Atmos. Chem. Phys. Discuss., 9, C9882–C9886, 2010
www.atmos-chem-phys-discuss.net/9/C9882/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Turbulent exchange and segregation of HO_x radicals and volatile organic compounds above a deciduous forest" *by* R. Dlugi et al.

Anonymous Referee #2

Received and published: 20 January 2010

This is an innovative study that makes an important and useful contribution to the literature. Overall, the paper presents novel results and is well-written and well-researched. This is a pioneering effort for further research on the interactions between transport and chemistry involving highly reactive species. I have only a few significant comments.

There is considerable discussion of the systematic errors in the statistical moments contributed by the limited sensor responses, but very little about the random errors. The averaging interval in the figures showing the statistical moments is 10 minutes. An interval this short has considerable random error, particularly for higher-order moments such as skewness and kurtosis. I suggest that some consideration be given

to estimating the contribution of random errors to the results. This requires determining the integral scales of the relevant variables, which would be of intrinsic interest. Furthermore, the integral scale approach also provides a quantitative approach to estimating systematic errors in measurements of statistical moments, including variances, covariances, and third- and fourth-order moments, of time-response-limited sensors. Again, I suggest that the authors give some estimate of these random and systematic errors, or at least acknowledge them.

This is particularly relevant since the mean wind is very light–the average being less than 2 m/s–for most of the time. This means that over the 10 min. average, the averaging length is of order 1 km or less. At 37 m height, this means that both the random and systematic errors may be quite large.

I apologize for the fact that I used the initial submission for my review, without realizing that I should have waited for the online version. I trust that the authors can use that version for addressing the comments.

MINOR COMMENTS

Abstract: ECHO is not defined here nor on p. 3, where it first appears in the text. It's finally defined on p. 4 after it has been repeatedly used.

p. 2, l. 2: "...at the canopy." Do you need "in" or "above"?

p. 3, l. 5: ...products of isoprene, i.e. methyl vinyl...

p. 3, l. 13 from bot.: Butler et al.

p. 4, l. 6: coniferous

p. 4, l. 12 from bot.: to find out the conditions for which a stand...

p. 4, l. 4 from bot.: The results showed, for example,...

p. 4, l. 3 from bot.: Do you really mean decreasing frequency? Or do you mean

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decreasing intensity?

p. 5, l. 6: "concurrently" instead of "in parallel"?

p. 5, l. 15: ...inhomogeneous mixing...

p. 5, l. 8 from bot.: The measurement procedures are derived...

p. 5, l. 3 from bot.: is J_i a molecular diffusion flux density?

p. 5, l. 3 from bot.: This is the first time I've seen "barycentric velocity." I looked it up online, and learned something. I don't know, but would it be worth (or possible) to add some definition?

p. 6, l. 9: It is also a non-convective flux.

p. 6, bot.: It should be Eq. (3), not Eq. (4).

p. 7, l. 3: ...describes the way in which...

p. 7, l. 12: This sentence seems awkward. I suggest starting with: Significant flux divergence can occur if the chemical...

p. 7, l. 7 from bot.: ...the species vary as if...

p. 7, l. 3 from bot.: ..., or between diffusion

p. 8, l. 5: with R_{\rho_i\rho_j} = k \overline{\rho_i\rho_j}.

p. 8, l. 9: "unmixed" instead of "nonmixed"

p. 8, l. 11: Fluxes of OH and HO_2, and isoprene, as well as its...

p. 8, l. 4 from bot.: where N is the number of data points separated by \Delta t...

p. 8, bot.: I always think of the z-axis as normal to the geopotential surface.

p. 9, l. 8: This allowed us to investigate the influence...

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p. 9, l. 7 from bot.: distances

p. 10, top: The principle of operation...

p. 10, l. 7: ...are recorded at 0.2 Hz, different from Spirig ... who recorded at 0.03 Hz?

p. 10, l. 14: Other operational parameters were...

p. 12, l. 8: considered

p. 12, l. 9: period

p. 12, l. 6 from bot.: ...recorded at 10 Hz...

p. 13, top: ..corrected to better than...

p. 13, l. 8: polynomial

p. 14, l. 8 from bot.: ...with respect to...

p. 14, l. 7 from bot.: The sentence, "Horst (1997) estimated..." seems so obvious that it need not be stated.

p. 15, discussion of Finkelstein & Sims can be shortened, or eliminated in my view. If included, I think it sufficient to say that they found that (8) is generally OK.

p. 16, top: I don't think it necessary to redefine the flux densities here.

p. 19, middle: It seems unnecessary to add that the surfaces of leaves near canopy top are heated by solar radiation unless the assumption is that there is no BVOC emission without solar radiation directly hitting the leaves. As long as the vegetation emits BVOCs, there should be correlation between temperature-w covariance and BVOC-w covariance.

p. 20, top: The isoprene flux is just equal to the sum of the flux at z_R and the integrated sink between the two heights. I'm not sure what is the point of comparing the flux divergence (1.4 x 10^(-4) to 7.7 x 10^(-4)) to the measured flux. If it is to point out the

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lifetime of isoprene, why not calculate the ratio of the concentration to the sink to give a lifetime? In the next paragraph, this could also be done for OH to show that the OH lifetime is very short, and thus the flux divergence can supply only a fraction of the OH reacting with other species; i.e. it must be generated locally. Therefore, you can obviate the need for assuming a 1 m layer, which seems artificial.

p. 20, l. 13, and elsewhere: "on the order of" instead of "in the order of"

p. 20, bot.: I don't know of any mechanism for convection to suppress instantaneous mixing. Could it be that convection can transport parcels away from the source so rapidly that they do not have time to mix?

p. 21, l. 9: Is the implication here that the measured segregation here is not affected by the time response of the sensor? I find that hard to believe. It certainly is not true in the limit of very slow responding sensors.

p. 21, bot.: Not sure what is meant by "smaller distances of the measuring volumes." Do you mean smaller tube lengths? Or smaller instrument volumes?

END OF REVIEW

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24423, 2009.