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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cospectral analysis of high frequency signal loss in eddy covariance measurements" *by* A. Wolf and E. A. Laca

Anonymous Referee #2

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The study discussed the spectral loss in cospectra of surface layer measurements of momentum, heat, water vapour and carbon dioxide over a short grassland site. After applying spectral transfer functions, the loss was estimated using a method that extrapolates the high-frequency end of the cospectra toward zero assuming a -4/3 decay in cospectral energy. The study focused on the mathematics behind the estimated loss, and was light on the physical interpretation of the results in the context of atmospheric stability and biological sink or source strength of the considered scalars. Although the study may contain some interesting results, I have some general concerns about the experimental setup, and the method applied to estimate the spectral loss. Please see the general and specific comments listed below.

General comments

1) A large portion of the findings and problems the authors discuss in their results‘ section seemed to be caused by the experimental setup. Measurements were taken at ≈ 5 canopy heights and raw data sampled and recorded at 10 Hz. As the authors acknowledged in the results‘ section, the spectra lack sufficient and important data in the inertial subrange as the high-frequency end of the spectra is limited by the sampling theorem (Nyquist frequency). A ‘standard‘ setup of a flux site over a short canopy would sample at 20 Hz (which allows to extend the resolved natural frequency to 10 Hz) at a sampling height of at least 6 or 7 canopy heights exactly because of the known height dependencies of eddy sizes, and the need to capture all eddy sizes contributing to the vertical exchange. I therefore assume that this paper was derived as an afterthought that arose in the post-processing. The question the authors need to ask themselves is if the data are suitable to estimate cospectral loss given the mentioned limitations, or does the experimental setup compromise this attempt a priori?

2) Besides the more philosophical nature of the comment above, the experimental setup has direct practical implication for the analysis: Sampling at a too small rate to resolve all energy-contributing eddies leads to aliasing in the spectra, which means that spectral energy gets folded towards lower frequencies without altering the covariance (or the integral over the entire cospectrum). An underestimation of energy at the high-frequency end of the resolved spectrum may lead to an overestimation in the calculated spectral loss.

3) The authors need to point out, in what aspects their method to estimate and correct for high-frequency loss was different from other studies. In particular, it would be extraordinarily helpful if the authors indicated any improvements over existing methods, or how their estimates compare to results derived by those (e.g. the use of modeled ‘ideal‘ spectra; experimentally derived spectra from the Kansas experiment; adjusting the spectra of carbon dioxide and water vapour exchange to that of temperature, etc). Without this comparison, the reader is left alone without any frame of reference, and the results stay isolated from those obtained above similar surfaces.

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4) I may suggest to the authors to restructure the current draft and focus on either a) comparison of methods to estimate the high-frequency loss including their method (more technical paper), or b) deeper interpretation of the results on the background of scalar similarity in surface-layer exchange and in the context of biological sink/source distribution of the scalars (paper with broader intent to the micrometeorological and biological communities). In my opinion, either direction would greatly improve the benefit of the contribution to the flux community.

Specific comments

a) Page 13152, line 22: Please replace the citation (Baldocchi et al. 2001) with a more appropriate one. This paper merely describes the FLUXNET strategy, and the role of eddy-covariance within this project. However, it cannot serve as a reference for eddy-covariance per se, or its role in land-surface exchange measurements, despite the fact that it is heavily cited.

b) Page 13156, Section 2.2: What QA/QC method was applied to filter for stationary periods with developed turbulence? Some outliers in later figures (e.g. Fig 3, nighttime conditions) suggested that the dataset still contained some erratic data approaching near convective conditions at night. The authors need to exclude those intervals for the computation of the mean spectra.

c) E.g. Page 13157, line 9, and later on: longitudinal velocity u is not a scalar, but a vector. Per definition, a scalar is a tensor of rank 0, whereas a vector is a tensor of rank 1, i.e. has a magnitude and a direction. Please correct.

d) Page 13157, line 19; Page 13159, line 10: Here and later on the authors referred to the ‘final four bins‘. It could be gleaned from the subsequent paragraphs that they meant the high-frequency end of the spectra, rather than the low-frequency. Please clarify.

e) Page 13157, line 14 and Equation (7): It seemed that the symbol *f* was used for the natural frequency and the normalized frequency. Please use symbols in a unique, coherent fashion throughout the manuscript to prevent misunderstandings.

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f) Page 13159, lines 25-28: Considering that the canopy is fairly small (0.3 m) and homogeneous horizontally and vertically, how do the authors explain different locales in the canopy for different scalars? Does that point to long-range, non-local transport of scalars? Some spectral work on scalar similarity in tall canopies has been done by Ruppert et al. (2006, BLM, 'Scalar similarity for relaxed eddy accumulation methods'), and differences in spectra indicated to differences in the vertical distribution of biological sinks and sources.

g) Page 13160, lines 9pp: It is not surprising that the slope in Figure 7d approached zero as stability approaches neutrality. In the case of $\overline{w\prime T\prime}$, the flux becomes zero (and changes its sign from unstable to stable conditions), because $T\prime \approx 0$, and not because the upward flux equals the downward flux as in case of the carbon dioxide exchange. In fact, as there is no observed sensible heat flux in neutral conditions (which is the exact definition of neutral), most similarity theory predictions fail (e.g. $\sigma_T T_*^{-1} \sim \zeta$). Hence, the uncertainty in the estimated high-frequency loss is much greater than its actual flux leading to unrealistic values up to 40 % (Figure 7c).

h) Page 13160, line 27: Throughout the manuscript, the authors assume the position of 'the neutral observer' in their writing, but switch to first person here.

i) Page 13160, lines 25pp: The method of subsampling the existing time series at lower sampling frequencies without block averaging is known as DEC (disjunct eddy covariance), and has its own implications on derived fluxes.

j) Page 13161, Section 4: The authors frequently use the expression 'undermeasure' to describe the 'underestimation by measurements'. Please remedy.

k) Page 13168, Fig 4: The curve for $\zeta \geq 1$ clearly contained some local maxima and minima, particularly for $\eta \leq 0.02$. What is the origin of these spectral peaks? Did the authors check and filter stable periods for occurring waves, and /or wave-turbulence interaction? In stable boundary layers, waves are often generated non-locally and can propagate over long distances.

l) Page 13169, Fig 5: The figures caption and the label of the y-axis mismatched.

m) Page 13172, Fig 8: The legend and the linestyles used in the plots mismatched. I

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suggest to chose a coherent definition of bins throughout the paper (either center or upper border) to facilitate the cross-comparison of results.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13151, 2007.