

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-907

Anonymous Referee #2

Referee comment on "Assessing the representativity of NH₃ measurements influenced by boundary-layer dynamics and the turbulent dispersion of a nearby emission source" by Ruben B. Schulte et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-907-RC2>, 2022

This a well written manuscript with a clear scope.

My main comment regards the basic assumptions in the manuscript. There are two fundamental assumptions in the manuscript: 1) the NH₃ concentration in the plume is treated as a decaying scalar decoupled from the background and characterized with a "typical" percentage conversion rate, 2) the same conversion rate is applied for the plume and the background. However, the near source volume interested by the plume dispersion may have a rather different (non-linear) reaction environment and the conversion rate may be locally different. Given the simulation context, the approximations above are needed, and a full check of their validity would perhaps require the simulation of NH₃ as a reactive scalar with an appropriate reaction mechanism. Nonetheless, given the experience of the authors in the subject of atmospheric turbulence chemistry interaction, it would be nice to add in the manuscript some more discussion about these assumptions.

Other comments

- 1) The scenarios simulated by the authors are analyzed separately. I think it would be nice to have some representation (or at least discussion) of the variability implied by the different scenarios if they are combined, e.g., large emission rate, geostrophic wind and low background simultaneously.
- 2) Line 227. I think it is misleading to refer to turbulent fluctuations as noise in an observation. I suggest removing the sentence.
- 3) Line 235. The authors calculate fI only for values of the mean concertation

above a fixed threshold. This is ok but it would be useful to write down how this threshold is significant compared to the local maximum plume mean concentration at the various downwind positions.

4) Line 379. Although I think that I understand how the authors give the estimate 6-15km, it would be useful a more detailed explanation for the less acquainted readers.

5) Line 420-423 are a repetition of the lines 415-418, please remove it.