REVIEWER#1 comments and author response:

Referee report of acp-2014-536

Estimating local atmosphere-surface fluxes using Eddy Covariance and numerical Ogive optimization

The authors have done a good job answering my comments to the original submission. I recommend accepting this paper. I find the paper interesting and hope this method will be evaluated also by other groups.

I just have a few additional comments and suggestions for technical corrections before the final publications which the authors and editor may consider.

1. I suggest shortening the title to: “Estimating surface fluxes using eddy covariance and numerical ogive optimization”. In my opinion labeling the fluxes “local atmosphere-surface” is superfluous.
Changed according to reviewer recommendation

2. Page 3, line 19-20: Here I would like to see a sentence on examples of the “certain conditions” when horizontal low-frequency contributions are significant (from Yi et al. and Zeri et al). It would improve the reader not accessing these papers just to get these examples.
Sentence changed to:
“Moreover, it has been commented that horizontal low-frequency contributions, which are typically assumed negligible, may become significant during conditions of low turbulence intensity and gravitational flows (Yi et al., 2008) as well as during flow disturbance associated with complex topography (Zeri et al., 2010).”

3. Page 7, line 10: comment: the noise you are referring to must be correlated between the scalar and the vertical wind speed signals in order to contribute to the co-spectra/fluxes. Random white noise would generally not contribute to your flux estimate.
The origin and type of “noise” considered here is clarified by adding the prefix “instrument-specific non-white” to “noise” throughout the paper. “Non-white” seemed to be the most appropriate term for noise that is not qualified and hence not necessarily Gaussian in nature.

4. Fig. 2 and 5-10, 13: legend, Change “Wind-origin” to wind direction. The meteorological convention for wind direction is defined as the direction from which the wind is coming from.
Additionally for Fig. 5-10, consider shortening the figure captions. These could possibly be shortened to “as Fig. 2 but…” and then specify conditions separating these cases from fig. 2.
I suggest a similar change for the caption to Fig. 12.
Changed according to reviewer recommendation

5. Fig. 12 I would suggest a relabeling of the x-axis to zL-1 instead of “atmospheric stability”.
Changed according to reviewer recommendation

OTHER COMMENTS BY AUTHORS:

1. Incorrect GPS coordinates of the RIMI sites were given. Coordinates and the site overview figure have been corrected accordingly.

2. Limits on Ogive integration (Eq. 2) was given in reverse. The equation has now been corrected according to the litterature (E.g. Foken et al., 2006).
3. Reviewer #2 was concerned with the high atmospheric CO2 concentrations shown in Figs 6 and 9. A detail missed by us during the writing process. After inspection it turned out that we had mistakenly converted molm-3 to ppmm rather than the usual ppmv for the sites Abisko and RIMI. Consequently a correction to the raw signal, and associated fluxes, were introduced to achieve ppmv values. This lowers the CO2 fluxes by a factor (29e-3Kg mol^(-1)_(dry air))/(44e-3Kg mol^(-1)_(CO2))=0.6 and increases Latent heat fluxes by a factor (29e-3Kg mol^(-1)_(dry air))/(44e-3Kg mol^(-1)_(H2O))=1.6. All relevant figures have been corrected and flux threshold results adjusted to: |Q_SENS|>40Wm-2, |Q_LAT|>20Wm-2 and |F_CO2|>100mmolm-2d-1.

REVIEWER#2 comments and author response:

I went through the revised manuscript. In the ACPD version it was very hard to understand what exactly was done. The manuscript has now gained in quality and is now digestible.

The idea is interesting and could help the interpretation of EC flux data. As far as I understand the paper a "pure" turbulent flux is derived from the mid frequency range (1-0.1 natural frequency). I am not fully convinced that the proposed method will become an "EC blockbuster" as the ogive optimization mechanism is not transparent and still has a subjective part. I also see a friction between the application of different long running means and the total length of the series. In my understanding both affect partially the time series in a similar way and the 10'000 ogive clouds are highly dependent from each other.

I was wondering on the indicated CO2 concentrations in figures 6 and 9. They lie between 585 and 600 ppm. Any explanation for these rather high values?
Your concern is warranted. After inspection it turned out that we had mistakenly converted molm-3 to ppmm rather than the usual ppmv for the sites Abisko and RIMI. Consequently a correction to the raw signal, and associated fluxes, were introduced to achieve ppmv values. This lowers the CO2 fluxes by a factor (29e-3Kg mol^(-1)_(dry air))/(44e-3Kg mol^(-1)_(CO2))=0.6 and increases Latent heat fluxes by a factor (29e-3Kg mol^(-1)_(dry air))/(44e-3Kg mol^(-1)_(H2O))=1.6. All relevant figures have been corrected and flux threshold results adjusted to: |Q_SENS|>40Wm-2, |Q_LAT|>20Wm-2 and |F_CO2|>100mmolm-2d-1.

The authors believe, but don't show it that their optimized "ogive" flux is then the best representation of the exchange flux of the footprint. It has to be kept in mind that this will be always a circular argument as the footprint of an EC measurement depend on flux data (u*, z/L,). Nevertheless I can recommend the publication of the revised manuscript in ACP. A publication might trigger the use of this method and a comparison with other approaches.

OTHER COMMENTS BY AUTHORS:

1. Incorrect GPS coordinates of the RIMI sites were given. Coordinates and the site overview figure have been corrected accordingly.
2. Limits on Ogive integration (Eq. 2) was given in reverse. The equation has now been corrected according to the litterature (E.g. Foken et al., 2006).