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

Richard H. Grant and Rex A. Omonode
rgrant@purdue.edu

Received and published: 13 December 2017

- 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 giving rationale for the geometric mean within the measured range. Since nothing is known about 11m 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. An 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 there 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
Since nocturnal inversions can also occur with substantial warm air advection, this description is there to indicate a radiation inversion. Retained.

As mentioned by another reviewer, molecular diffusivity is on the order of 10^{-5}\, m^2/s. Turbulent eddy diffusivities can range from near-molecular up to 10's of m^2/s, so I would leave the 10^{-3} out. Agreed. Removed.

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.

- 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.

Fig. 1.

C5

Fig. 2.

C6