

Interactive comment on “Estimation of nocturnal CO₂ and N₂O soil emissions using changes in surface boundary layer mass storage” by Richard H. Grant and Rex A. Omonode

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-The approach to determining 6.3m as being the “lid” to the surface accumulation seems a bit arbitrary and more a result of practical limitations than physical considerations. Looking at Fig. 4(c) and (linearly) extrapolating the segment from 5m to 8m, it appears that the 400 ppm line (i.e. most likely concentration above the surface layer) is reached in a remarkably narrow band between 10 and 12m. Maybe using the geometric mean of 11m and 5m (i.e. 7.4m) would be a better estimate of the depth of the accumulation layer? Nights other than 5 August should be checked to see whether this is repeatable. The lid top is somewhat arbitrary but based on the log profile of the wind

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giving rationale for the geometric mean within the measured range. Since nothing is known about 11 m or anything above 8m, I cannot see a justification for 7.4m or other estimate. Using 400 ppm as the threshold is also arbitrary. I believe assuming linear gradients in extrapolation is hard to justify for a stable BL.

-More points in the vertical would have helped to shed light on this; it is a shame (and puzzling) that the 3m level misbehaved the way it did. Yes, unfortunate. A line-integrated measure would be much better next time.

-It is also unfortunate that even though instruments were available that could have measured eddy covariance fluxes of CO₂ and N₂O, this was apparently not done. A third estimate of nocturnal emission fluxes could have been obtained by looking at windy nights through eddy covariance. Yes, but it was not possible at the time.

-The comparison between the accumulation method and the soil chambers needs to be quantified a bit better; presenting statistics in a table would be a good approach. This was not done due to the strong tendency of decreasing flux over time. However a table was added (Table 5) representing these time periods.

Specific Comments

-Page 1 Line 6: Annual emission budgets. A budget would include sinks. Unclear what an emission budget would be. Not changed.

-P1L9: remove “the concentration of” OK corrected

-P1L26: eddy covariance is the accepted working term. A correlation only goes from -1 to +1 and has no units. My mistake. Corrected. That was the ‘incorrect’ term first used for the method.

-P2L3: consistency with hyphens Fixed

-P2L6: there is a huge range of stable nocturnal boundary layer depths, so I would leave out the 100m, or say “on the order of 100m”. OK corrected

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-P2L8-10 information in the sentence is redundant Since nocturnal inversions can also occur with substantial warm air advection, this description is there to indicate a radiation inversion. Retained.

-P2L10 and elsewhere, Pendall OK corrected

-P2L22/23: as mentioned by another reviewer, molecular diffusivity is on the order of 10^{-5} m²/s. Turbulent eddy diffusivities can range from near-molecular up to 10's of m²/s, so I would leave the 10^{-3} out. Agreed. Removed.

-P3L7: It is standard practice to provide at least one sentence on the location (even though with the map in Fig. 1 it only takes a minute to find the place). Fixed.

-P4L4: Obukhov OK corrected

-P4L15, P6L24: remove the ' over w. OK, though should not matter

-P4L22 state the Schmidt number, if a constant was used 0.91 for CO₂ and 0.95 for N₂O- added

-P5L7: were these instruments cross-calibrated with the real-time instruments? No-Gas chromatograph samples too small to get equivalent real-time measurements without affecting pressure.

-P5L21: "at 8m" duplicated

-P6L5: see general comments. Seems like a rather arbitrary approach. Yes, it is. See comments above. As it stands, the magnitudes of the accumulation are obviously sensitive to the height. I have added this statement to the results and conclusions.

-P6L19: should this be 2.8m?

-P9: this section would be aided greatly by a table comparing the statistics of chamber vs. mass accumulation (averages, ranges, correlation coefficients etc.) Since chamber emissions decline throughout the period, a table with chamber measurements would

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be problematic. This is why I did not do so.

-Tables 2,3: as mentioned by another reviewer, definitely change the footnote numbers, which currently look like exponents This has been revised to clarify

-Fig. 2: presumably the x-axis is LT? Yes, added to caption

-Fig. 3: a precise definition for the change in wind direction is required. Why is it always positive? The overlap between the horizontal variance and wind direction points is a bit messy. It might be preferable to overlap the two variances. Wind direction differences are absolute values. It is now indicated in the caption and axis label. I have shifted the axis a bit to remove most overlap

-Fig. 4: wrong units on the vertical variance Corrected

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C4

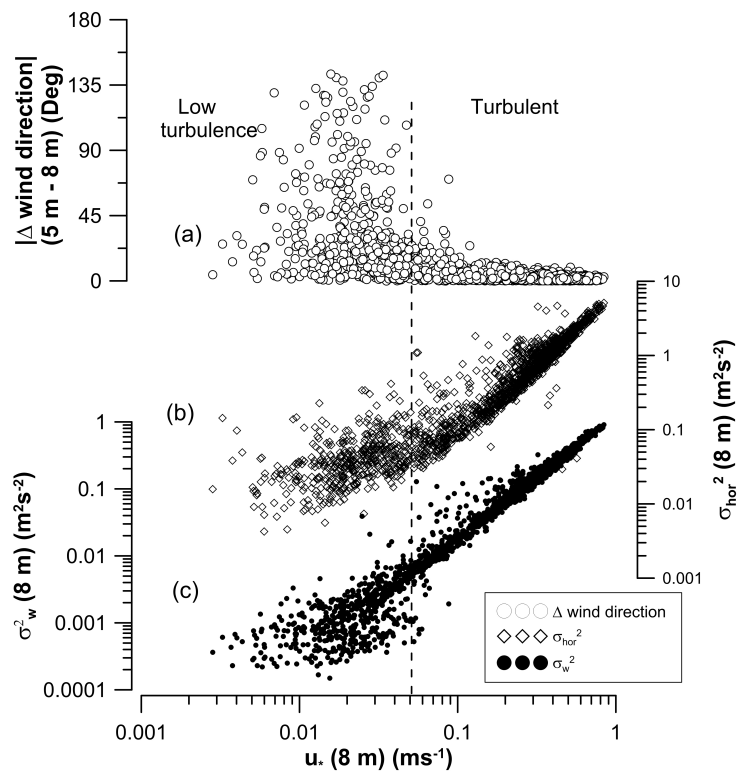


Fig. 1.

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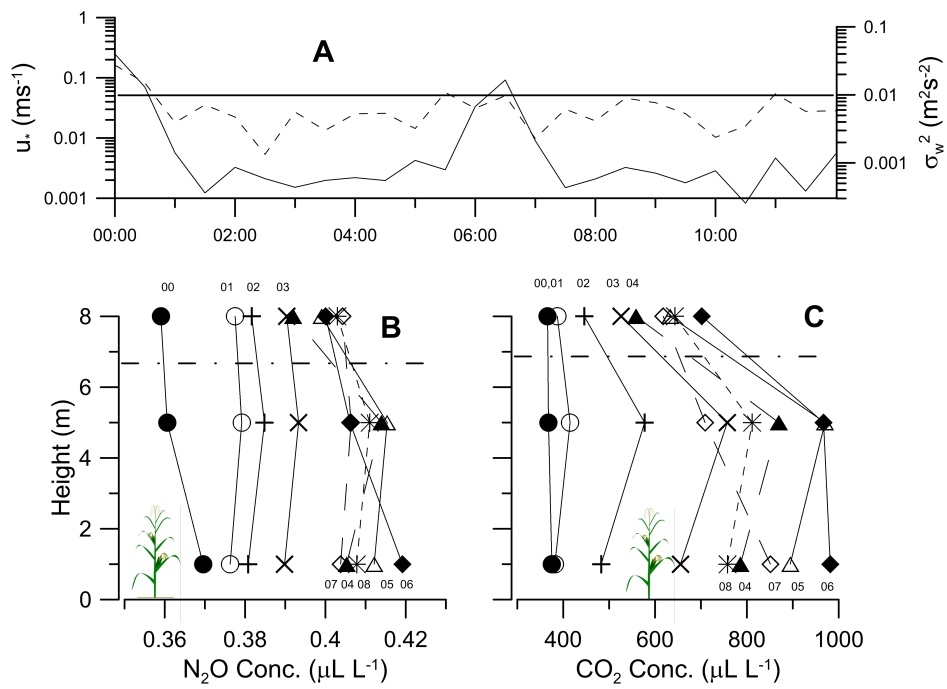


Fig. 2.

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