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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.

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Response to Anonymous Referee Comments 2 (RC C3413) to MS No. amt-2013-231: “The detectability of nitrous oxide mitigation efficacy in intensively grazed pastures using a multiple plot micrometeorological technique” by McMillan et al. in Atmos. Meas. Tech. Discuss., 6, 8959–9003, 2013.

We thank the two anonymous referees for their encouraging comments and helpful suggestions for improving this paper. We respond to each of the reviewers on a point-

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by-point basis in the following: (Anonymous referee comments in italics, our responses in regular font, relevant text from the current or revised manuscript in bold)

Referee 2.

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.

Agreed, and we have reduced the number of significant digits to 2

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).

We agree, and have now inserted the following sentence on page 8964 on line 10: "The flux gradient technique has long been established for the measurement of trace gas fluxes from agricultural surfaces, with early studies investigating CO₂ fluxes from wheat (Huber, 1952) and sugar beet (Monteith and Sceicz, 1960), and ammonia fluxes from grazed pasture (Denmead et al., 1974).

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

It is calculated for each measurement interval and in the manuscript we have clarified this point by inserting the sentence: **"The SEDN2O was calculated for each measurement interval resulting in 4371 determinations of this statistic during the**

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two experiments.”

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.

We feel that we have already discussed the equivalency of Kh, Km and Kg on page 8964 line 17, but to ensure that the term “aerodynamic FG method” is used here, we change the sentence beginning on line 13 on p8964 to read: bf “In this study, we use the aerodynamic flux gradient method where the flux of a gas, Fg, can be determined from the product of its vertical gradient above the surface of interest, (dCg/dz) and an eddy diffusivity term, Kg.”

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

We have inserted the parenthesised phrase: “**(calculated as 0.66 of canopy height)**”

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.

We have not seen significant negative gradients in measurements made in the relatively high nitrogen environment of pasture grazed by dairy cattle. In this study, the overwhelming majority (>99%) of the gradients were positive indicating that flux was almost entirely in the upwards direction. None of the negative fluxes were statistically significant. Very careful analysis of the timing of the sampling and transit from Z1 and Z2 was done to ensure we were not confusing air sampled at the upper limit with air sampled from the lower inlet. We have addressed the reviewer’s question by inserting the first sentence above into the paper.

Pg. 8974, Ln 22: Given the importance of $SE_{\Delta N_2O}$, I’m curious about its characteristics. Does $SE_{\Delta N_2O}$ scale on ΔN_2O ? Is the uncertainty better represented as a

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percentage of the measurement, rather than assuming a constant value (e.g., large ΔN_2O corresponds with large $SE_{\Delta N_2O}$? Could the assumption of a constant value for $SE_{\Delta N_2O}$ (0.023 ppb) lead to a too-conservative calculation for detectable flux levels?.

In Figure 1 of this response we show the relationship between $SE_{\Delta N_2O}$ and ΔN_2O . While there is a slight scaling, the relationship is not clear so taking the most likely value of $SE_{\Delta N_2O}$ from the ensemble population seems to be reasonable approach, and conservative compared with other studies that have cited the lowest recorded SE as their lower detection limit.

Pg. 8979, Ln 19: The value of $SE_{C_{Tr}}$ is given as 0.12. Does this have units, or is this the ratio of $SE_{C_{Tr}}/C_{Tr}$? Clarify. I'm guessing from the text that $SE_{C_{Tr}}/C_{Tr} = 0.12$. 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?*

The value is relative and therefore unitless. The confusion here was caused by an error in the manuscript where we provided the formula for the absolute, rather than the relative error in the transfer coefficient. This has now been corrected. The result is based on a Monte Carlo based approach to estimating the uncertainty associated with the input parameters to the diffusivity term. The study is concerned with random measurement error rather than the period to period variability which is likely to be more systematic (but not necessarily measured any less accurately). The study is now published (Mukherjee S, Sturman, AP, McMillan AMS, Harvey MJ, Zawar-Reza, P. Assessment of error propagation in measured flux values obtained using an eddy diffusivity based micrometeorological setup, Atmospheric Environment (2013), doi: 10.1016/j.atmosenv.2013.10.034) and we have updated the manuscript to cite this publication, and added further explanation in the manuscript.

Pg. 8985, Ln 8: I agree that if the FS-NOMAS system could be used to sequence

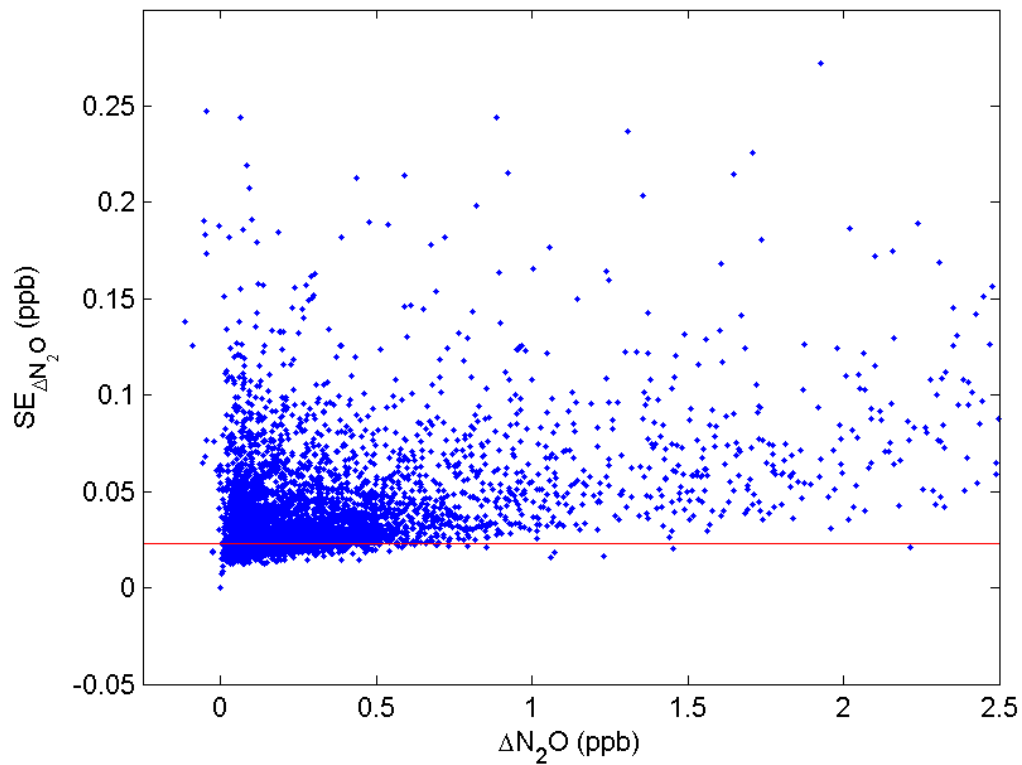
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between masts (mast 1, mast 2, mast 3, mast 4, mast 1, : : :), so that ΔN_2O for each treatment was estimated over the same time interval (e.g. 30 minutes), this could reduce the detectability limits of the flux differences (I assume the uncertainty of the transfer coefficient during the interval would no longer be important, as one would be using the same C_{Tr} for each of treatment calculation)? Based on the assumed value of $SE_{C_{Tr}}$ used in your calculations, how much could the flux uncertainty be reduced by eliminating $SE_{C_{Tr}}$?

Good question. The simple answer is that the uncertainty reduction would be by 12% of the absolute diffusivity and so the absolute reduction in flux uncertainty would vary with the magnitude of the diffusivity term. However we would gain additional uncertainty reduction by the added temporal frequency of the measurement. We felt that it is beyond the scope of this paper to estimate the additional measurement benefits of what is a hypothetical measurement system.

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

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