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Interactive comment on "Ammonia emissions from a grazed field estimated by miniDOAS measurements and inverse dispersion modelling" *by* Michael Bell et al.

Michael Bell et al.

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We thank the reviewer for the constructive and helpful comments. We are pleased that they believe that the paper is well-structured and written, recommending publication after minor revisions. In the following we address the comments and suggestions point-by-point. In addition we have corrected the small number of minor grammatical changes annotated on the manuscript.

General remark 1: Most importantly, I think it would be good to present a general "expectation" based on the forward modelling more clearly. Under "normal" neutral stratification of the surface layer emissions are taken up by the atmosphere and vertically (and horizontally) dispersed. Thus, the concentration will generally increase relative to

the background (S2), and in ideal circumstances with homogeneous emissions, S3 will generally be higher in concentration than S1. Also, the concentration will not increase linearly with distance, since vertical dispersion takes place. A clear description of this concept will help the reader to understand the need for non-homogeneous emissions in Period 2.

-> authors: The reviewer makes a good point, we have added a few sentences more to further clarify the inverse dispersion theory in section 2.4. We have also added more detail to our explanation of the need to address the emissions as heterogeneous in section 3.3.2.

General remark 2: the formulas sometimes lack units

-> authors: We have addressed this remark in the revised manuscript (see comments 6 & 8).

General remark 3: A third remark concerns the choice of Period 2 for the Q/EF estimates. This period is characterized by a more uncertain background. The reason given (Period 1 is too short) I find not convincing. More emphasis should be placed on Period 1, which seems more consistent from a methodological point of view.

-> authors: The emission estimates made during Period 1 are not suitable for an emission factor due to the high likelihood that emissions continued to occur from the plot long after the Period 1 measurements ended. Therefore there was a significant fraction of emissions from excretions deposited to the field during Period 1 that were not captured by the Period 1 downwind concentration measurements, because the Period 2 grazing period began immediately afterwards and the concentration receptors had been moved to different field boundaries. An emission factor based on the Period 1 emissions would largely underestimate the total emissions from grazing due to the Period 1 receptors "missing" the delayed emissions from cattle excretions. After the Period 2 grazing period, the miniDOAS receptors were left in place for several days, this allowed the Period 2 receptors to capture residual emissions from the excretions,

and thus provide a more complete estimate of the total emissions from grazing. It is for this reason that we believe that a Period 2 emission factor is more representative. However, the emissions from both periods 1 and 2 are presented, along with emission factors expressed in multiple ways for both. This reasoning for the selection of the Period 2 emission factor is explained thoroughly in the discussion section.

Comment 1: Page 3, line 71 - maybe redefine in main text.

-> authors: Done.

Comment 2: Page 4, line 131 - This sentence is extremely vague. It forces readers to go to the Sintermann publication. It is unclear what the slope is in this context. Please be more specific here.

-> authors: This sentence has been rephrased for clarification.

Comment 3: Page 5, line 153 - So, why would you need temperature at 1.4m and 2m. Are these compared, or is this second measurement more specific?

-> authors: The measurement at 1.4m height is the fast temperature measurement component of the sonic anemometer, logged at 20Hz and later processed by eddy covariance software. The temperature and relative humidity sensor at 2m height (HMP45C, Campbell Scientific) is the better sensor for recording changes in ambient temperature (more accurate absolute temperature readings). This has now been clarified in the text.

Comment 4: Page 6, line 174 - The description provided here does not clarify the reason for using the absolute value of 2/w0 in the calculation. Either refer again to the Flesch paper (for more details ....), or provide a more detailed description here.

-> authors: Done, we have now clarified the w0 term in section 2.4.

Comment 5: Page 6, line 176 - Also here, it remains unclear how these parameters influence the source estimate. At least you should mention here that the vertical mixing

depth (i.e. the turbulence) is the prime factor in linking a concentration enhancement to a source strength.

-> authors: We have addressed this comment in the revisions following general remark 1.

Comment 6: Page 7, line 236 - Is T in celcius? Please provide units for alpha and beta.

-> authors: Units for T and beta are added, alpha is unitless.

Comment 7: Page 8, line 258 - I think the authors are aware of the accepted vision that managed grasslands have bi-directional exchnage of ammania: at periods of high temeratures emissions may occur, while at other periods emissions are monitored over the same field. This view has led to bi-directional exchange models (Fowler ..,... Kruit,). The authors should at least mention this in the paragraph. and mention that the current implementation is a simplification of what is known. 1. Kruit, R. J. W. et al. Modeling the surface-atmosphere exchange of ammonia. Atmos Environ {44}, {945–957} (2010). 1. Fowler, D. et al. Atmospheric composition change: Ecosystems–Atmosphere interactions. Atmos Environ 43, 5193–5267 (2009).

-> authors: We have added a few sentences to section 2.6 explaining that surface-atmosphere exchange is bi-directional and the uni-directional resistance model approach is a simplification. We thank the reviewer for the references provided.

Comment 8: Page 9, line 285 - I think it is instrumental to give units of the variables. RH in %, T in Kelvin? Cb in ug/m3? u in m/s?

-> authors: Units have been added to equation 5.

Comment 9: Page 9, line 289 - Should QS5 in figure 4 be QS3?

-> authors: Yes, this mistake has now been corrected.

Comment 10: Page 10, line 239 - Here it seems odd to me that the range line in the figure on May 27 is above the lue and the black (implying negative emissions). Would

be logical to set cb to the minimium of the measured concentrations.

-> authors: The predicted background concentration (Figure 6, orange line) does not agree strongly with the measured background concentration. The predicted Cb exceeds the downwind S3 concentration measurements (blue line) on May 27 as a result of this. However, on May 27 we had active upwind concentration measurements (S2, red line), thus the measured Cb was used in the emission estimates. The sensitivity of the emission estimates to Cb uncertainty is explored in detail (Table 4, Section 4.4.2).