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Biogeosciences Discussions

Interactive comment on "The annual ammonia budget of fertilised cut grassland – Part 1: Micrometeorological flux measurements and emissions after slurry application" *by* C. Spirig et al.

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Response to anonymous referee #2

We thank anonymous referee 2 for the helpful comments. In particular, the suggestions for modifications of the methods/results section proved as a valuable guidance for improving the overall structure of the manuscript. We addressed all comments and followed the referee's suggestions for technical corrections with our revisions. Below we list the replies to all other reviewer comments.

Comment: Some "Materials and Methods" items are described within the "Results" part. They should be re-arranged, as suggested in the following (specific comments to sections 3.2. o 3.4.).
Reply: Paragraphs 3.2. to 3.4. were rearranged based on the referee's comments. However, we decided not to follow the suggestion of moving most of section 3.3 to the MM section. It does not belong in the MM section before the fluxes are described; indeed 3.3 describes the interpretation method that was adopted as a 'result' of the incomplete flux capture. Therefore, instead of moving parts of 3.3. to the methods section, we decided to take parts of 3.2 and introduce a new section 2.2.3. "Operation of the gradient system during slurry applications". Furthermore, we renamed chapter 3.4 for clarifying the different focus of 3.2.(flux measurements) and 3.4 (Cumulative losses).

Comment to p 9597, l 27 and followings: the value of the uncertainty should be quoted, or even a frame reporting its variation above the one of the repartition should be added in figure 6
Reply: Plots of both the absolute and relative error of the flux were added to figure 6 in the revised version.

Comment to p 9598, l 10: it should be explained how "a precision of concentration measurements" can be translated in "a flux detection limit", rather than in a precision of flux measurement (or term changed)
Reply (see also corresponding reply to referee1): This part has been extended and it is now explained how the flux detection limit was derived and under which conditions it applies. Under unstable and near-neutral conditions at concentrations near background levels, the precision is dominated by the uncertainty of the concentration difference measurement (as shown in Fig. 6). The latter precision is is 0.14 $\mu g\,m^{-3}$ (assuming 0.1 $\mu g\,m^{-3}$ precision for an individual concentration measurement), thus we take 0.28 $\mu g\,m^{-3}$ as the minimum detectable concentration difference. For the mentioned turbulence situations, typical flux/concentration difference ratios (usually referred to as

transfer velocity) were 0.015 to 0.03 ms^{-1} which translates to flux detection limits of 4.2 to 8.4 $ngm^{-2}s^{-1}$.

Comment to p 9600, l 10: equation 11 should be made more explicit, with both partition equations developed for example; the authors should be aware of the fact that they do not account for adsorption to organic matter (grass or slurry itself) which would decrease the concentration calculated
Reply: The revised text now includes the details on both the chosen Henry and acidic constant (new equations 11 and 12). It is true that accounting for adsorption of organic matter would affect the dissociation constant with the result of reducing the air concentration in equilibrium with slurry ammonium. On the other hand, we used pH as measured in slurry samples although it has been reported that slurry pH on the field typically increases during the first hour after application (Sommer and Hutchings 2001), which would increase the air concentration.

Comment to p 9601, l 9: the authors should give quantitative information on "smaller " and "larger"
Reply: The revised version now includes quantitative information on the lower and upper limit estimates in these particular cases as compared to the "measured" cumulative losses.

Comment to p 9602: methodological considerations should be more clearly separated on the ones on the device (from l 7 to l 19) and the ones on the methods for flux measurement/calculation (from l 25)
Reply: The two parts were more clearly separated by beginning a new paragraph for the methodological considerations, and also by explicitly mentioning that the "second problem" is not instrument-specific.

Comment to p 9605-9606: "5 Conclusion" the authors could have highlighted the possibility to use in a more generic way the methods developed in this paper (i) to take account of heterogeneous distribution of the slurry at the beginning of application and

(ii) for ammonia fluxes gap-filling
Reply: The chosen approach was the best we could do for assessing the magnitude of cumulative losses in this particular case. However, promoting these methods in a more generic way should be based on a data set with less data gaps in the initial phase and parallel independent measurements with a mass balance approach. Such an intercomparison is required for thoroughly discussing the validity of the chosen methods. Additional measurements at the Oensingen site were performed in 2009 (including mass balance approaches using integrative measurement techniques), specifically aiming at these questions.

Comment to table 2: Could the authors add a column giving the mean wind direction encountered during the volatilization event?
Reply: We are not sure whether we understand the reason for this suggestion. Maybe the reviewer looked for information supporting the footprint correction? There was no simple relationship of the mean wind direction (relative to field orientation) and the overall footprint correction. This is not too surprising, as the footprint is not only influenced by wind direction, but also by wind speed and stability. Furthermore, the strong dynamics of the emissions during the slurry application events make it difficult to relate the overall volatilization to a mean wind direction. Since we do not see the benefit of adding this information, we decided to leave table 2 unchanged (besides the other technical corrections as suggested by the referees).

Comment to p 9622: Figure 7 Rain is not easy to see
Reply: We agree that it is not very prominent, but for reasons of compactness of the plot we preferred not to change the corresponding scale – after all, it was not a heavy rain event, and exaggerating by using a different scale would be misleading.

Interactive comment on Biogeosciences Discuss., 6, 9583, 2009.