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Atmospheric Measurement Techniques
Discussions

# *Interactive comment on* "The detectability of nitrous oxide mitigation efficacy in intensively grazed pastures using a multiple plot micrometeorological technique" *by* A. M. S. McMillan et al.

**Anonymous Referee #2**

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This paper takes a detailed look at a flux-gradient system for measuring N2O emissions from soils, with a focus on determining the capabilities of the system for measuring emission differences between fields having different management practices. The paper is well written, thorough, and from a scientific standpoint, the study appears to be properly implemented and interpreted. I enjoyed reading this manuscript and I recommend publication. My comments below are minor.

Comments/Notes



Pg. 8960, Ln 17: In discussing minimum detectability of emission rates, the authors translate the minimum detectable concentration difference to the emission rate. Because the translation also depends on atmospheric diffusivity (transfer coefficient), there is not a consistent relationship between the two. Thus, in the authors example of the minimum detectable emission rate, there is no reason to put three significant digits on this value.

Pg. 8964, Ln 12: It would be appropriate to indicate the long history of using flux-gradient methods to measure agricultural fluxes – longer than suggested by these references. It benefits the reader, and gives added confidence in the authors, if they demonstrate this in citing important references. I'd add one or two of these more historic works in the citation list (e.g., Denmead, Simpson, Freney. 1974. Ammonia flux into the atmosphere . . . Science).

Pg. 8968, Ln 6: Not clear if SE is calculated for each measurement interval, is calculated once from all the variance information, from some trial period . . .

Pg. 8971, Ln 17: Somewhere in this section it would be helpful to indicate the flux calculation can be identified as the aerodynamic FG method, where it is assumed the diffusivity for N2O is equivalent to that for momentum.

Pg. 8972, Ln 3: How is the zero plane displacement height (d) determined?

Pg. 8974, Ln 5: Were "negative" gradients in N2O observed? I would be surprised if this was not the case. If so, some discussion of how those negative gradients should be interpreted would be appreciated. The subject of N2O absorption by the soil is fascinating, and is debatable. The authors' experience would be appreciated.

Pg. 8974, Ln 22: Given the importance of SE_DeltaN2O, I'm curious about its characteristics. Does SE scale on Delta_N2O? Is the uncertainty better represented as a percentage of the measurement, rather than assuming a constant value (e.g., large Delta_N20 corresponds with large SE_DeltaN2O)? Could the assumption of a constant value for SE (0.023 ppb) lead to a too-conservative calculation for detectable flux levels?

Pg. 8979, Ln 19: The value of SE_C is given as 0.12. Does this have units, or is this the ratio of SE_C/C? Clarify. I'm guessing from the text that SE_C/C = 0.12. My intuition says this is too low, as micromet relationships can show large period-to-period variability (e.g., relationship of windspeed gradients to u*, which is the basis of the FG calculation here). The derivation of this uncertainty value is unavailable to us (Ph.D. thesis) – can the authors summarize this critical result in a few sentences?

Pg. 8985, Ln 8: I agree that if the FS-NOMAS system could be used to sequence between masts (mast 1, mast 2, mast 3, mast 4, mast 1, . . .), so that Delta_N2O for each treatment was estimated over the same time interval (e.g. 30 minutes), this could reduce the detectability limits of the flux differences (I assume the uncertainty of the transfer coefficient during the interval would no longer be important, as one would be using the same C for each of treatment calculation)? Based on the assumed value of SE_C used in your calculations, how much could the flux uncertainty be reduced by eliminating SE_C?

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Interactive comment on Atmos. Meas. Tech. Discuss., 6, 8959, 2013.