored. The third one (CSA sqrt(H), sync) would then be those taking both attenuation and its derived time lag into account, following an approach described in the Peltola paper (Method 2). This comparison would then show that ignoring the time lag due to low pass filtering would lead to a time constant underestimation. If my interpretation is correct, I think that it should at least be explained by the authours.

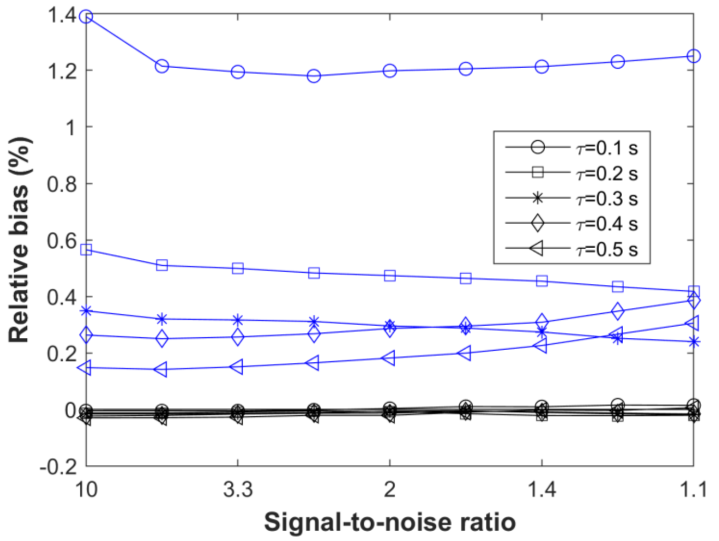
Besides this, I don't see the interest of the second approach (CSA sqrt(H)) as it does not correspond to any used methodology. Contrary to what the authors suggest (P5L21), this comparison does not address the debate on transfer function shape: indeed the real question, very well synthesized by Peltola, was to determine, in the PSA, which transfer function should be applied on cospectra: the function itself or its square root (with the same time constant). This is not what was tested here as the Lorentzian and its square root were separately adjusted on the same data set resulting in a quite trivial result, i.e. the time constant of the CSAsqrt(H) is about twice those of the CSAH. An approach fitting a Lorentzian on synchronised time (CSA H, sync) would probably be more relevant as it would mimic the Method 1 of Peltola presently (and erroneously) recommended by the ICOS protocols. Anyway, I think that these comparisons are of limited interest as they overlap with results of Peltola.

Finally, I found both analyses (as well on PSA as on CSA) too much focused on time constants, which are not an objective per se when applying spectral correction procedures, and not enough on correction factors (not presented in the study) or fluxes. Correction factors are not presented and only cumulated fluxes are presented and (too) shortly discussed. I think that the real efficiency of the approaches can better be evaluated by looking half hourly fluxes and I suggest the authors to look at the regressions between half hourly fluxes obtained with the different approaches.

In the whole, the paper presents some promising results but some of the proposed comparisons are not relevant to my opinion and some analyses are insufficiently developed and not enough focused on the real product of the spectral correction, i.e., the correction factors and the fluxes.

Besides this, the paper is generally well written and presented but there are still some presentation problems that I point in the specific comments below. In conclusion, I think that the paper needs a major revision before publication.

- General Response: We thank the referee for the comments. The hypothesis of the present study was "the success of the PSA and CSA usage in frequency response corrections depends on the attenuation condition and the level of signal-to-noise ratio", which was successfully investigated and concluded. Indeed, the study mainly focused on time constant, and cannot provide holistic understanding on spectral correction as highlighted by the referee. This is partly due to the lack of a robust method for calculating correction factors as shown by the companion paper (Peltola et al. 2021). As a result, the effect of variations in time constant estimations cannot be clearly seen in correction factor calculations. In other words, the methods used for time constant estimation does not have a significant effect on corrections factors, hence final fluxes. In the analysis of the originally submitted manuscript, the correction factors used to correct the artificially attenuated fluxes were calculated following the Fratini et al. (2012) approach and compared with the unattenuated fluxes. When using such an approach, the reference fluxes were not fully comparable with the corrected fluxes due to the low-pass filtering related phase shift effects, as shown in the companion paper (Peltola et al., 2021). That prevents showing the sole effect of the deviation in time constant estimation on correction factors and fluxes. Thus, we changed the calculation of reference fluxes, so that it would be calculated with Eq. 11 similar to other methods. For this, the time constants used for low-pass filtering is implemented in H_{emp}. It is worth noting that with this change the variation in correction factors would solely reflect the variation in time constant. That enables a good statistical comparison, however does not reflect the shortcomings of the method, Fratini et al. (2012) when used in real life data. The relevant results are shown below (Figs. 1, 2 and 3). These findings are consistent with the observed biases on time constant estimation, meaning that where the time constant and the low-pass filtering were overestimated (e.g. with the PSA_{I07} especially for tau=0.1 s), the spectral correction factor and thus the fluxes were overestimated, too. The correction factors of a case with tau=0.3 s and SNR=2 is also shown below (Fig. 3). We added the figure to the manuscript.



- Fig. 1 Relative biases of the cumulative fluxes derived with the various approaches compared with the reference flux as a function of SNR for different attenuation time scales (0.1-0.5 s), for PSA_{I07} (blue) and PSA_{A20} (black), calculated as, e.g., $100(F_{PSAI07} - F_{REF})/ F_{REF}$.

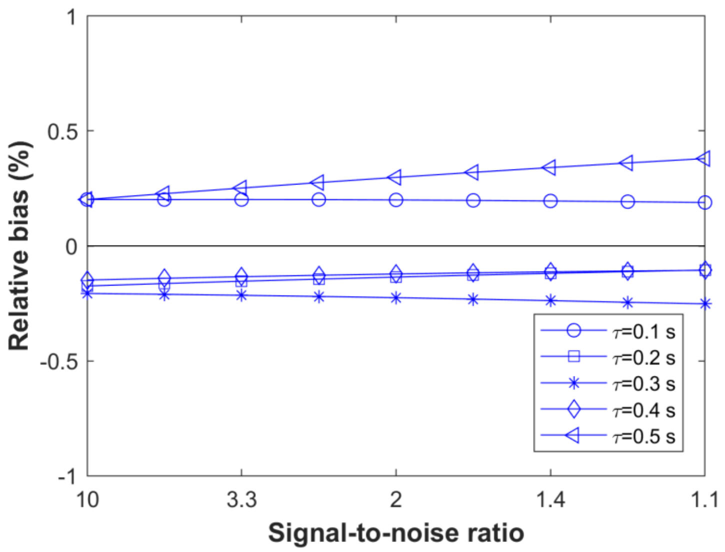


Fig. 2 Relative biases of the cumulative fluxes derived with the $CSA_{sqrt(H),sync}$ compared with the reference flux as a function of SNR for different attenuation time scales (0.1-0.5 s), calculated as, e.g., $100(F_{CSAsqrt(H),sync} - F_{REF})/ F_{REF}$.

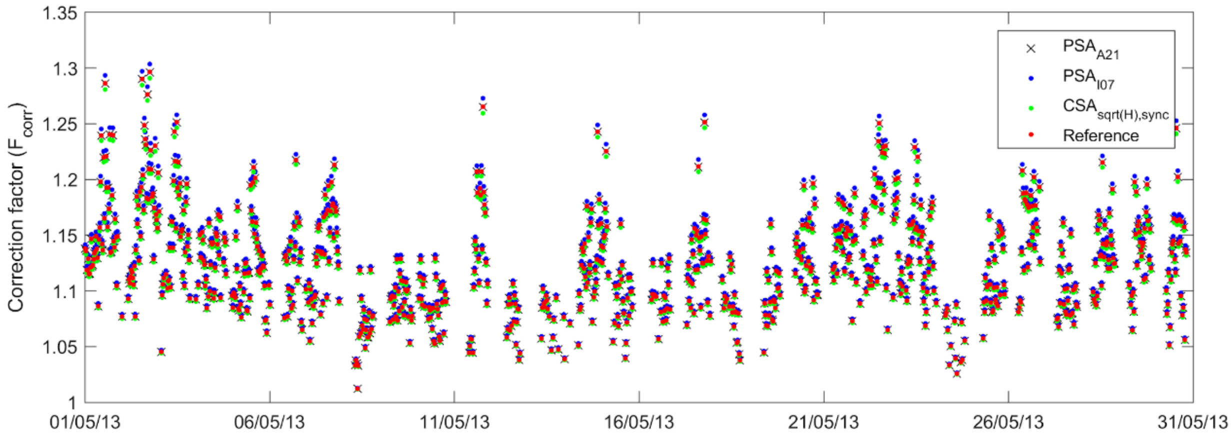


Fig 3. Correction factors (F_{corr}) of half-hourly fluxes calculated with different approaches, i.e. PSA$_{A21}$ (black cross), PSA$_{I07}$ (blue), CSA$_{sqrt(H),sync}$ (green) and the reference (red) for the case with tau=0.3 s and SNR of two.

- Regarding using the CSA variations without time-lag corrections, following the referee's suggestion we removed the CSA$_H$ and CSA$_{sqrt(H)}$ as it causes confusion and does not provide any contribution to the discussion in the literature. CSA$_{sqrt(H), sync}$ is the only CSA method used in the study. Additionally, we prefer not implementing CSA$_{H, sync}$ as suggested by the referee to prevent overlapping the findings in the companion paper. Moreover, we removed the Appendix B. "Assessment of the effect of quadrature spectrum on time constant estimation in the CSA" and the relevant info in the Section 4.2. "Time constant estimation with CSA" from the manuscript as it overlaps with the theoretical explanation of the phase shift effect in the companion paper.
- After these changes, in order to provide better flow, we changed the order of the companion papers as suggested by the referee.
- A concern related to the applicability of the new PSA (PSA$_{A20}$) method in real-world data is raised by the both referees. Thus, in order to demonstrate the performance of the PSA$_{A20}$ and compare it with PSA$_{I07}$ and CSA$_{sqrt(H), sync}$, we processed a real-world data, i.e. CO_2 fluxes from Siikaneva peatland site, for time constant calculation and added the results in the revised manuscript.

Specific comments

P2L11 and P4L13: In the frequency space these operations are not convolutions but multiplications (a convolution in the time space corresponds to a multiplication in the frequency space and conversely).

- Response 1: The term "convolution" is removed from the manuscript at P2L11, and replaced with the term "multiplication" at P4L13.

P3L10: suppress the "be"

- Response 2: Done.

P3L11: Is this really critical? As the time lag is mainly determined by the set up, it could be determined on periods of larger flux and extrapolated. On the other hand, it's true that for small SNR, a time lag estimated by covariance maximisation, would systematically select the time lag associated to the highest flux and would not necessarily correspond to a "physical" maximum, which could lead to bias small fluxes.

- Response 3: We agree with the referee, but argue that this issue is worth mentioning in this context due to its various implications on flux estimates, as discussed in Langford et al. (2015). Estimation of time lags during periods when the fluxes are large and then extrapolating to low flux periods works (see for example Rannik et al. (2015)) but naturally only if high fluxes are observed during the measurement period.

P3L13: Yes but the paper showed that the impact on correction factors and fluxes was not critical if adequately accounted.

- Response 4: This is explained in the general response above.

P5L21: See my general comments above. I think that the proposed experiment does not bring any relevant argument to this debate.

- Response 5: This is related to the implementing CSA_H and $CSA_{sqrt(H)}$, which are removed from the manuscript. Please see the general response above.

P5L29 and foll.: You also point below (P9L21) that, in the Ibrom procedure, the boundaries for regression fitting are fixed by eye. This should be specified here as you consider this as a limit of the method.

- Response 6: We thank the referee for the comment. We specified the shortcomings of the PSA_{I07} in Section 2.2.

P6L9: What does mean a "y axis intercept" in a log scale? In your case, this axis does even not appear in the figure!

- Response 7: Due to this comment we tried to clarify the text and rewrote this part of the manuscript as "where b is the ratio between noise variance and variance used to normalize the \chi power spectrum" (see Appendix A).

P7L21-24: I had difficulties to understand this paragraph but the problem is maybe simply that you should refer to Eq (7) rather than Eq (4) on L24.

- Response 8: Thank you for the correction. As you explained, there is a mistake with equation numbers. We now refer to Eq (7).

P7L32-P8L2: See general comments above.

- Response 9: This is again related to the implementing CSA_H and $CSA_{sqrt(H)}$, which are removed from the manuscript. Please see the general response above.

P9L17-23: This is somewhat a repetition of denoising description above (P5L29 and foll.) but only the I07 method is described. What about the A20 method? Clarify and avoid overlapping.

- Response 10: The referee is right about the repetition of noise removal procedure. Thus, we removed the Section 3.2.3 "Noise removal". We moved the important information about the frequency range for noise removal procedure to Section 4.1 "Time constant estimation with PSA".
- More details about the procedures of PSA_{A20} are now given in Section 2.2., and the schematic figure (Fig 1.) is enhanced accordingly.

P12 Fig 4, P15 Fig 6: Spectra and cospectra were presented in function of the real frequency. Did you take the possible cospectral shift with wind velocity when taking ensemble averages?

- Response 11: No we did not. It is hard to determine the peak of the individual cospectras due to their shapes. Furthermore, the aim of these figures is to show the difference between attenuated and not attenuated (co-)spectra and wind speed does not alter that difference (as the transfer function calculated with Eq. 3 in the manuscript does not depend on wind speed).

P13Fig5: Red line and shaded area are confounded.

- Response 12: Indeed, the red line is not clearly seen. Thus, we changed the color of the line representing the median from red to black.

P14L10: the term "raw" is maybe not very well adapted here as it represents in fact the ideal cospectrum, without attenuation and noise.

- Response 13: We made the necessary changes via replacing "raw" with "unattenuated and noise free".

P14L10: Was the cospectrum based on synchronized time series or not?

- Response 14: Yes, it was synchronized. We made it clear via modifying the sentence as follows, "...illustrates **the time-lag corrected** cospectra of three low-pass filtered cases…".

P14L10: It is in fact not so evident from the figure that noise contamination does not affect the cospectra shape. Do you refer to the fact that no linear increase is observed at high frequency? This could be specified.

- Response 15: Here we refer to no significant deviations that would affect the time constant estimations. We replaced the sentence "It shows that noise contamination does not affect the shape of the cospectra" with "It shows that the white noise contamination did not cause linear increase in the high-frequency end of the cospectra, enabling the time-constant calculation without additional procedure related to noise removal".

P14L12 and foll : See my general comment.

- Response 16: The CSA approaches without time-lag correction referred here are removed from the manuscript as stated in Response 5 and general response above.

P14L27: Yes but this is a problem that affects time constant determination but not correction factors and fluxes, as shown by Peltola.

- Response 17: Please see the general response above.

P15L2: As I also pointed in my review of the Peltola paper, I'm not convinced by the use of relative errors on cumulated fluxes to evaluate the performance of correction methods. On one hand, relative errors are often not informative (the relative error on a zero flux would be infinite !), on the other hand computing errors on cumulated fluxes only would hinder error compensation (between night and day, for example). I prefer a comparison between half hourly fluxes (by taking the regression slope, for example).

- Response 18: We tried showing the biases by comparing the half-hourly fluxes corrected with different approaches with the reference fluxes. An example of such comparison for a specific case (with tau=0.3s and SNR=2) is shown below (Fig 4). Since the biases are very small, the differences are not visible when showing them in a scatter diagram, hence the statistics of the regression analysis are not informative as well. In the Fig. 4 below, the reference line and the regression line are confounded due to similarity and the regressions slopes are 0.9856, 1.0001 and 0.9979, respectively. Indeed, the relative bias is not the best way for the comparison, but we believe that it is the only representative method for our findings, thus we would like to use it in the manuscript.

Note also that as the spectral corrections are multiplicative (not additive) they do not e.g. change the sign of the flux and hence showing their performance in a relative sense is natural.

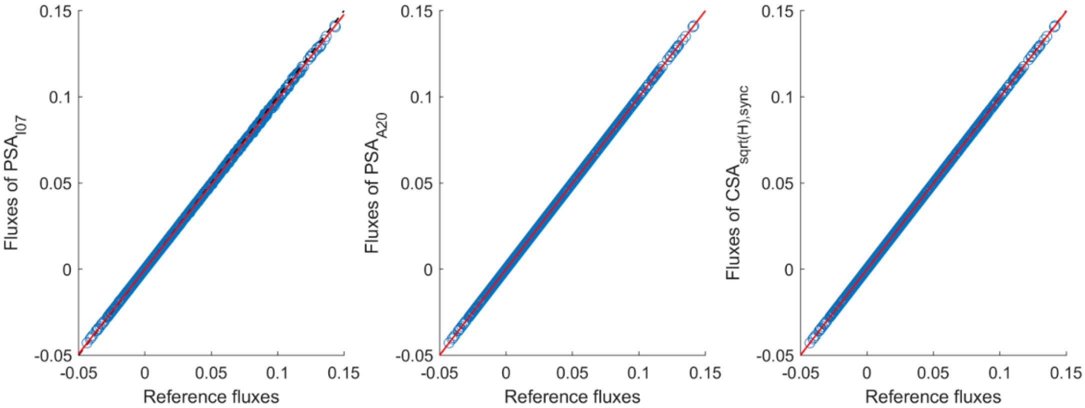


Fig. 4 Comparison of half-hourly fluxes calculated with different approaches (i.e.PSA_{I07}, PSA_{A20} and $CSA_{sqrt(H), sync}$, respectively) with the reference fluxes. The reference 1:1 line is shown with a dashed-black line, while the regression line is shown with solid-red line.

P15L5: In view of Figure 8, it is not clear to me that PSA20 is more accurate than PSI07. I just see that it underestimates systematically the flux value but there is no clue that the bias is smaller.

- Response 19: We now updated the Figure 8 and 9 (see general response above).

P15L6: Very close (<1%) is only the case for the low time constants (0.1 and 0.2 s). For higher time constants, it anyway reaches 2%.

- Response 20: We changed the calculation of the correction factors for the reference fluxes, hence the relevant figures (8 and 9) are updated. Please see the general response above.

References

Langford, B., Acton, W., Ammann, C., Valach, A., & Nemitz, E. (2015). Eddy-covariance data with low signal-to-noise ratio: time-lag determination, uncertainties and limit of detection. Atmospheric Measurement Techniques, 8(10), 4197-4213.

Rannik, Ü., Haapanala, S., Shurpali, N. J., Mammarella, I., Lind, S., Hyvönen, N., ... & Vesala, T. (2015). Intercomparison of fast response commercial gas analysers for nitrous oxide flux measurements under field conditions. Biogeosciences, 12(2), 415-432.