

Interactive comment on “Determination of field scale ammonia emissions for common slurry spreading practice with two independent methods” by J. Sintermann et al.

Anonymous Referee #1

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General Comments

This manuscript reports NH₃ emissions measurements with two micrometeorological approaches, using separate instrumentation for each. One of these is eddy covariance (EC), carried out with careful footprint considerations; the other uses open-path concentration measurements to feed a backward-Lagrangian stochastic (BLS) model for emissions computation. Both methods are not novel in principle but they are carried out with state-of-the-art instrumentation, their respective implementations contain novel aspects, and comparing them appears a worthwhile undertaking. Also novel is their application to measure rather transient emissions from a real-world agricultural

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practice. The subject is thus excellently suited for Atmos. Meas. Tech.

The emission rates derived with the two methods agree with each other within 30 %. As an independent check, these emission rates are used to predict NH₃ concentrations at the locations of impingers that were operated simultaneously. For the EC method, the predictions are found to agree excellently (bias-free) with the impinger measurements. For the BLS method, there is reasonable agreement with the impingers overall, but some apparent effects of measurement height are present.

The paper is well-written, the analysis and interpretation are very thorough, there is adequate discussion of the processes driving NH₃ volatilization, and the breadth of the cited literature, all relevant and appropriately-used, is impressive. I have thus no hesitation to recommend publication. The following comments are suggestions for further minor improvements.

Specific Comments

Sections 2.1.2 and 2.1.3: What is the accuracy of the FTIR, and is it path-length-dependent? What is the accuracy of the background instrument (Picarro)? Together, these determine the accuracy of upwind-downwind concentration differences fed into WindTrax.

p.2642 L5: Table 2 is mentioned before Table 1, they should be swapped.

p.2642 L19: "FAO, ISRIC and ISSS" is not in reference list.

Section 2.2: Were background readings compared between the various NH₃ instruments? This is important because the background is measured with a different instrument type.

Section 2.3.1, 2nd paragraph: The comments on flux divergence and footprint are all correct. This makes for a very complex data analysis. I wonder what accuracy should be expected from that?

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Eq. (1): May I suggest to replace “FP” by a single-letter symbol (not “F”), for a clear distinction between footprint terms and flux terms.

Eq. (2): Please explain the rationale for the bi-exponential approach (why two decay processes?). It is done later (p. 2659), but one sentence here would help. Was a simple exponential fit found to produce poor results?

p.2649 L20: What is c at 20 m a.g.l. needed for, since upwind concentration was measured separately?

Section 3.1: The crop area treated on 4 Aug is upwind of the grass area treated on 6 Aug. Fig. 1 suggests that the background instrument was upwind of both areas, on both days. How can you be certain that the crop area did not contribute to the NH₃ fluxes measured on 6 Aug? Sure, you observed a decline in the evening of 4 Aug, but NH₃ emissions will increase again the next day(s) as temperature rises.

Section 3.1, last sentence: Can you explain how the F_{ini} values relate to Fig. 4. They are about twice the peak values shown there. Are they just hypothetical fitting parameters? What happens before the peak is reached?

p.2651 L20-21: “6%, 6% and 22 %”, etc: percentages of what?

Section 3.4.1, “The bLS model was used... matched within 10%” This is an excellent way to use WindTrax, and the result is encouraging. Just note that, hypothetically, if an EC bias and a WindTrax bias were present and canceled each other, that would go undetected!

Section 3.4.1, end of first para and Table 5: It is a bit misleading to take a regression slope directly as a measure of relative error when there is a significant intercept present (unless the intercept represents a known source of bias, in which case it should be corrected for first). Linear regression also gives the largest weight to the largest values. A better test would be to take the ratio of the two methods for each run and then give the mean and SD of this ratio (and its trend over time, if one exists).

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Section 3.4.1, last 3 sentences and Fig. 10: This is an interesting test, but deviations from the 1:1 line could be caused by any of the following: bias of the EC flux correction, bias of WindTrax, or bias of the footprint model used to relate EC flux to surface emission rate. So, to really arrive at the desired conclusion about EC flux bias, one would need to show independently that WindTrax and the footprint model were consistent with each other.

p.2656 L10: “to more than 50%”: why not give the actual maximum error? “More than” can be anything.

p.2658 L7, “fetch... quite small that day”: Fig. 1 gives the impression that on both days the FTIR paths were along the downwind edge of the slurry strips, and that the strips all had similar lengths, so why would the fetch on 6 Aug have been shorter?

Section 5, 2nd para: Does the result of the methods comparison lead to a recommendation? Is EC to be favored because it is more accurate? Or is either method OK, provided the accuracy of the instrumentation is good enough? Is it possible to give guidance what accuracy would be required, for either method?

Section 5, end of 2nd para, “very small contributions on the subsequent day”: Better replace “very small” with “less than x%”. Do the authors have any proof how small the emissions were then, or is this just an assumption? If the latter, then this should be removed from the Conclusions.

Section 5, para beginning with “Generally”: Not only must the instrumentation be fast and accurate enough, but it seems that one also requires a rather complex analysis of footprint geometry and decay evolution, which makes it next to impossible to develop a “routine” procedure. In other words, one needs 1) expensive instruments, 2) a lot of expertise, and 3) many scientist-hours, to obtain the emission rates for a single slurry spreading. Do the authors see a way forward to reduce any of the requirements 1) to 3)?

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Table 3: Are the huge numbers in Rows 2-4 of the tau_2 column correct? If so, what do they mean?

Fig. 5: Do the short-term fluctuations of the fitted curves (red lines) represent changes in footprint contributions due to wind direction fluctuations? Please explain in the text.

Technical Comments p.2639 L16: remove "the" before "both". p.2654 L2: dito.

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