

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

Anonymous Referee #1

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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

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clearly defined (that is, $u \frac{dc}{dx}$). 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.

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. 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). The standard cospectrum model is criticized for being too simplistic but it is used anyway to determine a “locally meaning flux”. What do you mean by ogive optimization behaviors? 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.)

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.

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

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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.

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

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