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Comment on amt-2020-479

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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-RC1, 2021

General comments

This paper discusses different spectral corrections procedures for low pass filtering effects in eddy covariance systems. Despite eddy covariance has become the most common approach to determine, among others, CO2, water vapour or greenhouse gas budgets of ecosystems, the method still suffers from uncertainties due to random and, more worryingly to my opinion, to systematic errors. From this point of view each study providing a better understanding of measurements errors and improving correction procedure is welcome.

In that respect, the paper by Peltoli et al is important for at least two reasons: first it points a systematic error actually made by some eddy covariance data treatment softwares (including EddyPro, cf Sabbatini et al., 2018) which definitely requires a correction; secondly it clarifies the question of the cospectra transfer function shape and reconcile theory and observations. It also provides a new method to correct low pass filtering effects but I think that it's robustness and applicability to routine measurements needs still to be proven.

For these reasons, I think that the paper deserves publication. As it is generally well written and structured, I think that only minor revision is required before acceptation.

I would add that this study comforts me in the opinion that, despite the great interest of theoretical studies that help understanding the causes and modalities of low pass filtering by eddy covariance systems, empirical approaches relying as little as possible on theoretical hypotheses remain the most robust ones to apply frequency corrections on routine measurements. In particular, in the present study, the approach deducing a transfer function from cospectra rather than from power spectra (Method 2) remains one of the most robust. The fact that the shape of the transfer function and the time constant are not exact is not very problematic to my opinion, as it does not affect critically the values of the correction factor, which is the target. The Method 4 proposed by the authours could be an interesting alternative, as it also relies on cospectra but uses a different transfer function shape. However, it is more complex as it requires the determination of two parameters (against one for Method 2) and, if the method worked well in the present case where the high frequency attenuation was artificially introduced, I suspect (and they confirm on P16L9) that disentangling the two time constants could be sometimes difficult, even impossible.

My regret is that the authors do not detail an implementation procedure of Method 4 for routine measurements.

Specific comments

The paper is the second of a series of two papers on spectral corrections. I was first asked to review the first of them (Aslan et al, also available on AMT discussions) but had to wait the submission of this one to really understand some issues and methodical choices of the Aslan paper. As the present paper appears to me more "standing alone", I suggest to place this one in the first place and the Aslan paper in the second place. This is the order I followed for my reviews.

Introduction

The introduction offers a review of the knowledge about spectral corrections. It is clear and highlights the most important points. I have no specific comment about this except two small remarks :

P2L13: I think there's a typo ("contribute" rather than "contributed")

P3L7: Reference to Aubinet is not relevant I think as it refers to the chapter "night flux correction" in my book. I suggest to rather refer to the book itself or to a specific chapter (time lag is evoked in Ch 2 – Munger et al., 2012; Ch 3 – Rebmann et al., 2012 and Ch 4 – Foken et al., 2012).

Theory

I liked this chapter as it helps me to understand the issue of the cospectral transfer function shape. I must say honestly that I overlooked the debate about the presence or not of a square root in the cospectral transfer function shape (for my defense, I was more concerned in the past by the cospectral – Method 2 - approach than by the spectral approach – Method 3) but, when comparing recently spectral and cospectral approaches on crop sites data, I found a better agreement when applying the square root (Method 3) than not (Method 1), which contradicted the theoretical predictions by Horst (1997), among others. I thus found the explanations given by this paper clever and convincing.

Two remarks, anyway:

P5L27-P6L2: I don't see the interest of presenting the approximation on L29. I tested the equation on L29 and found it fitted quite loosely equation 6. In addition, as I understand, this equation was not used in the paper and equality between tlpf and tau was not assumed further. Maybe could you consider to skip this.

P5L29: I'm wondering about the equality (and below, the proportionality) between tlpf and tau. Indeed, these two time constants are a priori not physically linked (except when both result from tube attenuation, which is of course an important case) and I'm wondering if you don't loose generality by introducing this dependency. This question is discussed below but, in the end, there is no clear description on how you really implement the transfer function computation: do you fit an equation for H Hp based on equations (5) and (6) ? On equation (5) and those of P5L29? Do you consider tau and tlpf as independent parameters or do you relate them in some way?

Material and methods:

No specific comments. Clear and well presented. It is important to keep in mind (Sect 3.2.2) that the results presented below are not based on real measurements (I mean the attenuation is artificially provoked and does not reflect real attenuation processes), which is a limitation of the study (but this is well stated in the discussion).

Results:

P9L13-24: Same remark as above: the proportionality between tau and tlps is clear here as both time constant result from an artificial attenuation but how would this relation look like in the case of measurements with a real attenuation and a real time lag, possibly independent ?

P9L25-27: I was not sure to understand well: is it an approach that mimics the covariance maximisation procedure? If yes, it could be worth specifying it explicitly.

P10L2: What's the meaning of s in Eq 14 (second, I suppose, but I would not mix symbols and units in a formula).

P10L3-4: This sentence let me hunger. As high attenuation could occur often (especially for gases other than CO_2) this question should be clarified. Which attenuation levels do you consider? what is the order of magnitude of the bias? what is the impact of this bias on the next steps (correction factor estimation)?

P13 Fig 4: As I understand, the red curve corresponds to Method 1, the blue one to Method 3 and the black one to method 4. Is it correct? A direct reference to the method could facilitate figure reading (and why is method 2 absent from the figure?)

P14L5: isn't it rather by the ratio of cospectral peak frequency to the cut off frequency ?

P15Fig6: The legend is not fully clear. I suppose that the symbols refer to the sites and the colours to stability conditions. This should be stated more clearly.

P15Fig6: I'm intrigued by the curve of Hyytialla in unstable conditions for methods 2, 3 and 4. Why is the bias positive, contrary to other site/conditions? Can you comment on this?

I'm also intrigued by the fact that the Method 4 more overestimates the correction factor than Methods 2 and 3 (and thus seems to work less good) at Hyytialla in unstable conditions. Here also I would expect a comment.

P15L2-6: I think that the figure shows clearly that the Method 1 gives different results from the three other methods. To my opinion Methods 2, 3, 4 provide all reasonable estimates of the correction factors while Method 1 biases the correction factors due, as you showed in the theory section, to a misinterpretation of the theory. In this sense, giving a relation to quantify the bias introduced by Method 1 is maybe not very useful. It could appear more clearly that this method is wrong and should be definitely not recommended (which notably implies a modification of the ICOS protocol).

P16L33: Same remark as above: don't mix symbols and units in a formula.

P16L34: the meaning of x and y is not fully clear to me. Could you express the relation between these variables and time constants presented above?

P17L2, L5 and elsewhere: rather than referring to Section numbers, it would be more easy for the reader if you referred directly to the figures or tables presenting the results.

P17L7 and elsewhere: use a uniform notation to present the different methods ("Method X" is fine to me).

P17L9: one word is missing.

P17L10: As Hyytialla is equipped with a LI7200 and Siikaneva with a LI7000, I would have expected the inverse: a lower tau value at Hyytialla. Could you comment ?

P17L12 and foll: This section (and the legend of Table 3) should be clarified: In the text, are you presenting difference between correction factors? between half hourly fluxes? between cumulated fluxes? On which period? I finally supposed that you were comparing cumulated fluxes but this should be specified.

P17L12 and foll: I'm not convinced by the relevance of comparing relative differences on cumulated flux values. Indeed, relative values depend strongly on flux values (I suppose that H2O flux values at night should be low and in these conditions larger relative errors do not mean much). In addition, the low error on cumulated values may also result from partial compensation of errors (for example during day and night). I have the same problem when I try to compare different correction methods on my data set and I'm not sure to have the best solution. I prefer comparing the fluxes by looking at the slope between the fluxes submitted to different corrections. Anyway, in view of the preceding remarks, I'm not sure that the fact that Method 3 gives the biggest difference at both sites (L16) is really relevant.

P22L1; I feeled (of course!) concerned by the remark on our paper about the impact of dead volumes on the frequency response of gas sampling system. I could recognise that the fact that we didn't distinguish physical time lag from attenuation induced time lag led to cut off frequencies that are probably not really representative of the attenuation. However, the general decrease of the cut off frequency with increasing dead volumes (our Figures 5 and 6) and the need for reducing these volumes in the gas sampling system were important results that we showed in this paper, along those of Metzger et al. And this again reinforces my opinion that transfer functions based on observed cospectra and taking thus account of all attenuation processes affecting the system (even if in some cases we do not fully understand all of them) are to be preferred for routinely correcting measurements, as they provide more robust estimates of fluxes.