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Interactive Comment

Interactive comment on "Ammonia sources and sinks in an intensively managed grassland using dynamic chambers" *by* M. David et al.

M. David et al.

Received and published: 24 July 2009

We thank both reviewers for their constructive comments on our manuscript

As a whole, all typos mentioned by the reviewer have been corrected. They will not be recalled here.

In the following, texts in Italics refer to a quotation of the reviews. Our replies follow with one or more bullets. The quotations of additional text are in inverted commas.

Reply to Reviewer 1 General comments

COMMENT: One fundamental concern is the apparent lack of replication. … How can the authors justify that the results are not simply a matter of spatial heterogeneity across the experimental plot rather than management responses. This needs to be clarified in the presentation and discussion of the results



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REPLY - Actually there was no replicate for the different experiments. This was mainly because it has not been possible to manage more than three chambers at a time. We gave the priority to comparing different treatments. The observed difference were larger than the chamber accuracy and we could consider that they were also larger that the possible difference due to local heterogeneity (see comments below). Moreover a large part of the conclusions were derived from changes in conditions on one chamber. In this case the question of replicate is not so crucial. To explain and discuss this we added the following comments: o At the end of section 2.2 Experimental conditions and treatments: "Due to experimental constraints, it was not possible to run more than three chambers at a time. Consequently we could not make replicates for the different treatments in order to circumvent a possible effect of e.g. soil heterogeneity. However, to address the issue of the measurement precision, the chambers were tested prior to the field experiment in a greenhouse using a calibrated NH3 source. The estimated NH3 flux was within 10% of the input from the source. Moreover throughout the experimental period, one treatment (F1) was taken as a reference to ensure comparability between the different experimental runs. Finally, for some analyses, it was a change in conditions of one treatment (i.e. in one chamber) which was studied rather than a comparison between chambers. In this case, the problem of local heterogeneity and the need of replicates do not have the same level of importance." o At the beginning of the discussion (section 4): "Even though no replicates could be made for the different trials, most of the results showed significant differences or a clear trend after a change in conditions (Fig. 2). Moreover the F1 treatment which was applied over all the periods on different places showed little variations, which give an indication that spatial and time variability was certainly not large in the context of this field. The same applies for the treatment F5, which was applied twice on two different locations and gave similar trend when compared to F1. This gives confidence in the effects that were observed." o The standard error of the measured fluxes and environmental conditions were calculated for the two treatments which had replicates (F1 and F5) and showed a variability of around 25%.

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COMMENT: The title needs to be changed to be more informative about content of the work.

REPLY: - We agree with the suggestion of the reviewer, but do not think that an explicit reference to soil, plant and litter is necessary or helpful in the title. We propose the following title: Ammonia sources and sinks in an intensively managed grassland canopy.

COMMENT: I miss some hypothesis for the work. The aims are specified, but what are the hypotheses you are going to test. Also the focus on the extraction procedure as a mean to predict emission should be included in the aims of the work

REPLY: - Hypotheses of the study were added in the introduction: "The purpose of the present work was to check how NH3 fluxes integrate at the canopy scale in such a complex canopy as grassland. More specifically, this work aimed at assessing the hypotheses, suggested by former studies, that also in grass canopies NH3 would be emitted by the litter and recaptured by overlying leaves, and check whether the soil itself was a source or not"

COMMENT: Exclude results with the two French soils

REPLY: - We agree that the experiments and results over the two French soils does not add much to the paper except that it allows generalisation of the results found in Figure 5, which links the NH3 flux to the evaluated compensation point concentration. We hence think it is important to keep these data.

COMMENT: Perhaps an issue of writing philosophy, I'd recommend to move the data presentation given in Discussion (section4.4) into the results

REPLY: - The first paragraph of section 4.4 and results presentation have been moved to the Result section (3.4) - The rest of section 4.4 has been left in the Discussion, with additional comments.

Specific comments:

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COMMENT: 1 Introduction: Editing is needed in last paragraph

REPLY: Done

COMMENT: 2 Materials and methods. 2.1 Dynamic chambers: … what do you mean by surface?

REPLY - "base area" has been added in the text

COMMENT: The surface area of chmbere used for litter is not listed in Table 1?

REPLY: - For the litter experiment with C2 chambers, the surface area is not given because the measured fluxes do not refer to a canopy in place, but to an amount of vegetation in the chamber. However, the dimensions of the chamber are given in the text.

COMMENT: It would also be helpful if you mention Table 1 here

REPLY - Done

COMMENT: (ECN, Petten, NL): add reference, it's given later, but should be included here

REPLY - The reference for the Amanda analyser (ECN) has been added : Wyers et al. (1993)

COMMENT: The flow rate was measured. Delete, this information is already given in the text above

REPLY - The detail on flow rate has been removed

COMMENT: 2.2 Experimental conditions: : (Sutton et al.1) suppose the uppercase 1 refers to a footnote, but the footnote is missing

REPLY - The footnote referred to a note in an earlier page. It has been suppressed, as the paper is now published.

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COMMENT: Suggest Table 3 and Table 4 are reversed in order to follow logical order

REPLY - Table 3 and 4 have been reversed, as suggested

COMMENT: 2.5 Ammonia emission potentials: delete μ g in parenthesis

REPLY - Done

COMMENT: 3 Results 3.1 Plant and soil NH4+: First and second paragraphs: repetition of results already given in Table should be avoided

REPLY - The results given in the Table were suppressed from the text in both cases.

COMMENT: 4th paragraph: suggest to rewrite sentences …

REPLY - Your suggestion has been adopted. Thank you.

COMMENT: 3.2: Measured emissions from soil. Please explain to the reader how average and median emission were calculated

REPLY We believe that 'average' and 'median' are clearly defined terms (we used the standard definitions). The calculation was done accordingly and do not require additional explanation.

COMMENT: 3.3: Third paragraph: the unit for leaf area based NH3 emissions needs to be clarified

REPLY We agree that this is somewhat confusing. In the field experiment, the fluxes were directly calculated for land area (area of the chamber, i.e. 0.04 or 0.09 m^2). In lab experiment referring to the chamber area had no significance, because the measurements were made on vegetation litter brought from the field. In Table 5, the fluxes from CL1-3, originally in ng (m² leaf area) 1 s-1 were recalculated by multiplying with the LAI observed in the field experiment, for allowing comparison with the fluxes measured in the field (F1-F7). This has been clarified in the text: “For comparison with F1-F6 data, the fluxes measured under controlled conditions were scaled to the LAI

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measured in the field. The NH3 emissions was 41 ng m 2 s 1 NH3 on average, and maximum 95 ng m-2 s 1 NH3 (Table 5),…" and in Table 5 legend. We also agree that we got confused between the values scaled with LAI and values scaled with the soil surface. Thank you for noticing this. This changed the relative magnitude of the emissions and we have corrected the text correspondingly: "The NH3 emissions was 41 ng m 2 s 1 NH3 on average, and maximum 95 ng m-2 s 1 NH3 (Table 5), which is similar in magnitude to fluxes measured in (F6) and (F7), although the N content was smaller (Table 3) which may be explained by the lower nitrogen fertilisation in CL1-CL3 than in F1-F6."

COMMENT: 4 Discussion 4.1 Green leaves: you cite Van Hove et al. (2002) for having found larger emissions over. Suppose this should read compensation points.

REPLY - Yes, this has been corrected

COMMENT: 4.2: Suggest this is rephrased: The missing fraction …

REPLY - This was done according to your suggestion

COMMENT: Also, here you only refer to CS1, which emphasizes the argument that CS2 and CS3 can be omitted

REPLY - We agree that the results on the other soils (CS2 and CS3) did not add information. They were removed from the text and Table.

COMMENT: 4.3: Litter NH3 emission and relative humidity … It is known that microorganisms in the form of fungi and bacteria are abundant on leaf surfaces. Perhaps it is not only a matter of plant derived N, but also from microbial cells on the surface that NH4+ is derived. It would be interesting if the authors could go a bit into detail on this possible source of N

REPLY - With our approach at plant scale, we cannot do the distinction between microbial cells and vegetation cells. We can consider that they are all intrinsic parts of the plant and all contribute to ammonia fluxes. Moreover, they are certainly not a source of

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variation between the different parts of the plant. We just added: "The degradation process leading to NH3 emission is due to biochemical and microbial processes the leaf surface and inside the leaf (Farquhar et al., 1979), but it was not possible in this study to make the share between these two contributions."

COMMENT: 4.4: Emission potentials: first paragraph could be moved to Results

REPLY - The first paragraph was moved at the end of the Results section (3.4) with the same title. - The rest of this section was slightly modified and left at the end of the discussion

COMMENT: Table 3 (now 4): suggest the presentation of data follow the order (cut grass)-(main field)-(hay) to harmonize with the order in the text

REPLY - This was done

COMMENT: Table 4 (now 3): there is an inconsistent use of decimals …

REPLY - This was corrected

COMMENT: Table 5: Please explain the unit for leaf fluxes (see above). What is the meaning of the asterisks with the units?

REPLY - The units were explained (see comments on section 3.3) - The asterisks referred to the soil area or leaf area, but the explanation was lost. It is now detailed in the legend and the text. So the asterisks have been removed.

COMMENT: Figure 1: Some data points in (F6) and (F7) are not interconnected. Please explain why.

REPLY - The points which are not interconnected are due to missing samples, due to the sampling system malfunctioning.

COMMENT: In the text, it is suggested to include abbreviations for soil and plant temperatures

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REPLY - Abbreviations have been included

COMMENT: Figure 2: Modify text, as for figure 1

REPLY - This has been done

Reply to Reviewer 2

General comments:

COMMENT: There is however a point that I find should be expressed more clearly and discussed in larger detail, that is the use of "zero" air. … I suggest that it is pointed out already at the beginning of the discussion …

REPLY - This basic concept of our chamber system has been specified in the introduction by adding the following sentence: "The dynamic chambers were supplied with ammonia-free air in order to derive an emission under standardized conditions that could be considered as an emission potential and best compared to emission potentials estimated from plant apoplast extracts (Mattsson et al., 2009)". - This point is also recalled in the Material and methods section: "The air injected into the chambers was scrubbed of NH3 for two reasons: to avoid discrepancies between experiments, so that the results would not be influenced by the concentration of ambient air, and to estimate a reference emission. As a matter of fact, the compensation point of vegetation such as grasslands is often on the same order as the ambient concentration in agricultural areas. Moreover, this allowed for better precision in flux measurement and a simpler system since only one NH3 concentration measurement was required in the chamber. Under such conditions, only emissions can be measured in the chamber."

COMMENT: Maybe it should be mentioned in the title, e.g. "Potential ammonia sources and sinks …"

REPLY - We prefer not to include the term "potential" in the article title. the experimental approach does not give strictly speaking a potential, as the results

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presented are not only "potentials" and the fluxes are also influenced by temperature.

Specific comments:

COMMENT: p. 1628, I3: the definition of litter is somewhat strange. Normally, litter is only dead decomposing leaves lying on the ground. I wonder whether the two fractions included in the definition used here are emitting equal amounts of NH4+?

REPLY - We considered that litter was composed of all non-living leaves. We agree that this comprises leaves at different stages of decomposition with different potential for ammonia emission. However, the decomposition process is continuous and it would have been impossible to find an objective limit between senescing and dead leaves. We added some wording at the end of the introduction to explain what we meant: "litter (hereafter defined as senescing attached leaves, dead or decomposing detached leaves)"

COMMENT: p. 1628, l. 19: it is difficult to imagine how the chamber actually look like. A sketch or a photo would be useful

REPLY - We inserted a sketch and a photo as Fig. 1 in the text. The following figures have been renumbered accordingly.

COMMENT: p. 1629, I. 5-10: The conditioning of the air blown into the chambers are probably quite determining for the actual flux measured. As mentioned above, I missed a thorough discussion of this in the paper

REPLY - some precisions have been added in the introduction and at the beginning of the Material and methods sections (see above in the reply to General comments)

COMMENT: p. 1630, l. 27: "air relative humidity" should be "relative air humidity"

REPLY - We propose to use "relative humidity" instead of "air

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relative humidity".

COMMENT: p. 1632, I. 19: "stomatal extracts": I suppose these are the analysis of the apoplastic concentrations?

REPLY - Yes, this is the analysis of the apoplastic concentrations. It was corrected in the text.

COMMENT: p. 1641, l. 16: I miss some statistics for the relationship shown in Fig 4. I suggest to inclde the result of a linear correlation.

REPLY - The equation (power function) of the regression line in fig. 5 has been added.

Interactive comment on Biogeosciences Discuss., 6, 1625, 2009.

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