Atmos. Chem. Phys. Discuss., 8, S7641–S7645, 2008
www.atmos-chem-phys-discuss.net/8/S7641/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The use of disjunct eddy sampling methods for the determination of ecosystem level fluxes of trace gases." *by* A. A. Turnipseed et al.

A. A. Turnipseed et al.

Received and published: 29 September 2008

We thank Dr. Lenschow for his insightful comments and careful reading of our manuscript. Our responses to his specific comments follow.

Specific Comments:

Reviewer Comment: p. 3, l. 20: The sampling period depends on height - especially in the surface layer. With moderately strong winds at 2 m height, 0.2 s might not be short enough. The integral scale (IS) normalized by height above the surface is ˜ 1 in a convective PBL. This gives IS = 2 m. For a mean wind of 8 m/s, 0.2 s gives an averaging length of 1.6 m. This would likely result in some flux loss, roughly about 10%. Thus, it might be worth adding that, ($<$ 0.2 s for measuring heights $>$ 2m), or something similar.

Author Response. Pg 3., ll 20. We have noted this as suggested.

Reviewer Comment: Eq.(1) and (3) : The integration limit should be T_{avg}, since the integral is normalized by T_{avg}.

Author Response: Eq. 1 and 3. We have corrected as suggested.

Reviewer Comment. p. 5, l. 18-19: The ensemble average is the average obtained by averaging together an infinite set of repetitions of an experiment under identical conditions. Thus, it is unattainable in practice, but useful in theory. So, the right expression of (2) is not an ensemble average, but perhaps an estimate of the ensemble average.

Author Response: P. 5, ll 18-19. We have removed the reference to "ensemble average"; and just state that "the flux can be estimated from the covariance of the subset of points"

Reviewer Comment: p. 6, l. 5: "of this type" needs to be spelled out; i.e. frequency-dependent corrections.

Author Response: P.6, ll 5. We have corrected as suggested.

Reviewer Comment: p. 7, l. 5: What are the units of k?

Author Response: P. 7, ll 5. The units of k are Volume*s/m (m3s/m or m2s). However, the value of k is more fully explained in Section 3.3 (Eq. 21 and following paragraph) as it pertains to our system. We will include the units at this point in the text.

Reviewer Comment: p. 7, l. 8: ...and the mass *of scalar* (not e.g. of air)

Author Response: P.7, ll 8., We have corrected as suggested.

Reviewer Comment: p. 7, bottom: Eq.(10) uses c^+ and c^-, which are not defined. Similarly, {\overbar c} in(4) is not defined. I don't understand the derivation of (10). Since V^+ = V^- = V_{tot}/2,it seems that you could say on line 16 that you multiply

ACPD

8, S7641–S7645, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(8) by 1/2 instead of V_{tot}/2V, based on the expression on line 14. Am I missing something?

Author Response: cˆ+ = mˆ+/Vˆ+ (and cˆ- = mˆ-/Vˆ-). We will define these as suggested. {\overbar c} in Eq. 4 will be changed to read {\overbar c}disj and will be defined in the text as the mean density of c of the disjunctly sampled points (subsequent equations will also be updated). To attempt to clarify the derivation, we have re-written Eq. (9) paragraph to relate Vˆ+ (or Vˆ-) to w. Both sides of Eq. (8) can then be divided by Vˆ+. Rearrangement and substitution using the definitions of cˆ+ and cˆ- and Eq. (9) allow one to arrive at Eq. (10).

Reviewer Comment: p. 9, l. 13: What is the definition of "the standard deviation of the slope?" The slope of what? Is it related to the increase in error variance as a function of the sampling time interval normalized by the total record time?

Author Response: Pg 9, ll 13., The "standard deviation of the slope" is the standard deviation of the slope derived from linear regressions of disjunctly-sampled eddy co-variance heat fluxes vs. those derived via continuouse-sampling eddy covariance (EC). To clarify we have changed the wording of this paragraph to read: "We computed the standard deviation of the regression slope (sigma-m) from plots of disjunctly sampled heat fluxes vs. EC heat fluxes (Fig. 2a). Figure 2b then shows how sigma-m changes as a function of Dt/Tavg." Lenschow et al. (1994) have shown that the error variance (sigmaˆ2) of a disjunctly-sampled flux measurement depends linearly upon Dt/Tavg, indicating that the standard deviation should vary as (Dt/Tavg)ˆ0.5. We re-fit Eq. (17) to a square root dependence (i.e., the exponent = 0.5) to be more consistent with the Lenschow et al work and will report this in the text. Due to the scatter of the data, this equation gave an equally good fit (from inspection of the R2-value) as the previous fit (where the exponent was allowed to vary and gave a value of 0.609).

Reviewer Comment: p. 9, Eq.(17): Where did this equation come from? Is it purely empirical? It is certainly not the expression derived by Lenschow et al. (1994). Why

ACPD

8, S7641–S7645, 2008

did you not use their equation?

Author Response: Pg. 9, Eq. 17. This equation is derived empirically (as opposed to the Lenschow et al., 1994 equation) and we will note that in the text in the line following Eq. 17. This equation was derived to serve as an empirical guide that would give a simple estimate of the increase in uncertainty in a disjunctly-sampled measurement relative to the conventional continuous sampling without the need for turbulence data (i.e., the integral time scale is required in the uncertainty calculations of Lenschow et al., 1994) and was intended to be more useful in the design of DEC systems. We will note that the reader should consult the equations of Lenschow et al., 1994 to evaluate the actual uncertainty in any given DEC flux measurement.

Reviewer Comment: p. 10, top: I don't understand how an increase of 8% in overall error variance is equivalent to a 28% increase in standard deviation. Using Eq. (58) from Lenschow et al. (1994), I get a standard percent error of 15.5% for the values given on l. 2, instead of 30%. Why the discrepancy?

Author Response: Pg. 10. The comment that an increase of 8% in overall error variance is equivalent to a 28% increase in standard deviation was a misstatement in the text and will be removed. Dr. Lenschow is correct that the standard percent error for the sampling protocol described is 15.5% using Eq. (58) from Lenschow et al., (1994). The 30% value obtained from Eq. (17) represents additional error relative to continuous sampling, so they are not directly comparable. We have removed this comparison from the text except to say that our estimates of additional uncertainty vary with Dt and Tavg as expected from Lenschow et al., (1994) as discussed in the comment above.

Reviewer Comment: p. 11, l. 9-11: The deposition velocity (or the equivalent velocity used here) depends on using a reference height for specifying the mean concentration. What is the reference height(s) used for the estimates in Table 2?

Author Response: Pg. 11, ll 9-11. It is true the deposition velocity (or exchange velocity to include emission) is height dependent due to the concentration gradient.

ACPD

8, S7641–S7645, 2008

Interactive Comment

The reference heights in Table 2 should be 10 m and we will note that in Footnote (a) of Table 2.

Reviewer Comment: p. 11, Eq.(19): I think that there is a mistake here. The factor should be 1.6 instead of 2.5.

Author Response: Pg. 11, Eq. 19. The factor in Eq. 19 should be 2.5. The error occurs in the following paragraph (line 8) where the factor 0.8 appears on the wrong side of an equation. The text should read: "0.8*sigma(w,d)= |w|{overbar}".

Reviewer Comment: p. 18, l. 21-23: Did you use the maximum covariance (magnitude) as an indication of the time lag?

Author Response: Pg.18, ll 21-23. The time lag deduced from the cross correlation did use the maximum covariance to indicate the lag. We have noted that in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13413, 2008.

ACPD

8, S7641–S7645, 2008

Interactive Comment