

Interactive comment on “Are ammonia emissions from field-applied slurry substantially over-estimated in European emission inventories?” by J. Sintermann et al.

Anonymous Referee #2

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General comments

The paper is a very useful review of NH₃ emissions from cattle and pig slurry reported in the past 20 years across Europe. The data collected by means of experiments and observations represent a wide variety of environments, management practices, and different measuring techniques through the years. The high variability of emission factors emerging from this work is certainly challenging the interpretation of the results. The objective of the paper is to assess whether the reported emission factors that set the current inventory methodologies for agricultural NH₃ emissions are actually fitted to this purpose nowadays. I think the paper fulfils the objective, it's well written and

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certainly is very relevant for the discussion on emission inventories within the scientific community.

The authors well address the issues that underpin the high variability of the emission factors through the years and across different environmental conditions as well as measurement techniques, and state as a consequence the need for harmonisation in measuring and modelling protocols. The introductory part of the paper and the presentation of the materials and methods are satisfactory and very well presented. The different measuring and modelling approaches are discussed in an objective way, highlighting the pros and cons of each technique/model as well as discussing the uncertainties related to each technique. The data are analysed according to type of slurry, year, and size of experimental plot/field, and I find this very clear and sensible. The tables and figures are clear and of easy vision especially online (fig.2 is a bit challenging in printed form, but it is an online journal, so I don't think it is necessary to change it).

However, I feel that not enough importance has been given to the conclusive discussion part of the paper. Especially regarding the work in section 3.5: I find the novel “check method” for initial flux estimates very interesting, especially as it is applicable to all data sets (provided the parameters required). I think the discussion on the implication of the results needs to be expanded, providing more interpretation of the outcome, stating more explicitly what degree of difference in EFs is noted for the different datasets. I am aware of the results in table 2, but I think they should be further discussed in the text, maybe in section 3.5, or in the conclusions. In general, I think the reader would benefit from having some more “guided” conclusions, such as reporting for instance the amount of uncertainty linked to all aspects considered and reported in the first 4 sub-sections of section 3; expand and strengthen the general conclusions regarding the needs for improving inventories methodologies, for instance- but not necessarily- by making an example on how different an EF from the same treatment with different method can get. Even if it is a repetition, it can be useful to summarise it again at the end. Same for the need for harmonisation of methodologies and protocols: it has been

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mentioned in the paper, but I think it would help to have it together in the concluding remarks.

Specific comments

P10074, Line 7. Replace “. . .controlled, which” with “. . .controlled; this”

P10079, eq.6 By comparison with Menzi's paper, correct the formula: -9.15 with -9.51.

P10081 , L17. . . . if EFs “from” instead of “of”

P10084, L2. A bit of a pedantic comment, but primed letters (u', c') should be explained in the text

P10086,L19. Specify what z0 and F are in the text

P10087, l2. Why “were originally based”? are they no longer?

P10087, l13. Substitute idealised with ideal.

P10087, l16. The parameters specified should be spelled out, i.e. u* friction velocity, etc.

P10087, l17 parameterS

P10088,L11 Add a comma after “frequency”

P10089,L21. Missing parenthesis after (2001a)

P10090, L4. I don't understand the reference to Fig.1, the chart is not specific to Menzi's results; I think it adds confusion.

P10090, L23- 26. The “cold conditions” and the “warm summer days” are clear qualitative description, but I think it would be better to mention example temperatures relative to these conditions.

P10090, L26. The expression “modification factor” is not too clear: do you mean the modified emission factors?

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P10091, L2. Replace “an” with “a”; I think the vowel rule does not apply in this case, as the beginning of “European” has the same sound of “you”, and it may not count as a vowel.

P10091, L6. It is slightly unclear. Perhaps say “ ...indicates that field application of manure accounts for 31% of the total NH3 emissions”

P10091, L8. Move the parenthesis as . . .Dohler et al. (2002).

P10091, L10-12. Modify the sentence as: “. . . reduction of EF for NH3 losses from field application of slurry in the reference case would reduce the overall emissions by 10 to 15%.”

P10092, L3. Perhaps replace “delicate” with “sensitive”?

P10092, L7-9. In this sentence it should be repeated that there is a factor 2 bias between IHF and the rest.

P10092, L10. “. . .nor is it too clear”

P10092, L15. “. . .identical”

P10092, L16 “meteorological”

P10092, L14-17. Modify the sentence: A new series of measurements is urgently needed, in order to systematically compare emissions from medium and large plots under identical conditions, in terms of slurry composition and application technique, soil properties and meteorology using a range of different techniques.

P10092, L17. Replace “presented” with “present”.

Table2. Perhaps in the caption you could explicitly state what Fini, est 25%, 50%,75% are.

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