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Atmospheric Measurement Techniques Discussions
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Interactive comment on "A miniDOAS instrument optimised for ammonia field-measurements" *by* J. Sintermann et al.

Anonymous Referee #1

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General comments: The manuscript of Jörg Sintermann with the title "A miniDOAS instrument optimised for ammonia field-measurements" describes the further development of an instrument presented by Volten et al. (2012). Details of the technical upgrades (e.g. UV lamp, temperature regulation) and performance improvements are given. In addition, first field applications and inter-comparison measurements between instruments and with alternative techniques are presented. The research area of diffuse NH3 emissions especially from fertilized agricultural soils is very active and of interest for a number of readers and potential future users of the DOAS technique. Besides, exctractive sampling techniques have the tendency to suffer from adsorptive loss of NH3, which can be optimised but is a persisting issue as also detailed by the authors. I have a number of suggestions for technical corrections the authors might consider to improve the consistency and readability of the manuscript. As an example,

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the nomenclature of the BIPM for the expression of uncertainty should be followed. In summary, I suggest publication in Atmospheric Measurement Techniques with minor revisions.

Specific comments: Page 1 Line 24: The instrument precision is given in "%". It should be indicated , however, that this is relative to the determined concentrations.

Page 1 Line 24: The statement "Accuracy is larger than precision" is trivial and should be deleted.

Page 1 Line 24-26: It would be preferable for the reader to have a "combined uncertainty" including the different systematic and random sources of error, to estimate the potential of the presented technique. The most important error sources might be given in addition.

Page 1 Line 26: The acronym "I0" in defined as the light intensity emitted by the light source (page 3 Line 23), which is common definition (i.e. Beer Lambert law). Here the term "reference spectrum" is used for I0, please correct. In addition the term I0 is used throughout the manuscript for I with "baseline concentrations", this leads to severe misunderstanding and should be corrected – it might be best to introduce an additional acronym.

Page 1 Line 27: The term "minimum accuracy" might lead to misinterpretation, please replace.

Page 1 Line 27: The term "the limit of detection against I0" is not correct and might not be used, please rephrase.

Page 2 Line 32: The manuscript switches between "μg m-3" and "ppb", please unify here and elsewhere in the text.

Page 2 Line 49: The term "on its breadboard" might be deleted.

Page 2 Line 60: ". . . DOAS data evaluation procedure"?

Page 2 Line 63: What the meaning of “finite” in this respect?

Page 4 Line 103: In the experiment W3 the authors claim they used three miniDOAS downwind, here it is stated that two instruments were used, what is correct? Is the numbering of the instruments (S2 and S5) relevant or just for internal use, otherwise it is confusing the readers only, why not replacing it by N1, N2, . . . for the new instruments and O1 for the old one.

Page 4 Line 123: Please rephrase the term “The purpose is the elimination of P . . .” to something like “This is accomplished by high-pass filtering of Idiff . . .”

Page 4 Line 125: Cross-sections were fitted to the measurement spectra I assume?

Page 4 Line 126: For which parameters was the gas cell controlled? Please specify.

Page 5 Line 130: The phrase “across that range” is unclear and might be corrected.

Page 5 Line 138: The term “alpha sampler” might not be familiar to every reader, please change to “alpha passive ammonia samplers” or similar. You might state here that you used active and passive sampling devices for inter-comparison.

Page 6 Line 175ff: The abbreviations for the field experiments are seem arbitrary. It might be better to number the experiments consecutively.

Page 6 Line 188ff: Could you please give more details on the impinger measurements, e.g. flow rates, acid etc. or at least cite corresponding literature?

Page 7 Line 202: The sentence “and in parallel alpha passive samplers” is incomplete – no verb.

Page 7 Line 215: The term “IHF” should be defined when used first, but is defined later.

Page 7 Line 218ff: Which experiments are described here? I assume W3. . .

Page 8 Line 218ff: As details on the NH3 emissions are available and given in the text this would be the ideal experiment to compare to NH3 emission estimates determined

by the DOAS technique – Why the comparison is limited to concentrations and not emissions?

Page 8 Line 254: The term “slightly” is qualitative, what does this mean in numbers?

Page 10 Line 290-292: Spectral interferences are given in “%”, which is arbitrary, it would be better to give it in “μg m-3 / μg m-3” or similar.

Page 10 Line 302: The term “the scatter between both instruments” should be rephrased.

Page 10 Line 307: It’s hard to understand how the standard errors was estimated; I assume it’s based on the AMRA or OLS fit; but should be mentioned here also.

Page 10 Line 312: It is not a “concentration difference between S2 and S5” but a “difference in concentration determined with S2 and S5”.

Page 11 Line 324: On line 317 it was stated that the precision is 0.8 to 1.4 %, here a precision of 1.4 % is given, Which value is correct?

Page 11 Line 324: The term “the LOD μg m-3” seems awkward.

Page 11 Line 329: What is an “alpha sampler batch” please clarify.

Page 11 Line 339: The sentence “Since the signal-to-noise ratio with the baseline and I0 is inevitably larger than with I and I0, . . .” is hard to follow, please rephrase.

Page 11 Line 341: The term “the absorption present in I0” is confusing as I0 is the intensity of light emitted by the light source so without absorption.

Page 12 Line 362: “. . . which allows an independent concentration determination”?

Page 13 Line 414: The correct term is “Swiss national air pollution monitoring network”.

Page 14 Line 423: The term “NH3-SO2-NO cross-interference” is not correct as no NO variation was observed and only NH3 and SO2 varied.

Page 14 Line 443: The authors mention the possibility to use a trace dilution technique with SO2 or NO dosing. What is the LOD and the toxicity (e.g. MAK) for this components? I assume this approach might be too dangerous, please give a statement.

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