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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.

Anonymous Referee #2

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Review of "Estimating local atmospheric-surface fluxes using Eddy Covariance and numerical Ogive optimization.

General comments: The work deals with the important and hard topic of advection. The authors propose a method, the Ogive optimization method (OO method), which they claim effectively excludes advection contributions to a flux estimate from eddy covariance measurements. I think the authors have done a good job developing this method, their work is valuable and appear to have good use but can we be sure it really does what the authors claim?

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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.

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.

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.

Specific comments

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

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

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statement.

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

P 21389 l 24: you probably mean “approximations” not “estimates”

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

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

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.

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)

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.

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

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

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.

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.

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).

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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?

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.

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.

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.

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

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

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

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 low-frequency contribution alone” might be helpful. I think you need to rephrase these two sentences.

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

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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”.

P21400 line : please correct the capital S in “Sensible heat”

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

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.

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.

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.

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

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

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

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.

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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.

P21403, line 17: I interpret the “flux strength” as “CO2 flux strength”, please specify.

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

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.

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.

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?

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

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

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 post-processing.

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.

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.

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P21407 line 11: “Ogive spectrum” is an unusual terminology, please rephrase.

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.

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?

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.

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.

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.

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.

Figures and Figure captions

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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.

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 detrend? 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.).

Fig 3: As the schematic spectra are shown in log-log representation the are nebeath the cruve 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.

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

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

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

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,

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please choose one notation.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21387, 2014.