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Atmospheric Measurement Techniques Discussions

Interactive comment on "Estimation of nocturnal CO_2 and N_2O soil emissions using changes in surface boundary layer mass storage" *by* Richard H. Grant and Rex A. Omonode

Anonymous Referee #3

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General Comments

This manuscript describes an application of the method of determining surface fluxes by quantifying the build-up of the emitted gas in a thin atmospheric surface layer during stable conditions. The experiment took place in a corn field, and the gases quantified were CO2 and N2O. A comparison with soil flux chamber results is presented. This method has been around for a while but, unlike other micrometeorological approaches, has not managed to enter the realm of operational methods because it appears too difficult to automate.

This manuscript does represent a nice evaluation of the feasibility of the method. It

is well-structured and logically consistent, and deals with the identification of stable periods in a thorough manner. However, I do have a few comments on some things that can be improved upon before publication, and some that should be considered in future similar studies.

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. 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.

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. 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.

The comments of the first two reviewers are excellent, and in most cases I will try not to repeat what they have already pointed out.

Specific Comments

Page 1 Line 6: Annual emission budgets

P1L9: remove "the concentration of"

P1L26: eddy covariance is the accepted working term. A correlation only goes from -1 to +1 and has no units.

P2L3: consistency with hyphens

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”.

P2L8-10 information in the sentence is redundant

P2L10 and elsewhere, Pendall

P2L22/23: 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.

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).

P4L4: Obukhov

P4L15, P6L24: remove the ‘ over w.

P4L22 state the Schmidt number, if a constant was used

P5L7: were these instruments cross-calibrated with the real-time instruments?

P5L21: “at 8m” duplicated

P6L5: see general comments. Seems like a rather arbitrary approach.

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.)

Tables 2,3: as mentioned by another reviewer, definitely change the footnote numbers, which currently look like exponents

Fig. 2: presumably the x-axis is LT?

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.

Fig. 4: wrong units on the vertical variance

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