**Atmospheric
Chemistry
and Physics**
Discussions

Interactive comment on "Estimating local atmosphere-surface fluxes using eddy covariance and numerical Ogive optimization" *by* J. Sievers et al.

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REFEREE # 2

In my opinion the work is interesting however I am not as convinced (as the authors are) that this method 1) only filters out advective contribution 2) correctly estimates fluxes from situations when traditional EC methodology fails.

1) "Disturbances" or irregularities (in comparison with model (ideal shapes) in the low frequency range of the spectrum is in this manuscript treated as advection. I view this as an over-interpretation. These might alternatively be due to other causes such as

mesoscale motions which might (or might not) be undersampled for a given time period or intermittent turbulence, i.e. during stable conditions. During very stable conditions a large part of the flux can occur in such intermittent events which this method would filter out. Thus, we shouldn't "fool" ourselves by claiming the method to only filter out advection, sometimes it does perhaps but not always.

AUTHOR: Focus in the paper has been shifted towards filtering out of the more general "low-frequency contributions" of which advection can be a component along with e.g. topographical forcing on the observed flow or large-scale meteorological phenomena, such as gravity waves, deep convection and large roll vortices.

2) Figure 8 is good example. What makes the authors so sure that the modelled Ogive is the actual "true" flux? Can you really claim that a 'true' flux exists during such a period? The spread among the members is significant and in 8B most members appear to indicate a negative flux yet the model suggest positive flux.

AUTHOR: The argument in question has been elaborated

I think one way to, at least partly, validate the method is to evaluate universal functions e.g. normalized standard deviations as functions of stability, or possibly also some bulk coefficients, where the fluxes and variances have been processed with the OO method.

I suggest the authors to either downplay their conclusions that the OO method only filters out advection (when it actually more generally filters out low frequency contributions) or make a stronger case that it actually does.

AUTHOR: Conclusions downplayed and focus shifted to a more general filtering of low-frequency contributions.

SPECIFIC COMMENTS:

P21389, line 12: suggest replacing "exchanges" with something more specific such as "vertical surface fluxes" or similar.

AUTHOR: Changed according to reviewer recommendation

P21389 l 15: So your conclusion is that you cannot measure small fluxes using the EC method without significant contribution by advection? I find this quite a strong statement

AUTHOR: The conclusion has been downplayed a bit.

P21389 l 23-27: long sentence, consider splitting.

AUTHOR: Changed according to reviewer recommendation

P 21389 l 24: you probably mean "approximations" not "estimates"

AUTHOR: "Estimates" refer to the number of flux estimates attainable by the use of the Ogive optimization method. "Estimates" changed to "flux estimates".

P21390 l 14: As I mentioned above, low-frequency fluctuations are not equivalent with advection.

AUTHOR: Changed according to reviewer recommendation

P21390 l 7: Suggest using a "e.g." before the Baldocchi reference

AUTHOR: Changed according to reviewer recommendation

P21392-21393 l 26-27, 1: on the dependence of co-spectra with stability. Atmospheric stability does not have to be determined from sensible heat flux. Bulk Ri number would be an alternative parameter.

AUTHOR: Any reference to this circular dependency has been removed throughout the paper.

P21394, eq. 1, a formality, the covariances (e.g. wt) are not defined in the text. Also 3 equations are presented and should be labeled separately (a, b, c)

AUTHOR: Changed according to reviewer recommendation

P21394 l 24, eq (1) is actually only describes a unit conversion. What you are referring to is if the covariances estimated by the EC method truly represent the vertical flux. Please correct this.

AUTHOR: Changed according to reviewer recommendation

P21395 l 22 why does a "convergence to an extremum" reflect a change in flux direction?

AUTHOR: The formulation has been removed, in favor of the subsequent sentence which more accurately described the intended point: "Such conditions may be understood as reflecting an equilibrium point between two conflicting flux contributions..."

P21396 l 1, please specify what divergence you are referring to.

AUTHOR: Sentence rephrased to: "Analogously points of abrupt divergence of the Ogive function, as opposed to convergence to an asymptote, may likewise be interpreted as associated with the onset of a second flux contribution."

P21396 l 4, Fig 1 I think you need to make a comment the area beneath the cospectral curve is not proportional to the total flux as you have chosen a log-log representation.

AUTHOR: Upper row of plots are log-log, lower row are semilog (i.e. y-axis is linear). Axis labels have been updated to clarify the matter.

P21396 l 12, in this general description I think it is fair to say that that also the turbulent flux may be positive or negative (not only positive) Additionally, as mentioned previously, the blue area is not due to advection only.

AUTHOR: Sentence rephrased to: "Both turbulent fluxes and the low-frequency contribution, shown in Fig. 1B as a blue region of Ogive-divergence relative to asymptotic convergence, may be positive or negative, though the former has been illustrated as positive here."

P21396 l 13, please specify the term Ogive divergence (it can easily be misinterpreted

as part of the OO method which is described later in the text).

AUTHOR: Reference to asymptotic convergence added for context.

P21396, l14, You state that "more typical : : : is the case (Fig 1c)". This seems to me express the authors' personal view but can you support this statement with any references?

AUTHOR: The plausibility of the case in question is implied by the unclear existence of a spectral gap, according to (Lee, et al., 2004). The wording "typical" has been removed and the reference has been added.

P21398 First paragraph, I think the description of how you define the "members" in the OO method needs to be a bit clearer. I appreciate that you use short mathematical notation but to simplify for the reader I suggest rephrasing this paragraph. A schematic figure illustrating the reasoning would be helpful.

AUTHOR: The paragraph has been simplified. No figure is added though.

Some specific questions (which might be due to my confusion): Is T1 and alpha describing the same periods? T1 is defines as "Averaging interval time" and alpha as "temporal resolution of flux estimates". Is "A" in the equation on P21398 line 3 equal to "alpha"? P21398 line 8: is "the minimum dataset length" equal to one of your described parameters (T1, alpha?). If so, it contradicts a previous statement where alpha was set to 5-15 min.

AUTHOR: Your confusion is warranted. Alpha was mistakenly used for T1. The matter has been corrected. All alphas have been renamed as T1.

P21398 l 20: Please explain why you specify the requirement non-static surfaces? To my knowledge the momentum fluxes are always negative, except perhaps in some special cases with fast moving waves.

AUTHOR: Statement removed

P21398 l 25, U should be defined as the horizontal along-wind component.

AUTHOR: Changed according to reviewer recommendation

P21399 first sentence: This sentence is missing something, please rephrase.

AUTHOR: Reference to the iterative bisectional algorithm added for context

P21399 lines 18-21: I find the sentence starting with "in this study: : :" hard to interpret, please clarify.

AUTHOR: Sentence rephrased

P21399 lines 24-26: These statements are simplistic. Tuning the averaging time can very well be sufficient in many cases. In some cases "a parameter controlling the lowfrequency contribution alone" might be helpful. I think you need to rephrase these two sentences.

AUTHOR: Sentence added: Note that both tuning the flux averaging time and subtracting a running mean from the observed signal may, in many cases, provide sufficient separation of turbulent fluxes and low-frequency contributions. Here we apply both to arrive at a more generally applicable approach.

P21400 line 2: I am not sure what is meant by "running mean resolution". Are you referring to different window sizes in a running mean filter which you combine with different averaging times? If I understand you correctly I would also use a phrase such as "data-set lengths" or similar instead of "averaging times".

AUTHOR: Sentence rephrased: "Consequently some combination of data-set length (averaging time) and running mean window size might allow. . ."

P21400 l 9: I suggest using "window" instead of "resolution".

AUTHOR: Changed according to reviewer recommendation throughout paper

P21400 line : please correct the capital S in "Sensible heat"

AUTHOR: Changed according to reviewer recommendation

P21400 l 9: Is it necessary to use a filter length of 1 second? This is a very strong filter and would in no case yield useful flux estimates. To save computational time, the minimum filter length could instead be set to a few minutes.

AUTHOR: In the context of a mass Ogive calculation, the Ogives resulting from an excessively strong filter will become outliers and thus irrelevant during the optimization of the model Ogive. Nevertheless, I agree, 1 sec is too strong. Minimum filter length changed to 1min.

P21400 l 19 and lines 22-23: I do not understand how this necessarily means that the period is influenced by advection. The large scale oscillations could equally well be large scale turbulent motions. I think you need to change your interpretation in this text. See also below for my comments regarding Fig. 2.

AUTHOR: The section has been reformulated according to the new figures, and the wording "large-scale motions" or "low-frequency motions" used instead.

P21400 line 25: Again, I think you need to downplay your interpretation. It is not clear from Fig. 2 that the OO method is necessary. The traditional Ogive appears to give a perfectly nice flux estimate, please rephrase.

AUTHOR: Rephrased to: Unfortunately not all Ogive density maps indicate as well defined fluxes as for Fig. 2. In such cases, answering the overall question of most likely flux requires the application of an Ogive model.

P21401, lines 2 and 7: frequency f is missing unit. Also, I the syntax for frequency is a bit unusual (5*10E-2 would be my choice instead of 10E-1.3).

AUTHOR: Changed according to reviewer recommendation

P21401, line 7: I think it would have been useful to compare the OO method with the spectral peak method for some cases.

AUTHOR: No changes made.

P21401, line 16: Strictly speaking, the cospectrum should have a slope of -4/3 if you are multiplying the spectral estimate with frequency (as you appear to be doing).

AUTHOR: Changed according to reviewer recommendation

P21403, I think you need to comment Fig 4 more in the text, guide the reader through it, otherwise I cannot a motivation to keep it. I suggest that you move some of your descriptive text in the figure caption to your running text. Are the green lines corresponding to the 18 black lines in B? Additionally, I think you should state something on what you base your subjective visual inspection on.

AUTHOR: Figure moved to Appendix where a more in-depth description is possible

P21403, line 15: It would be useful with a map showing the different sites. Also, please add references to publications which have used data from the different sites.

AUTHOR: Map (Fig. 4) and references added.

P21403, line 17: I interpret the "flux strength" as "CO_2 flux strength", please specify.

AUTHOR: All species' implied. "(Q_SENS, Q_LAT and F_CO2)" added

P21403, line 22: What is the distance to the lake from the tower? Please specify.

AUTHOR: Changed according to reviewer recommendation

P21404, lines 7 and 20: I think you have mixed up the notation, on line 7 it should be alpha_ABI and line 20 alpha_RIMI.

AUTHOR: Changed according to reviewer recommendation

P21404 2.5.2, what was the sampling rate set to at RIMI? Also why do you restrict the data selection to late evening/night and mornings? These periods are known to be challenging for the EC method, especially mornings (transition periods) and nights when strong stratification may develop. You would also get more data to evaluate your

method with if you choose longer periods.

AUTHOR: Sampling rate (20 Hz) added. We don't restrict the data selection. The accepted fluxes (according to visual inspection) were simply predominantly from the late evening/night and morning. The comment has been removed from site description as it is repeated in the results/discussion.

P21404 2.5.3 what was the sampling frequency set to at Young Sound? Are the height specifications referring to height above tower base, above ice or sea level?

AUTHOR: Sampling rate (20 Hz at POLYI+DNB and 10Hz at ICEI) added. Heights are relative to the snow-surface. This has been added.

P21405 l 23: To be precise I think you mean "topographical induced advection".

AUTHOR: Changed according to reviewer recommendation

P21405 l 25: Why do you choose different alpha? Please motive.

AUTHOR: Sentence added: The higher resolutions of flux-estimates, relative to the Abisko and RIMI sites, were chosen for the purpose of another study concerning CO2-fluxes on sea-ice. P21406 line 8: I wouldn't call coordinate rotation and linear de-trending "instrument corrections" (as is the headline of this section. Rather, different steps in the postprocessing.

AUTHOR: "and post-processing" has been added to the headline, and the content has been moved around to separate instrument corrections from more general post-processing procedures.

P21407 3.1 I would like to know when the "typical cases" are observed. What are the meteorological conditions? I think this would be very useful, setting the OO method results into a context. Additionally, as mentioned previously, I would like to see comparison with Fourier cospectra for some of the cases. The Ogive is based on this but nevertheless, it would ease the interpretation.

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AUTHOR: All figures have been updated to give more insight into met. Conditions and now features a Fourier co-spectra for comparison.

P21407 line 7: I do not agree that the high frequency damping would consists of as much as 10% of the total flux. The correction appear to be closer to 2 W/m2. P21407 line 11: "Ogive spectrum" is an unusual terminology, please rephrase.

AUTHOR: I think you misunderstand the method here. Going back to equation (4) the Ogive optimization flux is $\hat{F}(O^2)=F_0+F\_HF$. In this particular case the components are: $F_0\approx$-48 Wm^{-2} and $F\_HF\approx$-5 Wm^{-2} hence the total Ogive optimization flux estimate is $\hat{F}(O^2)\approx$-53 Wm^{-2} and the high-frequency offset equals $(-5/-48)\cdot100\%=10.4\%$ of the total estimate. I have added "$F\_HF\approx$-5 Wm^{-2}" and a reference to equation (4) for clarification. Note that equation (4) has been changed slightly due to a mathematical error which was found following submission.

P21407 line 12-13: AS mentioned previously, I am not convinced you are separating the turbulent and advective parts, rather you are filtering out low frequency contributions to the flux estimate. Are both Fig 6a and 6b necessary? You are not commenting both separately so I think one of the is enough.

AUTHOR: Only one figure is retained and the other type of low-frequency influence is described in the text.

P21407 lines 17-19. Same as for Fig 6, I think you can skip either 7a or 7b, both are not necessary. Additionally, in the method description, the best model solution is chosen subjectively. How is this subjective selection performed in these two cases?

AUTHOR: I concur. Figure 7a is removed. As for the subjective evaluation it is stated in the result that: "The Ogive optimization method is seen to appropriately yield the turbulent flux contribution with the strongest density influence.". Though the difference in Ogive density between the two modes seen in Fig. 7 is subtle, the method still discerns which is more appropriate. Hence very little subjective evaluation is going on

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in this particular case. Had both modes been of same Ogive density, proper subjective evaluation would be based on (1) the quality of the fit, and (2) the length of the time-series responsible for the modes. If both fits are equally good, the choice would fall on the mode produced by the Ogives which represent the shorter time-series as they represent a more instantaneous flux-estimate relative to the mode produced by longer timeseries. The matter has been elaborated in the text.

P21407 line 28: Please avoid the interpretation "trustworthy". This appears very subjective, what do you mean with "trustworthy"? It is output from the current model where you filter out low frequency contributions.

AUTHOR: "Trustworthy" removed.

P21408 Fig 11, eq. 5. In order to make the statements you make in the Abstract regarding determining which flux magnitudes are affected by low frequency contributions you should plot against observed flux (F30min) not FOO as F30min is what most groups are calculating. Additionally, it would be helpful to plot this parameter (eq. 5) against for instance stability or some other met. Parameter that might be of importance (e.g. BL height if available). This would put the results into a context.

AUTHOR: Flux difference now shown relative to F^30min. Results changed accordingly. Flux difference also shown relative to atmospheric stability (Fig. 12)

P21410 Paragraphs starting with "shifts": These are interesting cases. I have observed this type of behavior during periods with fog (not published). Worth investigating further.

AUTHOR: No further analysis conducted for this study.

P21411 line 17: "The method has furthermore been shown to allow for flux estimation despite severe signal disruption" This statement I think needs rephrasing. Yes, you have shown that the method can be used during these cases, but so can other methods. I don't agree that you have shown you have estimated physically correct fluxes during these periods. Can it even be made if you have such a very poor signal? It

appears artificial i.e. making bricks without straw.

AUTHOR: The statement has been downplayed: "The method has furthermore been shown to at times allow for flux estimation despite signal disruption."

Figures and Figure captions Fig 1: Please rephrase the initial sentence to a more common format i.e. You shouldn't start with "The figure illustrates: : :" Also I would avoid putting interpretation of the figures in the caption, these should be made in the running text. Keep with just describing the figure.

AUTHOR: Changed according to reviewer recommendation

Figure 2 (and similar figures 5,6,7,8,9,10): I would like to see some clarifications in these figures. A color bar defining the grayscale, a legend explaining the black vertical bars. Unit is missing on x-axis. How are the w' and T' calculated? Using a linear de-trend? Please specify. In my view the w' sub figure is not necessary, you don't make a strong case in the text to motivate keeping this figure. Personally I think a time series plot of the covariance would be more useful. I would also go for a time series of T instead of T'. This would yield the additional info. I would also like to see a comparison with a traditional Fourier spectrum in a lin-log representation, and additionally a description of the mean met. conditions (e.g. stability, wind speed, RH etc.).

AUTHOR: Changed according to reviewer recommendation

Fig 3: As the schematic spectra are shown in log-log representation the area beneath the curve is not proportional to the total flux. If you plot in Lin-log representation you would realize that the orange areas don't contribute quite as much to the total flux as they are perceived in the current log-log representation. This should be clarified in the running text.

AUTHOR: The intention was for the Ogive to be plotted on a linear y-axis and for the Cospectrum to be plotted on a logarithmic scale. This has been clarified on the respective axes.

Fig. 4: As for Fig 1 caption, Please rephrase the caption to only describe the figure. Interpretation should be moved to the running text.

AUTHOR: Changed according to reviewer recommendation

Fig 5, subfigures are not labelled. And as previously, rephrasing the caption would be desirable here to strictly only describe the figure.

AUTHOR: Changed according to reviewer recommendation

Fig 6: Same as Fig. 5 (and also for Fig 6-10), additionally, you have filtered out low frequency contributions, not necessarily advective influence.

AUTHOR: Changed according to reviewer recommendation

Fig 11. The subfigures are really too small to be able to read properly, I suggest enlarging all of them. The Daneborg site is referred to as DNB in the site description, please choose one notation.

AUTHOR: Changed according to reviewer recommendation

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