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Interactive comment on "Ammonia emission measurements of an intensively grazed pasture" *by* Karl Voglmeier et al.

Anonymous Referee #2

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This manuscript is an important contribution to the field of ammonia emission evaluation and more widely nitrogen management in agriculture. Indeed, this manuscript presents, evaluates and analyses the results of a field scale methodology for evaluating nitrogen losses as ammonia volatilisation from two intensively grazed pastures in Switzerland. The method is based on inverse Lagrangian dispersion modelling and line integrating open path DOAS instruments. Two cow herds with differing nitrogen diets are compared over small fields ensuring a quite high grazing density hence insuring a good sensitivity of the method. The uncertainties of the methodology are evaluated and the homogeneity of the dung patches is assessed based on camera monitoring and observations. The inverse dispersion methodology is also evaluated against a controlled ammonia source of similar dimension as the fields and proves to work quite well.

This study convincingly demonstrates that N-rich diets lead to larger emissions than N-balanced diet (about 30% larger). The methodologies are scientifically sound and the methods and results are well presented, although some clarifications are needed both on the methodology and presentations (see comments below). The figures and tables are clear, and the manuscript is well written. The topic is of great interest for both the scientific community and decision makers.

General comments

* The methodology used for gap-filling emissions during night-time (and to a lesser extent for wind sectors coming from the surrounding farms) may be questionable. Indeed, the authors assume that night-time fluxes may be gap-filled based on day-time fluxes, but ammonia fluxes are fundamentally based on thermodynamical equilibrium at the surface (gas-liquid and acid-base equilibriums). This means that (1) the surface ammonia concentration is exponentially increasing with surface temperature due to the gas-liquid or Henry equilibrium, and we would hence expect lower emission at night due to lower temperature and hence lower concentration at the surface; (2) similarly, since ammonia fluxes are proportional to a concentration difference between the surface and a reference level, lower turbulent exchanges at night are expected to decrease night-time ammonia emissions. This means that using daytime fluxes amplitude and dynamics may systematically bias the emissions towards higher values. I would recommend a discussion on that point which may include a study on the temperature and u* dependency of the ammonia emissions. Authors could refer to e.g. Flechard et al. (2013) for details on ammonia the points raised above.

* Reference to the work of Moring et al. (2016) is lacking. In reference to this work, I wonder if considering the source as a mosaic of emission and deposition hot-spots rather than a distribution of emission patches would conceptually change the results presented here. Could the author elaborate on this question?

* The authors have made a good job in synthesising complex experiments and method-

ologies, though there is sometimes a feeling of over-simplification, which makes the overall evaluation of the quality of the results difficult. I would hence recommend adding a supplementary section to provide some details, in particular regarding:

(1) The uncertainty analysis requires more details and especially on the gap-filling of emissions using the standard curve on Figure 5. An example showing a reconstructed emission would be beneficial here. It is difficult also to understand if the uncertainty analysis on gap-filling spans the actual variability in the fluxes shown in Figure 5 (the error-bars in Figure 5 would also need to be explained).

(2) The methodology used to derive the dung patches distribution, the way relative deviation of this dung patches Is calculated and the way it is used to correct cow-based emissions all need clarifications. I think authors should mention previous work on patches emissions by Moring et al. (2016).

(3) The description of the artificial source quality would benefit from more details on the homogeneity of the emissions, the pressure and flow rates stability. Some more examples on measured concentration and retrieved emissions during these trials would also be beneficial as these data were not published previously (to my knowledge).

Detailed comments

* P4-L7: delete “important” before reference spectrum

* P4-L14-20: some details on the meteorological instruements may be useful. Please evaluate also how important may be high frequency losses on u* and H with 10Hz acquisition at 2 m above ground.

* P4-L25-30: 44%-49% missing values for low ustar may actually bias the analysis (see major comments)

* P5-L6: please give number of thousands of trajectory

* P5-L9: please give units of E, C and D. I woud also suggest to explicit the hypotesis

behind this equation: actually Cdown = D*S + Cup, where not other nearby sources are assumed.

* P5-L15-18: are the two hypothesis of a uniform and continuous distribution and of a random uniform distribution of sources strickly identical for inverse dispersion?

* P5-L31: the work from Moring et al. (2016) should be referred to here and after.

* P6-L4-8: the method used correct the cow-based emissions based on images and GPS needs to be detailed, as suggested in the major comment section.

* P8-EQ-2: The meaning of ti is unclear.ïĄĎCUD and Dup and Ddown should also be time dependent. Please clarify. I would suggest rather using t and Edef, i(t) etc.

* P9-L11-15: I woulld suggest showing the concentration inter-comparison figure in a supplementary material.

* P9-L28: please explain what is a “systemic” unncertainty.

* P10-L2: I am not sure the word “stable” is appropriate here as it may be understood as “stabe thermal stratification”.

* P10-L7-10: I wonder what is thevariablity in the release rate between the critical orifice. I also wonder if the atmospheric pressure has an influence onf the realese rate at 30 minutes but also over short time scales (seconds). Finally, what is the expected (or even recorded) effect of wind speed variations on release rates : would one expect some ventury effects on the release rates?

* P10-L28-32: It is quite unclear what the “relative deviation of the dung density” really is. I would suggest providing the exact equation.

* P11-L1-3: it is unclear how exactly missing values are obtained from regression analysis. Could the authors elaborate on that?

* P11-L6-12: I would suggest giving details on how the uncertainties are aggregated

(may be an equation in a supplementary section?)

* Table 2: I would suggest finding a way to separate more clearly G and M in this table as in Table 3

* Table 3: I suggest only proving 1 digit for temperature and none for rainfall.

* Figure 5: could you specify the meaning of the error-bars. I would also suggest using negative time values on the left.

* Figure 8: Could you provide error bars on both released and inverse modelling with measurements. I would also suggest changing one

* Figure 10: Please explicit the term “relative deviation” in the legend.

* Figure 11. Please explicit if error bars are standard deviation, standard errors or interquartile.

References

Flechard, C.R., Massad, R.S., Loubet, B., Personne, E., Simpson, D., Bash, J.O., Cooter, E.J., Nemitz, E. and Sutton, M.A., 2013. Advances in understanding, models and parameterizations of biosphere-atmosphere ammonia exchange. Biogeosciences, 10(7): 5183-5225.

Moring, A., Vieno, M., Doherty, R.M., Laubach, J., Taghizadeh-Toosi, A. and Sutton, M.A., 2016. A process-based model for ammonia emission from urine patches, GAG (Generation of Ammonia from Grazing): description and sensitivity analysis. Biogeosciences, 13(6): 1837-1861.

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