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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 # 1

I am not in favor of recommending this paper in its present form for publication because I have difficulties understanding its novelty even though I have read it several times. My difficulties are as follows:

1) The emphasis of the paper is on advection. The authors argue that advective influences on eddy flux measurements can be removed by a proper ogive analysis. A key assumption is that advection effects are confined to low frequencies whereas "locally

meaningful" fluxes are in the high frequency range. Their approach is unusual. In the past, the problem of advection is studied with mass conservation equations, but not from a time series perspective. In the conservation equations the advection term is clearly defined (that is, u dc/dx). In their approach, the definition of advection is ambiguous. It appears that any ogive that does not confirm to a standard model is blamed on advection. However, what they are really dealing with are low frequency eddy contributions and artifacts of non-stationarity. To say that one can get rid of advection effects using a time series analysis tool is a stretch in my view. Their emphasis (on advection) seems misplaced. Their real contribution is another method for data quality control, which is still a useful addition to the published literature.

AUTHOR: The other referee had similar concerns. Consequently the focus of the article has been shifted towards low-frequency contributions in general, which may include any of the following: topographical forcing on the observed flow, advection or large-scale meteorological phenomena, such as gravity waves, deep convection and large roll vortices. As for the unique time-series approach, the method is intended as an alternative to quantifying low-frequency influences fully by a 2D array of EC systems which can be very expensive.

2) Description of the method is scattered in several places. I don't know how to replicate their procedure, even though I have a reasonable amount of experience dealing with turbulent time series. If the authors choose to revise the manuscript, presentation of the method should be made more logical. (Ask someone outside your group to see if he/she knows how to reproduce your work.)

AUTHOR: We have expanded and refined the technical explanation of the method. Feedback from other investigators suggested that the corrections had clarified the application of the method.

I don't know how to perturb the time series and why we need a large ensemble when in fact non-stationarity features are clear from one realization (the actual observation).

AUTHOR: I disagree. The point of this study is that non-stationarity features are not always clear from the actual observation (i.e. 30min, linear detrend) because the high and low frequency flux contributions are dynamic and a 30min average is fixed. See e.g. Fig. 7 and Fig. 8.

The standard cospectrum model is criticized for being too simplistic but it is used anyway to determine a "locally meaning flux".

AUTHOR: All "standard" co-spectrum models are originally based on the co-spectrum model applied in this study (eq. 3) with empirically determined constants. Rather than using preset constants from other studies an optimization approach allows to determine constants reflecting the actual atmospheric conditions for each individual observation instead. As long as the boundaries within which constants are determined are physically reasonable our approach is thus more likely to yield a representative co-spectrum.

What do you mean by ogive optimization behaviors?

AUTHOR: The explanation has been elaborated for greater clarity.

3) Graphics are very crowded. Unnecessary details and symbols distract the reader from the main message they want to convey. Graphic fonts are too small. It is exhausting to read the long captions.

AUTHOR: Individual figures have been limited to a single example rather than two. Graphic fonts have been made bigger. Captions on similar figures have been streamlined ("If you've read one you've read them all). The other referee asked for more contextual meteorological information and regular fourier co-spectra comparisons which have been added. I.e. requested changes have been met based on compromise.

4) The language is not yet up to publishable standards. There are many cases of syntax error and confusing sentence structure. Not helping the reader are liberal use of math symbols and abbreviations – some of which are not defined (e.g lines 15-

20, p 21389) – and exceedingly long sentences. On this last point, let me give one example: "Accordingly, we can distinguish between two principal applications of the EC technique: process-oriented studies in which fluxes are being linked to local biochemical processes for parametric insight into universal causal flux-relationships and up-scaled through numerical modeling efforts, and long-term net ecosystem-exchange studies in which the flux estimates are understood to be site-specific, applying only for the unique conditions of ecosystem heterogeneity, topography and large-scale meteorological flows experienced during the study." (lines 18-24, p 21391) This sentence has 71 words. The meaning of the sentence is lost in my struggle to recover from exhaustion after reaching the end of the sentence. The authors should seek help from a colleague whose native language is English.

AUTHOR: A number of syntax errors have been corrected, long sentences have been split and the language in general refined a bit.

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