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Atmospheric Measurement Techniques Discussions

Interactive comment on "Areal-averaged trace gas emission rates from long-range open-path measurements in stable boundary layer conditions" *by* K. Schäfer et al.

Anonymous Referee #1

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General The MS first considers various techniques available for measuring trace gas fluxes over large spatial scales of order of a ha or more and notes that micrometeorological methods are required, but all of them have problems during stable atmospheric conditions due to low levels of turbulence. The authors also conclude that non-intrusive methods are preferable for trace gases emitted from soils because they avoid the influences of the environment on the emission process. They appear to favour long-range open-path concentration measurements, but note that ideally, path-integrated wind and turbulence measurements made in the same control volume as the concentration measurements are required. The authors conclude that unfortunately, present instrumentation cannot provide the necessary accuracies during stable and very stable conditions

and move on to consider using open-path gas measurements in a flux-gradient approach. They observe, however, that successful application of this method requires confidence in the gradients in gas concentration and confidence in the applicability of boundary-layer turbulence theory. Noting that under stable, light wind conditions, the continuous 'steady-state' turbulence of the surface boundary layer breaks down and the layer can have phenomena like intermittent and wave-like turbulent structures, they declare that they will deal only with what they call quality-assured determinations of fluxes under low wind, stable or night-time atmospheric conditions. One assumes that quality-assured data have been filtered for outlier values and values of parameters such as u and ζ that fall within certain ranges. Provision of a table that shows what data have been filtered and what the acceptable ranges for the various parameters are would be a welcome addition. Finally, the authors present various flux-gradient methods and examine how well 3 of them succeeded in estimating fluxes in 2 experimental situations: one in a study of N2O fluxes from grassland and the other in an examination of NH3 fluxes from cattle lagoons. One of the methods used a similarity approach that does not assume log-linear relationships between wind speeds or gas concentrations and height while the other 2 used MOST flux-gradient approaches that assume log-linear profiles of wind speed and concentration. The flux calculations were tested by comparing with chamber data in the grassland experiment and integrated horizontal flux calculations in the lagoon experiment. It was concluded that the first of the approaches, the similarity approach, provided the best estimate of gas fluxes in stable conditions while the other methods were of limited value because the wind speed and concentration profiles were not log-linear as assumed by MOST. While I think the topic of the paper of the paper is important and fits well with the journal's scope, I find the MS too long and too discursive in its present form. I feel its length could be reduced by a considerable amount, maybe 40 or 50% with the heaviest reductions in Section 1, the Introduction, and 3, Methods. Those Sections contain interesting information, but much of it is unnecessary. Examples include Table 1 which is a useful reference source in other contexts, but this paper is about open-path flux-gradient techniques, not

AMTD

5, C255–C259, 2012

closed-path techniques and it doesn't probe the strengths and weaknesses of chambers. A lot of Section 3, particularly Section 3.2 on open-path flux-gradient methods is conventional micrometeorology and could be abbreviated. The telling of the story seems to dart off in different directions unexpectedly in these 2 Sections and I suggest that their structures be tightened to more concise and logical forms I am concerned that there is no really convincing demonstration either in the paper or in the references that flux-gradient relationships developed for measurements in the vertical plane apply over long horizontal paths of around 100 m. The authors consider the problem, but seem convinced that their quality assurance measures will overcome it. To me, something more convincing seems necessary, although I must admit that I can't say what it is; perhaps more chambers or anemometers or scintillation measurements or better still some demonstration either through measurement or modelling that time integrated means will iron out local perturbations. I also feel that a weakness in the paper is the lack of an unequivocal measurement of source strengths. Reliance on just 4 chambers (which gave quite different fluxes) and IHF methods to test the flux gradient methods seems rather risky. A good deal of editing for colloquial English is necessary. I have made a number of suggestions in my Specific comments, but other changes still need to be made. Specific p.4, line 14: suggest "other" for "further" p.4, line 15: delete "the" between "of" and "source" p.4, line16: suggest move " and sink" to to previous line after source; delete the end bracket after"2006" and the forward bracket before "Chapuis-Lardy" p.5, line 6 : delete "the" p.5, line 11: I don't know of an instance where an open-path TDLAS instrument has been used for eddy covariance. Perhaps give a reference. p.5, line 18: suggest insert "the" between "called" and "integrated" p.5, line 19: "Lagrangian" for "Lagrange" p.5, last line: perhaps "evaluating" for "approximating' p.6, line 3: suggest "low turbulence levels" for "insufficient turbulent transport" p.6, line 12: suggest delete the first "and"; insert "recently" between "and" and "for" p.6, line 13: suggest move "of QCL" to between "versions" and "are" p.6, lines 13-16: closed–path FTIR deserves a mention here p.6, line 17: suggest move "path-integrated wind and turbulence measurements" to beginning of line; add "are" after it p.7, lines 3 & 4: The

AMTD

5, C255–C259, 2012

statement that non-intrusive flux measurements are available for CO2 and H2O only is not true. The open-path Licor 7700 instrument measures CH4 at a point in just the way the authors describe: non-obtrusively & without any enclosure or sampling or measuring cell and is intended for eddy covariance measurements, while open-path lasers are available for all 3 of N2O, CH4 and NH3. p.7, line 2 from bottom: See comment about "quality-assured" in my General comments p.8, line 7: suggest reword the sentence to read "The footprint function describes the relationship....." p.8, lines 10 & 11: suggest reword the sentence to read "It is the objective of the study....." p.8, line 13: suggest "of " for "to" and "defining" for "defined" p.8, line 15: suggest "However," for "But also,"; "analysis" for "analyses"; "of" for "to": "determining" for "determine" p.8, line 17: suggest delete "the" p.8, line 18:suggest "analysis" for "analyses" p.8 line 19: suggest delete the second "the" p.9, lines 4 & 5: suggest "changes" for "change" and "take" for "takes" p.9, line 6: suggest delete the 2nd "the" p.9, line 8: suggest insert "to occur" after "fluxes" p.9, line 16: suggest "with" for "to" p.9, line 20: suggest "in the present context" for "here" p.10, line 8: suggest delete "the" p.10, line 16: "at" for "in" p.11,line 2: "upon" for "from" p.12, line 3: clarify if 1-, 2- or 3-D p.13, line 17: zF is often set at the geometric mean of z- and z+, e.g., Paulson (1970) p.13, line 21: "on" for "from" p.13, line 22: insert "the" after "as" p.14, discussion of ψm and ψc after Eq.(2): It is usual to equate ψm and ψc in neutral conditions. Is that what is intended here? p.17,line13: insert "at" after "set" p.22, line 11: delete "is applied which" ? p.22, line 5: suggest "used" for "was using" p.23, lines 5 to 7: The sentence here needs rewording for clarity p.23, lines 10, 12, 14: "at" for "in" p.23, line 22: insert "is" after "and" p.26 & 27: I wonder if the discussion in the first 2 paragraphs of this new section is necessary p.27, last paragraph: Perhaps justification for the use of IHF to represent the real flux is needed. Reasons could include simplicity of concept, few theoretical assumptions, etc. p.28, lines1 & 2: The meaning is unclear to me. Suggest reword to clarify. p.30, lines 2 and 14: "at" for "in" P. 30, line 7: insert "in" after "gaps" p.31, lines 1,5, 6: "at" for "in" p.31, line 1: suggest delete "was" p.31, line 2: insert "was" after "height" p.31, lines 12-16: This sentence needs rewording to clarify what is being said p.31, line 16: delete "up"

AMTD

5, C255–C259, 2012

p.32, line 9: insert “the” before Indiana p.32, lines 11 to 13: Higher emissions during near-neutral conditions might also be due to effects on soil and lagoon temperatures, which will change equilibrium gas concentrations p.33, line 7: the range $0 < \zeta \leq +10$ would exclude all unstable cases because ζ would be < 0. p.34, line 3: “periods” for “period” p.34, line 10: Where is Sect 5.1? Should it be Sect. 3.2? p.35, line 8: “layers’ for “layer” p.35, line 11: suggest move “calculated” to before “N2O” p.35, lines 10-17: There is too much crammed into this sentence. Suggest split it and rewrite. p.35, last 3 lines: meaning unclear p.38, 1st reference: Year of publication missing p.38, 2nd reference: 2 years of publication. Maybe one of the years belongs to the 1st reference p.48, Table 1: NH3 is a sticky and very soluble gas and chambers are not recommended for measuring NH3 fluxes. p.57, Figure 8: What does the curved line represent? p.58, Figure 9: What is the distinction between diamonds and squares? p. 59, Figure 10: Same comment as Fig. 9 p.60, Figure 11: What does the curved line represent?

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1459, 2012.