Atmos. Chem. Phys. Discuss., 7, S6485–S6487, 2007
www.atmos-chem-phys-discuss.net/7/S6485/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S6485–S6487, 2007

Interactive Comment

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

A. Wolf

adamwolf@stanford.edu

Received and published: 31 October 2007

Thank you for your constructive comments. You correctly summarized our approach and results, but I don't think you appreciate the motivating force: everyone's data is compromised in eddy covariance studies. No one can claim perfect energy balance closure. The theoretical basis for this has yet to be determined, and this paper presents one argument for why energy balance may not be achieved. The referree is correct that the motivation for the study arose during post-processing, but nonetheless the hypothesis and its testing preceded the analysis presented here. We found that, yes, cutoff losses could explain some lack of energy balance closure. It was not our intent to write a general paper on high frequency damping losses.

This paper was intended to be consise, practical, and to expand on the sparse literature examining observed cospectra, particularly those of water and carbon dioxide. I was not aware of any other study which used our approach for correcting cutoff losses (as apposed to damping losses) of fluxes. As such, there is really nothing else to compare it to. Werner Eugster in another comment presents a function to estimate the loss beyond this cutoff frequency given by Panofsky and Dutton, but we are not sure that this equation is appropriate for estimating lost flux, because it needs to be integrated in log-linear space.

I sympathise with the referee's desire for us to make this paper more definitive. Regarding scalar similarity, I have made an analysis of the correlation coefficients between T and C, T and Q, and Q and C under different stability regimes (included in a separate manuscript in review), and find that the scalars are much less correlated under more neutral conditions. I consider lack of similarity in this system to be a matter of fine-scale spatial heterogeneity because Q and C are exchanged by leaves, but the soil in between the grass tussocks are main generators of heat. This is argued (again in the separate manuscript) on the basis of the discrepancy between two eddy covariance systems located 10m apart. Thinking more about it however, I don't think this explains why in this paper the different scalars have such different cospectra, specifically Q and C versus T. I would prefer to excise this line rather than expand upon it.

I did do comparison of these data with the Kansas spectra, but in the end did not include it. The Kansas cospectra were different, being shifted to lower frequencies. I will take your suggestion and make a more complete comparison.

A couple points of disagreement: I disagree that our setup was non-standard. 10 Hz is a very common measurement frequency in eddy covariance. I am unaware of a canonical sensor height of 6 or 7 canopy heights (do you have a citation?), but clearly our study underlines the need for higher towers than we used, and it would useful if this was widely appreciated.

Also, I did not remove any data from consideration, neither as stationary periods with

ACPD

7, S6485–S6487, 2007

Interactive Comment

developed turbulence (comment b), or stable periods with waves (comment k). I found it more justifiable to present all the cospectra, not with ad hoc removing of outliers. At the minimum, until you mentioned it here, I would not have thought to exclude these conditions based on other eddy covariance QA criteria. Perhaps these conditions could be identified on the basis of their cospectra and used to flag them as part of a more general approach to eddy covariance QC. That is not the goal if this paper, but it would be useful.

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

ACPD

7, S6485–S6487, 2007

Interactive Comment