

Editor Comments to revised manuscript amt-2017-278

In the first part, referee comments are listed that have not been answered by the authors in a satisfactory way. Original referee comments (RC) and author responses (AR) are shown in blue. The new editor comments (EC) are printed in black.

In the second part, additional editor comments are listed that are not directly related to referee comments.

RC(#1): The only major addition I would like to propose is a broader discussion of the technique in the context of other techniques used to estimate gas exchange between land and atmosphere. In particular, I would like to see a comparison with the eddy covariance and the radon mass balance techniques (e.g. Biraud et al., 2002, Tellus, 54B, 41-60) in terms of their precision and the scale of the observed 'footprint'.

AR: *As indicated below, an eddy covariance component to the project was not possible but would be helpful in future efforts to evaluate the method. The fetch indicated study (Biraud et al, 2002) cannot be used in the current study since: 1) they assumed wind flowing (.....)*

EC 1) The authors gave some detailed consideration to this referee comment in their response, but did not provide any additional discussion in the revised manuscript. This is not satisfactory. As suggested by the referee, additional discussion should be included in the manuscript on the precision and the footprint of the mass accumulation method compared to other common micrometeorological methods.

RC(#1): Mass accumulations, first paragraph: Were the comparable fluxes cited here done in a similar climatic region, with similar land management (e.g. N fertilisation)?

AR: *I cannot determine where/what you are referring to.*

EC 2) This referee comment obviously refers to the first paragraph of Section 3.2, where the results of the present study are compared to Wagner-Riddle et al. (2007), Venterea et al. (2005) and Mosier et al. (2006). Please address the reviewer comment in the manuscript text.

RC(#1): Page 9, Discussion of lower N₂O accumulation compared to chamber fluxes: Another possible explanation is that chamber fluxes were measured during the day, when soils tend to be warmer than during the night. Other parameters being equal, N₂O flux from soil increases substantially with soil temperature.

AR: *Diurnal chamber flux measurements were made during this part of the season with measurements showing very little difference. I have added the results of the short study.*

EC 3) Despite the short additional study with diurnal/diel chamber measurements, the first sentence of Section 2 is still misleading ("...using three methods during the night between 2000 and 0400 local time...") because the main part of the chamber measurements were made during the day. Please reformulate this sentence to make it more accurate.

RC(#2): 14. Page 6, line 9. It would help the audience to use consistent units. In this paper, most readers would really prefer that you use units such as $\mu\text{mol}/\text{m}^2/\text{s}$ throughout. The fertilizer community often uses mass of N, but mass units really don't help this paper (and you use mass of N₂O, not N). In this particular line, we are given a concentration in $\mu\text{L}/\text{L}$ and then you switch to gradient of mg m^{-4} .

AR: *I have changed all flux units to $\mu\text{mol}/\text{m}^2/\text{s}$, $\text{nmol}/\text{m}^2/\text{s}$, and equivalent accumulation units.*

EC 4) I agree with the revised units. However, at some positions in the manuscript the old units still need to be changed: e.g. in the abstract and in Section 3.1. Please check the entire manuscript for consistent units.

RC(#2): 16. Page 8, line 26. The literature reported in Table 1 is quite selective. Please tell us why you chose these specific papers.

AR: *Including every paper would be pointless since this is not a review paper. I sought out*

representative studies (similar crop conditions and soils) that used good techniques.

EC 5) Include a statement about the literature selection in the text or in the Table caption.

RC(#2): 19. Page 10, line 9 and 12. It looks like the accumulation method was a factor of 2 to 5 less than the chambers. These statements appear to mislead that they were close.

AR: *I have added a table (Table 5) illustrating the differences and changed the text to clarify.*

EC 6) The introduction of Table 5 with a comparison of time dependent averages is a useful improvement of the manuscript. However, I have difficulties to identify some of the averages in Table 5 with the data points shown in Figs. 7 and 8. It should be clarified, which data sets (symbols/colors in Figs. 7/8) were included in the calculation of the Table 5 averages. Moreover the use of the term "comparable" for the indication of agreement between the different flux methods is very non-specific and non-quantitative (e.g. Page 9, line 11/13; Page 10, line 15 in the revised version). This should be rephrased in a better way.

RC(#2): 25. Figure 6. If this is an accumulation starting at 1900, why don't the accumulations start at zero?

AR: *Axis label fixed*

EC 7) The renaming of the y-axis labels to "Accumulation domain ..." is confusing and does not satisfactorily address the referee comment. This needs to be improved.

RC(#3): Page 1 Line 6: Annual emission budgets.

AR: *A budget would include sinks. Unclear what an emission budget would be. Not changed.*

EC 8) I think the referee wanted to point out that some greenhouse and other gases show a bi-directional behavior (unusual but sometimes occurring also for N₂O). This should be considered with such a general statement.

RC(#3): P4L22 state the Schmidt number, if a constant was used

AR: *0.91 for CO₂ and 0.95 for N₂O- added*

EC 9) Given that the observed K_c values listed in Table 3 are much larger than the molecular diffusion limit, the turbulent Schmidt numbers rather than molecular Schmidt numbers should be used here!? Please explain.

RC(#3): P5L21: "at 8m" is duplicated

EC 10) Still needs to be corrected (at Page 5, line 29 in the revised version)

ADDITIONAL EDITOR COMMENTS

(page and line numbers refer to the revised manuscript version)

EC 11) Page 1, line 13-14: "Fluxes during calm nights ...". It is not clear which flux results are presented here, because in the previous sentences both accumulation and diffusive fluxes have been mentioned. Please specify.

EC 12) Page 1, line 26: Why is the 'integrated horizontal mass flux method' not usable under calm conditions? It does not use any turbulence parameters.

EC 13) Page 2, line 21-25: It should be mentioned that a pure molecular diffusion (without intermittent turbulence) virtually never happens in the real world surface layer (as also indicated by the K_c results in Table 3).

EC 14) Page 3, line 13-17. It is quite difficult for the reader to understand on which fields the described fertilisations have been applied. Moreover, the information in line 17 ("spring pre-plant application of 200 kg/ha on a field directly south") seems to contradict the information on Page 9, line 4 ("The field south of the tower, on which no N was applied during the year

..."). Therefore please clearly mark and number the mentioned fields in Fig. 1 (maybe the image section has to be adjusted for that) to illustrate their relevance for the present results.

EC 15) Page 3, line 19: Here a lower measurement height of 2.8 m is specified while later in the text (Page 4, line 9) a height of 3 m is indicated. Which one is correct?

Please make sure that coherent values for the measurement heights are used throughout the entire manuscript.

EC 16) Page 4, line 16: How did you determine the MDL for u^* ? In my experience, this MDL value is too small. Like the trace gas fluxes, also u^* (and K_c) determined by eddy covariance is affected by large uncertainties under calm (intermittent turbulent) conditions. Therefore the uncertainty of u^* is mainly limited by intermittency (non-stationarity) rather than by sensor precision. This issue needs to be discussed.

EC 17) Page 4, line 29-30: I assume that the reference to Mosier et al. (2006) refers to the "vented static chamber method" and not to the "two months of measurements"! If true, the reference should be placed earlier in the sentence.

EC 18) Entire text: the use of the concentration units nLL^{-1} , μLL^{-1} is very unusual in the trace gas flux literature. It is also not a SI unit and not consistent with the flux units used in the manuscript. I would recommend to use $nmol\ mol^{-1}$ (or ppb) and $\mu mol\ mol^{-1}$ (or ppm) instead, without need to change the numerical values.

EC 19) Page 7, line 18 and Page 8, line 7: In the available pdf-document, some letters/symbols are not displayed properly.

EC 20) Page 8, line 11: Correct reference to "Mahrt"

EC 21) Page 8, line 13: "with or without estimated diffusion" is somewhat confusing. Better use the formulation in Table 4: "with or without measurable diffusion". In addition the statement in this sentence needs some explanation.

EC 22) Page 9, line 8: Wrong section numbering. Change to "3.4"

EC 23) Page 9, line 25: Why "above-canopy accumulation flux"? According to Eq. 1, the accumulation flux was calculated within and above the canopy!

EC 24) Page 10, line 14: The second part of the sentence lacks a verb.

EC 25) Page 10, line 16/17: How could there be unstable conditions, when at the same time the diffusive fluxes were not measurable and the temperature gradients (acc. to Table 3) were positive? Do you consider the observed stability sign as significant?

EC 26) Table 2 footnotes: Λ is the 'Obukhov length' (not stability, which would be z/Λ)

EC 27) Table 4: Obviously the original gradients in the 3rd/4th column have been replaced by gradient fluxes in the revised version. Accordingly the header should be changed to "Gradient flux at ...". Also the indicated units in these two columns seem to be wrong (not appropriate for indicated values). Why are the CO₂ gradient fluxes so large? Please use appropriate and consistent flux units in Table 4.

EC 28) Fig. 7&8: It is unclear, where the chamber measurements labelled "North fld", "South fld", and "no N" have been performed. Please explain and indicate the position of the different chamber measurements in Fig. 1b.