

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

W. Eugster

werner.eugster@ipw.agrl.ethz.ch

Received and published: 31 October 2007

The questions about high-frequency losses come up every so often in the scientific discussion and to make progress, it would be desirable to include the following papers in their considerations:

Eugster, W. & Senn, W. A Cospectral Correction Model for Measurement of Turbulent NO_2 Flux Boundary-Layer Meteorol., 1995 , 74 , 321–340

have shown how damped cospectra look like, and that the factor H_x , that the authors define as Eq. (5) is actually the square-root of the factor shown in Eq. (24) of Eugster & Senn (1995). It does not become clear why the authors do not define $H_x(f) = (1 + 2\pi f\tau_{wx})^{-2}$ to be consistent with what Eugster & Senn published. It might just be a

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

typo, but this might be an essential typo! Please note that what the authors named τ_{wx} corresponds with the damping constant L in Eugster & Senn (1995).

Another omitted source is:

Horst, T. W. On Frequency Response Corrections For Eddy Covariance Flux Measurements Boundary-Layer Meteorol., 2000 , 94 , 517–520

who clearly explains why the concept of adding transfer functions is not the correct approach (the reasoning behind this is that we are actually in complex space), but my experience so far was that this is not a big deal because the assumptions (first-order damping) are the same in Eugster & Senn 1995) as in other approaches. More important in my view are the differences at the low-frequency side.

In any case, the Eugster & Senn (1995) model uses z/L , the Monin-Obukhov stability as one of the parameters to estimate the fraction of flux loss (the other parameters are: z , measurement height above displacement height; τ_{wx} in the terminology of this paper; and the mean horizontal wind speed \bar{u}).

I would have been excited if the authors had tried to show that our concept was wrong, or (better) confirmed that this is a good approach, but could be improved given their additional insight that they got with their work.

A last point: If you ignore damping, and only look at the losses due to cut-off, you will find the following resource worthwhile to be cited:

Panofsky, H. A. & Dutton, J. A. Atmospheric Turbulence John Wiley Sons, 1984

On page 208, bottom, you will read:

If the stress or heat flux has been measured to an effective cutoff frequency f_0 (because of instrumental limitations), then the remainder is given by

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

$$\int_{f_0}^{\infty} C_o(f)df \quad (1)$$

which, because of the -7/3 law, equals $\frac{3}{4}f_0C_o(f_0)$ provided that f_0 is a frequency beyond which the inertial-range equation can be assumed to be valid (Panofsky & Dutton, 1984).

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

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper